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GASES IN METALS[1]

BY LOUIS JORDAN[2]

INTRODUCTION

It is a natural assumption that two specimens of a metal or alloy of identical chemical composition, which have been prepared in the same way and have received the same mechanical and thermal treatments, should possess the same properties and react in the same manner under like treatments. Such a state of affairs is, in some instances, apparently not true. Again, metals or alloys of apparently identical chemical composition prepared, however, by different smelting, melting or refining processes may differ very appreciably in certain properties or in quality of service. In both of these cases judgment as to similarity of chemical composition is almost always based on results of analyses which are incomplete in that the kinds and amounts of "gases" present in the metal are not considered. The term "gases" used in this manner includes those substances found in metals in either combined or uncombined condition which, in their ordinary free state, exist as gases. The term thus includes oxides, nitrides, and hydrides in metals as well as uncombined oxygen, nitrogen, hydrogen, or other gases.

The existence of appreciable amounts of gases in practically all metals and alloys has long been recognized. A relation between differences in properties of otherwise similar material and differences in the gas content has frequently been postulated but definite proofs have not often been available.

METHODS FOR DETERMINING GASES IN METALS

Nearly sixty years ago Thomas Graham[3] called attention to the volume of gases, three times the volume of the sample, evolved from a meteoric iron heated in an evacuated porcelain tube. From that time to the present day the occurrence and the significance of gases in metals have been the subjects of investigation by many workers. A summary of investigations relating to gases in ferrous alloys from the time of Graham's work up to 1918 has been published by Alleman and Darlington.[4] This paper is so readily available for reference that it is not profitable again to review at any length these earlier investigations.

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[1] Published by permission of the Director of the U. S. Bureau of Standards.

[2] Chemist, U. S. Bureau of Standards, Washington, D. C.

[3] T. Graham, *Proceedings*, Royal Society, Vol. 15, p. 502, 1866–67.

[4] G. Alleman and C. J. Darlington, *Journal*, Franklin Inst., Vol. 185, pp. 161–98, 333–57, 461–80 (1918).

Heating or Fusion in Vacuo.—These early experiments were for the most part concerned with the development of methods for determining the kinds and quantities of gases evolved from metals heated in vacuo. Little attention was paid to the correlation of results of these analyses with the history of the production of the metals or with their quality. Considered in a very general way, they showed that the gases obtained from ferrous metals consist of hydrogen, nitrogen, carbon monoxide, and carbon dioxide. The total volume of gas obtained from a metal varied greatly, some experiments yielding 70 volumes of gas from 1 volume of metal. A few attempts were made to determine gases evolved on drilling a metal under some liquid; water, oil, or mercury.[1, 2] These methods were finally judged to yield only a part of the true gas content of the material.

Austin[3] concluded that the extraction of gases from metals could not have been complete in any of the early investigations in that the metal samples were not fused. The method he proposed consisted in heating in vacuo a metal bar to its melting point by its own electrical resistance. This method obviously permitted only an instantaneous attainment of the melting temperature in a small portion of the sample and still left doubt as to the completeness of the extraction of gases.

More recently Ryder[4] has employed a method, similar to that of Austin, in which a filament of the metal sample is mounted in a glass bulb and heated by its own electrical resistance. This method has permitted very accurate work at temperatures up to 1000° C. The sample of metal is necessarily rather small and special volumetric methods for the analysis of the small amounts of gas evolved are necessary.

A method permitting the complete fusion of the metal sample and maintaining it in a liquid state for some time while gases could be pumped out was described by Goerens and Paquet[5] in 1919. Gas-free antimony and tin were added to the iron or steel sample and the mixture heated to 1150° C. in a magnesia crucible within an evacuated silica tube. At this temperature, the resulting alloy of iron, antimony, and tin is liquid and the fused silica tube will hold a vacuum. This

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[1] F. C. G. Muller, *Bericht*, Chem. Ges., Vol. 12, p. 93 (1879); *ibid.*, Vol. 14, p. 6 (1881).

[2] J. E. Stead, *Journal*, Iron and Steel Inst., Part II, pp. 526 and 572 (1882); *ibid.*, Part I, p. 114 (1883).

[3] G. W. Austin, *Journal*, Iron and Steel Inst., Vol. 86, Part II, p. 236 (1912).

[4] H. M. Ryder, *Transactions*, Am. Electrochemical Soc., Vol. 33, pp. 197–204 (1918).

[5] Goerens and Paquet, *Ferrum*, Vol. 12, pp. 57–64, 73–81 (1919).

method has quite recently been employed by Oberhoffer[1] and his co-workers in Germany to studies of the deoxidation of various steels. It is also reported to have had some application in other European laboratories.

Alleman and Darlington[2] developed a molybdenum resistance furnace for the fusion of iron and steel in vacuo for the extraction of gases. They melted steels in contact with aluminum oxide (RR alundum) and collected exceedingly large volumes of gas, in some cases 200 times the volume of the metal. Great difficulty was experienced in securing and maintaining refractory tubes which would not be porous at the temperature employed (1650° C.). Doubt may also be entertained as to the absence of reaction between the carbon in the steel and the refractory oxide sample containers.

Still a third method involving the complete fusion of the sample in a vacuum remains, namely, the melting of the metal in contact with carbon. This method was proposed by Walker and Patrick[3] several years prior to the proposal of the antimony-tin method by Goerens. It was originally suggested as a substitute for the Ledebur[4, 5] method for the determination of oxygen. Recent work at the U. S. Bureau of Standards has shown that this method can be adapted to the accurate determination in metals of oxygen and hydrogen, and possibly also of nitrogen.

The three vacuum fusion methods, namely, fusing in refractory oxide crucibles (Alleman and Darlington), fusing with antimony-tin alloy (Goerens and Paquet), and fusing in contact with carbon (Walker and Patrick), have all been employed and compared in the Bureau of Standards laboratories. The adaptation of the Ajax-Northrup high frequency induction furnace to the extraction of gases from metals by any of these three methods, the development of a train for the absorption of all gases as rapidly as they are evolved from the molten sample, and the gravimetric determination of these gases, will all be described in detail in a Bureau of Standards Scientific Paper to be issued at a later date.

Each of these three fusion methods has been applied to both carbon-bearing and carbon-free metals. The fusion of a low (0.25-per-cent) carbon steel in refractory oxide crucibles showed that in a vacuum at temperatures somewhat above the melting point of the steel, reduction of the refractory oxide was brought about by the carbon of the steel when crucibles of magnesia, alundum, alumina-silica, or zirconium silicate were used. Such a reaction obviously vitiates any values for evolved CO or CO_2. Zirconia-alumina crucibles were found to permit the fusion of the low-carbon steel without decarburization of the metal, and oxygen evolved in such tests is judged to be derived from the metal sample. However, in the case of higher (0.7-per-cent) carbon steels, even the zirconia-alumina crucibles were reduced.

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[1] P. Oberhoffer and A. Beutell, *Stahl und Eisen*, Vol. 39, pp. 1584–90 (1919).

[2] *Loc. cit.*

[3] W. M. Walker and W. A. Patrick, Eighth International Congress Applied Chem., Vol. 21, pp. 139–48 (1912).

[4] A. Ledebur, Leitfaden für Eisenhütten Lab., Ninth Edition, p. 154.

[5] J. R. Cain and E. Pettijohn, U. S. Bureau of Standards *Technologic Paper No. 118* (1919).

The same steels were fused with antimony-tin alloy in magnesia crucibles. Magnesia crucibles were used by Goerens[1] and Oberhoffer.[2] In spite of the failure of such crucibles in the direct fusion of steels, as already noted, it was thought that they might serve satisfactorily in this method from the fact that carbon is not soluble in the antimony-tin-iron alloy but is rejected to the surface of the melt as free carbon. Free carbon (graphite) does not reduce magnesia at 1150° C. in vacuo. The results for oxygen and hydrogen obtained on the low-carbon steel were practically the same as obtained by direct fusion of the steel in zirconia-alumina crucibles. With the higher carbon (0.7-per-cent) steel, however, very high and erratic results for oxygen and hydrogen were obtained. In view of the very much lower results for oxygen obtained later on the same steel by fusion in contact with graphite, it seemed probable that a large portion of the oxygen obtained by fusing the higher carbon steel with antimony-tin was derived from reaction with the oxide of the crucible.

The same two steels were analyzed for oxygen and hydrogen by the third method, namely, fusion in graphite crucibles. With the low-carbon steel, the same values for hydrogen were obtained as in the two previous methods, but the results for oxygen were nearly double those by either of the other methods. Fusion in contact with carbon is, therefore, necessary for complete extraction of oxygen in low-carbon steels. On the other hand, less oxygen was found in the 0.7-per-cent carbon steel by fusion in graphite than by either of the other methods. In this case it is certain that direct fusion of the steel in a refractory oxide crucible gave oxygen derived from the crucible material and it is probable that a similar reaction with the crucible took place even in the fusion with antimony and tin. In the case of either steel it is believed that the fusion in contact with carbon gave the more correct results for the oxygen content of the metal.

The same three methods of fusion were applied to a "carbon-free" metal—electrolytic iron. Again the results from direct fusion and fusion with antimony-tin were practically identical, but fusion in graphite gave much higher recovery of oxygen. It has also been found that, with oxidized electrolytic iron, the Ledebur method (reduction in hydrogen for one hour at 950° C.) did not give as much oxygen as the fusion of the same material in carbon—0.9 per cent of oxygen by hydrogen reduction at 950° C. for 1 hour and 1.7 per cent of oxygen by fusion in graphite.

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[1] *Loc. cit.*

[2] *Loc. cit.*

The results of the tests of these three methods of vacuum fusion as applied to low-carbon steel, high-carbon steel, and electrolytic iron are summarized in Table I. It is believed that the method of fusion in contact with graphite is the most generally applicable of the three methods for the correct determination of total oxygen and hydrogen in both carbon-free and carbon-bearing metals.

Table I.—Comparison of Three Vacuum Fusion Methods for Determining Oxygen and Hydrogen in Metals

Type of Metal Sample	Method of Fusion[1]	Oxygen, per cent	Hydrogen, per cent
0.25-per-cent Carbon Steel	*A*	0.0093	0.0015
	B	0.0105	0.0010
	C	0.0205	0.0015
0.72-per-cent Carbon Steel	*A*	0.032	0.004
	B	0.044	0.003
	C	0.0027	0.0003
Electrolytic Iron	*A*	0.0092	0.0013
	B	0.0088	0.0017
	C	0.0163	0.0011

[1] *A* = direct fusion in zirconia-alumina.
B = fusion with antimony-tin in magnesia.
C = fusion in graphite.

Other Methods for Determination of Gases.—The Ledebur method for oxygen in ferrous materials has been critically tested by Cain and Pettijohn at the Bureau of Standards and shown to be of limited application[1, 2] and not to indicate differences in oxygen content of steels having different deoxidation treatments.[3]

The Goutal method for determining CO and CO_2 in steel by solution in copper-potassium chloride solution has also been examined by Cain[4] and shown to yield CO and CO_2 generated by the action of the cupric salt on the carbides present. A somewhat similar solution method has recently been described by Oberhoffer and Piwowarsky.[5] This latter method depends on the solution of the metal sample in mercuric chloride solution or in bromine water and the determination of liberated gases. The method is claimed to determine only gases existing as such in the metal and not those formed by reaction between oxides and carbon as in vacuum fusion methods.

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[1] J. R. Cain and E. Pettijohn, U. S. Bureau of Standards *Technologic Paper No. 118* (1919).

[2] J. R. Cain and L. Adler, U. S. Bureau of Standards *Scientific Paper No. 350* (1919).

[3] J. R. Cain and E. Pettijohn, U. S. Bureau of Standards *Scientific Paper No. 346* (1919).

[4] J. R. Cain and E. Pettijohn, *U. S. Bureau of Standards Technologic Paper No. 126* (1919).

[5] P. Oberhoffer and E. Piwowarsky, *Stahl und Eisen*, Vol. 42, p. 801 (1922).

Amalgation with mercury for the extraction of gases has been applied to certain metals, for example, high-magnesium alloys.[1]

Methods for Nitrogen.—The vacuum fusion methods have often determined nitrogen by difference after the volumetric estimation of all other gases. The Allen method is the only direct method for nitrogen that has had any wide application. This is an acid solution method and determines only nitrogen present in metals as certain nitrides, and does not determine uncombined nitrogen which has been shown to exist in certain ferrous materials.[2]

An investigation is now in progress in connection with the work on vacuum fusion methods at the Bureau of Standards, which aims at the determination of the nitrogen in the gases evolved from vacuum fused samples by the absorption of evolved gases in metallic calcium vapors. Nitrogen so absorbed is in the form of calcium nitride which very readily is converted to ammonia by solution in acid and then determined by the usual method of distillation from an alkaline solution.

IMPORTANCE OF GASES IN METALS

The question may be asked, "Why be concerned with the determination of the few hundredths or thousandths of a per cent by weight of gases in a metal?" The answer is that in some instances we are quite sure that the presence of comparatively small amounts of gas does influence certain properties of metals. Methods should be available for the quantitative determination of such gases. In instances where we are at a loss to explain differences in properties of materials of apparently the same composition and often lay the blame on the gases, quantitative analytical methods should be available for determining whether this blame is placed justly or unjustly.

It is believed that a brief consideration of a few theoretical aspects of the solubility of gases in metals, and of a few cases in practical metallurgy in which gases are generally considered to have an important bearing, will make evident the desirability of the development of accurate methods of analysis for gases and the application of such methods in metallurgical research.

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[1] H. L. Simons, *Chemical and Metallurgical Engineering*, Vol. 27, p. 248 (1922).

[2] L. Jordan and F. E. Swindells, U. S. Bureau of Standards *Scientific Paper No. 457* (1922).

THEORETICAL ASPECTS OF SOLUTION OF GASES IN METALS

Solubility and Temperature.—Determinations of the solubility of gases in metals have not been numerous. Most of the work in this line is to be credited to Sieverts.[1] Possibly the most interesting and surprising conclusion from his results is that in many cases the solubility of gases in both solid and liquid metals increases with increasing temperature, with a very sharp increase at the melting point of the metal. It is not easy to make this behavior of gases and liquid metals conform to our usual kinetic conception of the relation of temperature to the solubility of a gas in water. Wilsmore[2] has pointed out that water should not be considered the typical liquid solvent. The behavior of many metals on freezing apparently bears out the contention that the solubility of the gas decreases as the temperature is lowered. The "spitting" of silver and the "spewing" of over-poled copper on solidifying are quite generally named as examples of this decrease in solubility. Many other metals, including at least some of the platinum metals, steels and low-carbon irons, may show very vigorous evolution of gases during freezing.

In explanation of the increased solubility of gases at higher temperatures, there has been suggested the formation of oxides, nitrides, and hydrides which are more stable or more soluble, or both, as the temperature increases.[3] Porter[4] likewise calls attention to the fact that whatever the mechanism of the solution, the solubility of the gas in the metal must increase with temperature, if the solution is formed with the absorption of heat. Slade[5] claims that the "spitting" of silver is exactly what is to be expected from the temperature composition diagram of silver - silver oxide and the formation of a metal - metal oxide eutectic. As liquid silver containing a small amount of its oxide in solution is cooled from temperatures above the dissociation temperature of free Ag_2O, silver separates and the liquid phase becomes more and more concentrated in silver oxide until the partial pressure of oxygen from the dissociation equals the existing oxygen pressure in the gaseous phase of the system. The concentration of oxide in the liquid phase will always be such that these two pressures are equal until, at the eutectic temperature, all the remaining oxide separates and immediately dissociates, causing the sudden evolution of oxygen.

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[1] A. Sieverts, *Zeitschrift für Elektrochemie*, Vol. 16, pp. 707–13 (1910); *Bericht*, Chem. Ges., Vol. 43, p. 893 (1910).

[2] N. T. M. Wilsmore, *Transactions*, Faraday Soc., Vol. 14, p. 253 (1919).

[3] W. Rosenhain, *Transactions*, Faraday Soc., Vol. 14, pp. 224–7 (1919).

[4] A. W. Porter, *Transactions*, Faraday Soc., Vol. 14, p. 260 (1919).

[5] R. E. Slade, *Transactions*, Faraday Soc., Vol. 14, p. 248 (1919).

Mellor[1] discusses the three possible conditions in which oxygen dissolved in silver may exist, combined as oxide of the metal, dissolved as diatomic oxygen, or dissolved as monatomic oxygen. The experimental observation that the concentration of dissolved oxygen is proportional to the square root of the pressure of the oxygen gas eliminates the possibility of the dissolved oxygen existing in the diatomic form, but is the condition to be expected if the oxygen is in either of the other two forms.

Morey[2] states that the spitting of silver on freezing is a familiar example of the "second boiling point" of saturated solutions. If we plot the solubility-temperature and vapor pressure - temperature curves for saturated solutions of a volatile solvent and a non-volatile solute we find that, with sufficiently high pressures, there is no break in the solubility curve at the boiling point of the solution saturated at one atmosphere. Both solubility and vapor pressure increase past the boiling point with increasing temperature, but the vapor pressure curve soon reaches a maximum and then falls. Some point on this descending portion of the vapor pressure curve represents a pressure equivalent to atmospheric. This is the second boiling point and is, evidently, reached on *cooling* solutions of small amounts of the volatile component in the non-volatile component just as the usual normal boiling point is reached by *heating* solutions of small amounts of the non-volatile material in the larger quantity of the volatile substance.

Relation of Gases to Intercrystallin Material.—Andrew,[3] Rosenhain,[4] and Johns[5] have all laid considerable emphasis on the apparent relation of the solubility of gases in metals to the presence of the amorphous phase in metals. Their belief is that in the intercrystallin material is the location of dissolved gas. Many properties of metals are closely related to the nature of this intercrystallin material. Hence, gases if located in these intercrystallin films affect the properties of the metals seemingly out of proportion to the small amounts present. It is said that cold-worked palladium, which contains much more of the amorphous material than the annealed metal, absorbs more hydrogen than the same metal after annealing; also, that carbon monoxide penetrates iron along crystal borders at temperatures below those at which it can diffuse through the crystals, that is, below the lowest critical point. Both these observations point to solubility of gas in the intercrystallin material rather than in the crystallin metal.

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[1] J. W. Mellor, Inorganic and Theoretical Chemistry, Vol. III, pp. 342–4 (1923).

[2] G. W. Morey, *Journal*, Philadelphia Engineers' Club, Vol. 35, pp. 509–19 (1918).

[3] J. H. Andrew, *Transactions*, Faraday Soc., Vol. 14, pp. 232–9 (1919).

[4] *Loc. cit.*

[5] Cosmo Johns, *Transactions*, Faraday Soc., Vol. 14, pp. 261–2 (1919).

It has also been said that a metal which is inactive (crystallin) as regards gas absorption can be rendered more active (containing more amorphous material) by alternate absorption and extraction of small amounts of gas; that is, amorphous metal is generated by the alternate solution and extraction of gas. Andrew[1] has employed this hypothesis in an explanation of the embrittlement of iron by caustic solutions.

Johns[2] has suggested the following as a "working hypothesis" of the mechanism of the occlusion of gases by metals. "Occluded gases are dissolved chiefly, if not entirely, in the under-cooled liquid phase of the metal which forms the cement between the individual crystals. The gases are soluble in the liquid metal but not in the solid. Crystallization concentrates the dissolved gases in the intercrystallin films. These films may become supersaturated and gas evolved after solidification had proceeded so far that no actual blowholes could be found. There would, however, be discontinuities which would profoundly affect the physical properties of the metal. It is thus that the defects in steel known as 'flakes' are probably caused."

EFFECTS OF GASES IN PRACTICAL METALLURGY

Iron.—The ability of cast iron to contain iron oxide in the presence of the normal amounts of carbon, silicon, and manganese, and the nature of the effects of oxygen if present in such material, has recently been declared by Elliott[3] to be one of the moot questions of cast iron. The paper of J. E. Johnson, Jr.,[4] stating that the presence of oxygen in cast iron is beneficial, is possibly one of the best known contributions to the literature on the practical importance of gases in metals. Johnson's contentions were that oxygen in cast iron makes it more sensitive to chilling, yet when cooled slowly only slightly harder than normal iron but of finer grain; also that graphite in high-oxygen iron is finely sub-divided as contrasted with large aggregates of graphite in a similar iron which has a low oxygen content. Stork[5] has brought forward further experiments to prove the validity of Johnson's theory of oxygen in cast iron. There is still a considerable difference of opinion as to the correctness of the above views regarding oxygen in cast iron. In discussions of the subject it frequently has been pointed out that satisfactory methods for the determination of oxygen have been lacking.

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[1] J. H. Andrew, *Transactions*, Faraday Soc., Vol. 14, pp. 232–9 (1919).

[2] Cosmo Johns, *Transactions*, Faraday Soc., Vol. 14, pp. 261-2 (1919).

[3] G. K. Elliott, *Transactions*, Am. Electrochemical Soc. (Preprint) April (1922).

[4] J. E. Johnson, Jr., *Transactions*, Am. Inst. Mining and Metallurgical Engrs., Vol. 50, p. 344 (1914).

[5] W. L. Stork, *Transactions*, Am. Mining and Metallurgical Engrs., Vol. 62, p. 497 (1920).

The quality of pig iron cannot be finally judged by the ordinary chemical analysis.[1] Certain lots of pig iron will yield poor grade, brittle castings while other lots of pig iron of apparently identical chemical composition will give a satisfactory product. Similar differences, apparently due to the quality of different lots of pig iron, are encountered in the refining of pig iron to steel. Certain gray iron producers are at the present time desirous of purchasing pig iron under specifications as to its oxygen content, so firmly do they believe that this is the cause of certain troubles.

The graphitization of cast iron has been explained by Honda and Murakami[2] as being brought about by carbon monoxide in the iron. This gas is set free within the metal during freezing and then decomposed according to the equation,

$$2\,CO = CO_2 + C$$

The carbon dioxide formed by the first reaction is then thought to oxidize available iron carbide and regenerate carbon monoxide,

$$CO_2 + Fe_3C = 2\,CO + 3\,Fe.$$

This cycle is repeated with the constant formation of graphite. Honda and Murakami found that very little graphitization could be produced in a cast iron melted in a vacuum; that bubbling nitrogen through molten cast iron decreased graphitization, probably due to the sweeping out of carbon monoxide; but that bubbling carbon monoxide or air, or the addition of iron oxide, increased the graphitization on account of the tendency in each case to increase the amount of carbon monoxide in the cast iron.

Steel.—On the basis of method of manufacture, steel is classified as bessemer, open-hearth, electric, and crucible steel. It is generally considered that the quality of a steel is to a considerable extent dependent on the method of manufacture and that the order in which these steels have been named probably also is the order of increasing quality. These differences in quality are dependent on several factors. It is interesting and probably significant that the order of increasing quality is also the order of increasing accuracy in the control (elimination of the excess) of gases present during the melting or refining. It is well known that bessemer steel contains more nitrogen than any of the other classes of steel named above and by this characteristic is most easily distinguished from them.

Whether made by bessemer, open-hearth, or electric furnace practice, all steels must be deoxidized, that is, be treated with additions of scavengers. Deoxidation procedures have, until recently, been developed and applied with very little trustworthy information as to the actual changes taking place in the amounts and nature of the gases whose influences are to be controlled. Whether deoxidation is primarily a removal of oxygen and other gases from steels as the name implies, or is a method of increasing the solubility of gases in steel, has been a question of dispute.

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[1] W. Traphagen, *Transactions*, Am. Inst. Mining and Metallurgical Engrs., Vol. 62, p. 278 (1920).

[2] K. Honda and T. Murakami, *Sci. Reports*, Tohoku Univ. (1) Vol. 10, p. 273 (1921).

Poor deoxidation practice may be responsible for many difficulties in the subsequent fabrication and treatment of steel. Lately an explanation has been set forth by McQuaid and Ehn[1] for the differences observed in carburizing and case-hardening qualities of different lots of steel of apparently identical chemical composition. Their explanation of "abnormal" steel, a steel which does not case-harden properly, is that oxides exist in the finished steel, oxides which had their origin in improper furnace practice. Here again, however, the evidence connecting the observed differences in case-hardening quality with differences in oxide content of the steel is circumstantial and not supported by any analytical results for oxygen in steels of known good and poor quality. The difficulty in hardening steel from certain heats after carburization is, however, a real and serious difficulty at the present time.

The process of carburization, preliminary to case hardening, has until recently been considered as simply an absorption of carbon in the surface layers of the iron or mild steel. It has been shown,[2, 3, 4] however, that the absorption of nitrogen is a considerable proportion of the change in chemical composition of the surface layers of materials treated either with cyanide or with most commercial carburizing compounds and that this nitrogen probably plays a part in the subsequent hardening.

The "caustic embrittlement" of boiler plate steels in service is generally believed to be due to hydrogen penetrating the metal. This type of failure in boilers is usually found in sections of the plate between rivet holes and in the rivets themselves. In other words, it occurs most frequently or at least is first observed in those portions of the metal that have undergone strain or are under stress. This is in concordance with the belief, mentioned above, that strained or cold-worked metal absorbs hydrogen more readily than unstrained metal.

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[1] H. W. McQuaid and E. W. Ehn, *Transactions*, Am. Inst. Mining and Metallurgical Engrs., No. 1135-S (February, 1922).

[2] G. R. Brophy and S. B. Leiter, *Chemical and Metallurgical Engineering*, Vol. 23, p. 968 (1921).

[3] H. Fay, *Chemical and Metallurgical Engineering*, Vol. 24, p. 289 (1921).

[4] W. E. Ruder and G. R. Brophy, *Chemical and Metallurgical Engineering*, Vol. 25, p. 867 (1921).

A similar relation of the amount of deformation to hydrogen embrittlement was found by Langdon and Grossman[1] in an investigation of the embrittling effect of acid-pickling on carbon steels. In working with cold-rolled steel strips, the brittleness developed by acid-pickling was greatest for that steel which had received the greatest amount of cold work.

Welding.—The gases in steel which is to be welded by the gas torch or by the electric arc, as well as those in welding rods or electrodes, have a bearing on the strength of welded joints. Gas is sometimes liberated from the parent metal as the heat of the welding torch melts layers of metal adjacent to the weld. Gases in arc welding electrodes cause marked differences in the behavior of the molten tips of electrodes of different types of steel and iron as well as in the quality of the deposited weld metal.[2]

One of the most noticeable characteristics of electric arc weld metal is its very high content of nitride nitrogen.[3] Such metal contains from 0.1 to 0.15 per cent by weight of nitrogen, equivalent in volume to form six to nine times the volume of the weld metal. Gas weld metal also contains more nitrogen than normal steels, usually in the range 0.015 to 0.03 per cent by weight.

Enameling.—In the manufacture of enameled metal products, difficulties are sometimes encountered which are attributed to gases. It is well known that pickling articles to be enameled will cause trouble from absorption of hydrogen in the metal if too concentrated an acid is used in the pickling.[4] It has also been said that the deformation by drawing or pressing undergone by a sheet of steel before enameling may very considerably increase the difficulties due to occluded gas.

Non-Ferrous Metals.—Similar examples of effects commonly attributed to gases in the refining, working, and service of non-ferrous metals could be given in great variety: difficulties in annealing, brittleness due to hydrogen, variations in annealing temperatures due to the presence or absence of occluded gases, porosity of castings, blistering in rolled sheets, variations in electrical resistance, variations in dimensions, and so on.

APPLICATION OF METHODS OF ANALYSIS FOR GASES

The foregoing enumeration, necessarily brief and in little detail, has been given of instances in which the behavior of metals may be dependent to a greater or less degree on dissolved or combined gases originally present or introduced during service. These instances are given by way of justification for the development and application in metallurgical research of methods for the determination of gases in metals. These methods are not always easily devised and it must be admitted that such methods as we now have are far from ideal.

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[1] S. C. Langdon and M. A. Grossman, *Transactions*, Am. Electrochemical Soc., Vol. 37, p. 543 (1920).

[2] O. H. Escholz, *Electrical World*, Vol. 75, p. 1473, Vol. 76, p. 118 (1920).

[3] H. S. Rawdon, E. C. Groesbeck and L. Jordan, U. S. Bureau of Standards *Technologic Paper No. 179* (1920).

[4] C. Treischel, *Journal*, Am. Ceramic Soc., Vol. 2, p. 774 (1919).

The method for oxygen and hydrogen by vacuum fusion in contact with carbon admittedly falls short in that it does not distinguish between the different combinations or forms in which oxygen doubtless occurs in metals. The same objection, however, can be made to methods for the chemical analysis of steels for sulfur or for silicon. Yet our knowledge of the rôle of such constituents in steel has advanced from the data obtained as to the total content of these respective elements in steel.

It is hoped that the vacuum fusion method for oxygen and hydrogen described in the first part of this paper will prove of some value in the study of gases in metals. It has already been applied to several problems, for example, the determination of the gas content of steels and monel metal at various stages of the deoxidation process. The results obtained by this method of analysis show in all cases a progressive decrease in gas content which is clearly related to the stage of deoxidation or to the quantity of deoxidizer added or the method by which it was added, that is, whether in the furnace or in the ladle.

TABLE II.—OXYGEN AND HYDROGEN IN TITANIUM-TREATED RAIL STEELS

Heat Identification	Deoxidizer Added	Oxygen, per cent	Hydrogen, per cent
I	Silicon—No Ferro-Carbon-Titanium	0.0027	0.0003
J	Silicon—No Ferro-Carbon-Titanium	0.0048	nil
K	8 to 10 lb. per ton Ferro-Carbon-Titanium	0.0020	0.0002
L	8 to 10 lb. per ton Ferro-Carbon-Titanium	0.0010	nil
W	10 to 12 lb. per ton Ferro-Carbon-Titanium	nil	nil
X	10 to 12 lb. per ton Ferro-Carbon-Titanium	nil	nil

For the sake of illustration, the data from a few such tests are tabulated. In Table II are shown the results of gas analyses on samples from six heats of rail steel.[1] The first two heats were deoxidized with silicon; in the four remaining heats additions of ferro-carbon-titanium took the place of the silicon deoxidation, with greater proportions of ferro-carbon-titanium added in the fifth and sixth heats than in the third and fourth. The results for oxygen and hydrogen in steels thus treated show clearly the decrease and final complete removal from the steel of all oxygen, probably including that combined as oxides of iron, silicon, and manganese.

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[1] These data are taken with permission from a U. S. Bureau of Standards Technologic Paper by G. K. Burgess and G. W. Quick which is now in preparation.

A similar correspondence between gas content of the metal and deoxidation practice was shown by the analysis of samples of monel metal.[1] Samples representing three stages in the deoxidation showed progressive decreases in gas content, while material which had received all additions of deoxidizers showed differences in gas content depending on whether the additions had been made wholly or in part in the furnace, where time for reaction was probably allowed, or whether the deoxidizers were simply added to the ladle. These results are summarized in Table III. The first three samples were all from the same heat.

TABLE III.—GASES IN MONEL METAL

Deoxidation	Oxygen, per cent	Hydrogen, per cent
None	0.0214	0.0026
Partial	0.0156	0.0017
Complete, in ladle	0.0029	0.0009
Complete, in furnace	nil	0.0005
Partly in furnace, completed in ladle	nil	0.0004
Complete, in ladle	0.0050	0.0011
Complete, in ladle	0.0015	0.0008

This method of analysis is at present being employed in more complete and systematic studies of the progress of refining and deoxidation in several ferrous and non-ferrous materials and to the investigation of a number of the problems mentioned in this paper, as the study of the quality of case-hardening steels, caustic embrittlement of boiler-plate, "flaky" steel and the quality of metals for enameling.

Acknowledgments.—The careful comparisons of various vacuum fusion methods for determining gases in metals and the development of the modified Walker-Patrick method finally employed, have been carried out by Mr. J. R. Eckman, Associate Chemist of the Bureau of Standards; valuable assistance in analytical work has been given by Mr. W. P. Barrows, Assistant Chemist; Mr. J. R. Cain, formerly Chemist of the Bureau, rendered valuable advice and assistance in the early development of the vacuum fusion method for gases.

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[1] These samples were from special castings furnished by courtesy of the International Nickel Co.

DISCUSSION

Mr. James A. Aupperle[1] (*presented in written form*).—We have always been interested in the determination of gases in ferrous metals, and have done considerable work in determining the effect of some of these gases, from a corrosion standpoint, in iron and steel. We are interested in Mr. Jordan's and the U. S. Bureau of Standards' work on gases in iron and steel to the extent of maintaining a fellowship at the Bureau of Standards at Washington. **Mr. Aupperle**

We have made atmospheric corrosion tests on bessemer steel obtained from various sources in which the copper content was fairly uniform, and in which the nitrogen content varied from 0.006 to 0.019 per cent. The results of these tests showed that the sheet containing the high nitrogen lasted 13 months, and the sheet containing the low nitrogen lasted 33 months or almost three times as long. We have studied the failures of other forms of ferrous metals in the form of pipe and spikes. Both of these materials corroded very rapidly. The pipe contained 0.041 per cent of nitrogen and the spikes contained 0.033 per cent of nitrogen. This is almost ten times as high as the nitrogen content of commercially pure iron.

Mr. Jordan's summary is probably too broad. It states:

> "The relations between solubility of gases in metals and temperature are somewhat different from the usual conceptions of the solubility of gases in ordinary liquids such as water. Many observations lead to the conclusion that gases are dissolved in metals chiefly, if not entirely, in the intercrystallin cement."

This is only true in metals having intercrystallin brittleness. It is a well-established fact that gases combine with iron and these constituents can be seen under the microscope within the crystals themselves and not in the intercrystallin cement. Oxide of iron, for instance, occurs in ferrous metals in the form of a finely divided globular constituent. It is also well known that nitrogen forms nitride needle-shaped crystals which can also be identified by the microscope as existing in the crystals and not in the intercrystallin cement.

It is a recognized fact that either hydrogen, oxygen or nitrogen causes brittleness in ferrous metals. We have studied electrolytic iron containing several hundredths of one per cent of hydrogen. This

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[1] Chief Chemist, American Rolling Mill Co., Middletown, Ohio.

Mr. Aupperle iron was so brittle that it could be shattered with a hammer. The same iron, however, when heated in an ordinary Bunsen flame, became very malleable. After heating in the atmosphere to 1000° C., the metal was no longer brittle. Analysis showed it to be free from hydrogen. By taking this malleable piece of iron free from hydrogen and heating it in an oxidizing atmosphere to 1000° C., it was possible to render the iron brittle due to what is known as intercrystallin brittleness. Investigations have shown that such brittleness is due to the presence of ferrous oxide in solid solution. The difference between the oxide of iron disseminated throughout the crystals of ductile iron and the oxide of iron in solid solution which is responsible for intercrystallin brittleness can be determined microscopically. The former needs no etching while the latter constituent requires a special etching reagent to resolve it.

The malleable electrolytic iron can be heated in an atmosphere of ammonia at high temperatures with the formation of iron nitride. We have prepared some iron containing as much as 7 per cent of nitrogen and have found this material to be as brittle as glass at room temperature.

Stromeyer[1] has shown that nitrogen is nine times as harmful as phosphorus, rendering the metal extremely cold short.

It is difficult for one to grasp the significance of the large amount of gas which can be held in solution by various metals. Mr. Jordan has shown that various investigators have found that 1 cu. ft. of steel will evolve from 70 to 200 cu. ft. of gases when the metal is fused.

What Mr. Jordan says about the manufacturers of gray iron specifying the oxygen content is very interesting, as it shows that metallurgists are realizing the importance of controlling the gas content of their products.

The United States patent office granted patents several years ago for ferrous metals in which the oxygen content was limited.

Many years ago we started an investigation on the gas content in ferrous metals and inspected the interior of an ingot after the ingot had been split with a circular saw. The vast difference between the honey-combed structure of mild steel as compared with commercially pure iron which was uniform, dense and almost free from blowholes, indicated one of the reasons for the superiority of commercially pure iron over steel for vitreous enameling purposes. The gas content of the steel is responsible for blisters, warping of the metal and fish scaling of the enamel.

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[1] *Journal*, Iron and Steel Institute, Vol. LXXIX, No. 1, p. 404 (1909).

Mr. Schwartz

MR. H. A. SCHWARTZ[1] (*presented in written form*).—The fact that metallurgical products of identical composition, but of different methods of manufacture, vary in physical properties seems thoroughly established. In the absence of data there is always a tendency to ascribe such variations to the presence of undetermined elements.

For many years Mr. Moldenke ascribed many unexplained variations in the pouring and annealing properties of cast iron, and particularly of malleable cast iron, to the presence of oxygen and oxides. Others, including the writer, were inclined to doubt the possibility of the existence of iron oxide in cast iron. Still later, Johnson ascribed favorable properties to oxygen in iron, and incidentally many analysts, including some of the writer's associates, apparently found considerable amounts of oxygen.

We are now shown, largely through the exhaustive work of Cain, and now of Jordan at the U. S. Bureau of Standards, that all former conclusions can, at most, be shrewd conjecture, and that no firm basis of fact exists upon which our ideas may be founded. Such a basis is now being established at the Bureau of Standards, and it seems likely that we must revise some of our opinions in the light of reliable analytical information. The writer's laboratory has already benefited by the co-operation and advice of Mr. Jordan in our study of the oxygen problem to the extent that there has been established the fact that a very large part of the oxygen content of white cast iron is introduced during sampling.

The view that no large amounts of oxygen existed in high-carbon metal has received adequate analytical support after its apparent contradiction by the results of the Ledebur method determinations. It seems that we are on the verge of a decision as to whether we must regard oxygen as of any commercial importance in various products, and particularly in cast irons. The publication of the Bureau's complete results will be eagerly awaited by those who have occasion to study the problem.

It is much to be regretted that the only accurate method for determining these elements requires the use of such expensive equipment. As a rule, the rôle of any element in metallurgy becomes well understood only when its determination becomes common practice. High frequency transformers are a luxury beyond many laboratories, and the fact that the cost of equipment for this determination may easily exceed that for all the rest of the work of a small works' laboratory will undoubtedly delay the introduction of such work in many

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[1] Manager of Research, The National Malleable Castings Co., Cleveland, Ohio.

Mr. Schwartz places. Unfortunately, also, the method will probably give the oxygen contained in sonims, and it is very doubtful whether such oxygen, *per se*, affects the metal.

If the cost handicap can be overcome, and some means devised for distinguishing between the oxygen of the metal and of slag inclusions, the foundry industries will be furnished a long-needed means of extending their knowledge in a direction where now little beyond speculation is possible.

Mr. Styri MR. HAAKON STYRI.[1]—This is a very interesting paper and I believe the results show a distinct advance over previous work. There is some information missing, however, that I think could easily have been given or at least indicated, by a metallographic study of the slag inclusions. You will see my point in referring to Table II which by analysis gives a smaller quantity of oxygen with increasing additions of ferro titanium. That is easily explained. If to molten steel you add titanium or another element which combines with oxygen, you will get an oxide which is very difficult to reduce, and you will not get evolution of carbon monoxide at the temperatures used in these fusion tests. If you have the reduction performed with silicon and manganese, you get some form of silicate inclusions in the steel, and these inclusions can readily be reduced by the carbon present in the steel at these temperatures. In other words, the analyses given do not show that the steel reduced by the titanium is free from oxide inclusions, and that is of course what the consumer is interested in. There might be just as many inclusions after zirconium or titanium reduction or deoxidation as with silicon and manganese.

Another point refers to the general practice in steel mills—it is well known that when aluminum is added to ingots, gas evolution from the ingot is prevented, if it is not added reduction of silicon manganese compound present may result with corresponding formation of blowholes. I hope that this question will be taken up in more detail in the technologic paper which is promised and proved with microscopic analyses of the samples before analyses are made for oxygen by this or similar methods. It will probably be very difficult to show inclusions after the fusion is made, because the samples are so small. I have never been able to find any inclusions in spoon samples from a melt, but I have found spots indicating the presence of oxide. I do not think we will advance greatly until we find some method of determination of the solubility of gases in steel at different temperatures, and in particular, the changes in solubility from the solid to the liquid state for oxygen, carbon monoxide and nitrogen.

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[1] Chief of Research Laboratory, S. K. F. Industries, Philadelphia, Pa.

Mr. Outerbridge

MR. A. E. OUTERBRIDGE, JR.[1]—I take the privilege of saying a few words on this subject, confining myself entirely to the discussion of cast iron. Nearly twenty years ago I exhibited two cast-iron bars before the American Institute of Mining Engineers.[2] These bars had been cast in one mold; they were test bars poured from one ladle of soft iron, such as is used for making pulleys or small parts. Both bars were 15 in. long when poured in the mold and 1 in. square in section. For the purpose of preventing any change in their length by rapping of the pattern, two iron yokes were used to form the ends of the molds. The shrinkage of the bars was measured by inserting the bars in a standard yoke mounted on a frame. When these bars were removed from the mold they both measured $14\frac{13}{16}$ in. in length and 1 in. square in section. When exhibited, one bar was of this length and section; the other bar was $16\frac{1}{2}$ in. long and $1\frac{1}{8}$ in. square in section—an increase of 40.98 per cent in cubical volume! This "growth" of cast iron, as it has been designated by engineers, was caused by frequent heating of the bar to about 1400° F. and cooling, with special efforts to prevent oxidation. In heating the bars they were placed in a steel pipe 20 in. long, which would hold eight bars side by side. A small piece of charcoal was placed in the pipe and the ends closed with clay. The pipe was placed in the furnace and heated to a high temperature; we found in the course of our investigations that the best temperature for creating the largest growth in one heat was about 1400° F., although a much lower temperature would accomplish the purpose. It was found that with one such heating the bars had "grown" about $\frac{1}{8}$ in. in length, with a slight though measureable increase in cross-section. At the end of the third heating, the bars would no longer go in the standard yoke, showing that they had taken up all the original shrinkage of the metal. That process was continued in the case of the bar referred to for 54 heatings, resulting, as previously mentioned, in a length of $16\frac{1}{2}$ in. and dimension in cross-section of $1\frac{1}{8}$ in. There was so little difference in the weight of bars thus treated compared with untreated comparison bars, that I stated in my paper at the time that there was practically no increase in weight.

The bars thus treated retained their metallic properties. When machined and scraped and examined under a low-power magnifying glass, a difference in grain or structure could be observed, and when the bars were broken there was an easily discernible difference in fracture.

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[1] Metallurgist, William Sellers and Co., Inc., Philadelphia, Pa.

[2] A. E. Outerbridge, Jr., "The Mobility of Molecules of Cast Iron," *Transactions*, Am. Inst. Mining Engrs., Vol. 35, 1904; *Journal*, Franklin Institute (1904).

Mr. Outerbridge There was evidence of internal gas in the treated bars. Thus, specific gravity determinations were made on pieces sliced from both untreated and treated bars. Specific gravity was determined by first weighing in air and then in distilled water. The specific gravity of the untreated bar was found to be about 7.2. When the piece from the treated bar was dropped into the distilled water, gas began to come off and continued coming off for one-half hour. The piece was taken out, dried on the surface, and the increased weight showed the amount of water that it had absorbed. And what was the gas that came out? It was nothing but common air. The metal had become porous by the pressure of expanding gases in the bar causing separation of the molecules; they were unable to go back again and the piece was full of air which entered as the bar cooled. When it was dropped into the water, the water seeped in and drove the air out. After driving off the water, the piece was varnished to prevent re-absorption of water when again immersed and specific gravity was determined as 6.3.

I have stated that special efforts were made in these experiments to prevent the oxidation of the bars in heating and cooling them by placing with them in the closed steel pipe a piece of charcoal. These experiments were subsequently (1909) taken up in England by Carpenter and Rugan under appropriation, I was informed, from the Carnegie Research Foundation. Toward the end of the year, a paper was published by these investigators,[1] in which they said that the statement of mine that the two companion bars were alike in weight was incorrect. They found that their heat-treated bars increased in weight, but when I examined the paper I found that they had made no provision whatever to prevent oxidation, the very thing that I was anxious to avoid.

My opinion is that gases in cast iron do exist, and if you are sufficiently interested to look at the *Journal* of the Franklin Institute for March, 1888, you will find a paper there, an illustrated paper, entitled "The Relationship Between the Physical Properties and the Chemical Constituents of Cast Iron," in which the prognostication is made, that oxygen will be found in all cast iron melted in cupolas. Now that was a good many years ago. There was no opportunity of proving such a thing, but that is in writing and I am consequently exceedingly interested in this line of investigation.

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[1] H. C. H. Carpenter and H. F. Rugan, "The Growth of Cast Irons after Repeated Heatings," *Journal*, Iron and Steel Institute, No. II, 1909.

THE SIGNIFICANCE OF TOOL TEMPERATURES AS A FUNCTION OF THE CUTTING RESISTANCE OF METALS

By H. A. Schwartz[1] and W. W. Flagle[2]

Introduction

A number of observers, notably Benedict and Lukens,[3] have studied the stresses in twist drills. Smith and Barr[4] have briefly studied the behaviour of drills in malleable iron. Olsen[5] has described a machine well suited for such investigations. The present authors have published data obtained in investigating by such a machine the relation between drill stresses, the physical properties of the material, and the cutting conditions when working malleable cast iron.[6] No information, however, was available dealing with tool life, and an extension of the work in a direction calculated to give information along this line seemed warranted in the Research Department of the National Malleable Castings Co.

Herbert[7] has demonstrated that the life or performance of a tool steel is a function of its temperature. It seemed, therefore, that if to the readily measured drill stresses were added information as to temperature, the factors affecting tool life, when working various materials, would be more fully available.

Apparatus

We accordingly set about adapting the Olsen Efficiency Machine to give information also as to drill temperatures. A set of high-speed twist drills, $\frac{3}{4}$ in. in diameter and 3 in. long (in the flutes), having two oil holes in the body between the flutes, were made for us by the Cleveland Twist Drill Co. The drills, when dull, were in all cases automatically ground to standard form by the maker. As a matter of fact, regrinding was required only on two occasions. Repeated tests with standard material on drills in use never showed dulling due to wear in an amount indicated by changes in the observed constants.

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[1] Manager of Research, The National Malleable Castings Co., Cleveland, Ohio.

[2] Engineer of Tests, Research Department, The National Malleable Castings Co., Cleveland, Ohio.

[3] "An Investigation of Twist Drills," *Bulletin No. 103*, Engineering Experiment Station, University of Illinois, Vol. XV, No. XIII (1914–18).

[4] "The Relation Between Machining Qualities of Malleable Castings and Physical Tests," *Transactions*, Am. Found. Assoc., Vol. XXVIII, p. 330.

[5] T. Y. Olsen, "An Efficiency Testing Machine for Testing Taps and Dies," *Proceedings*, Am. Soc. Testing Mats., Vol. XIV, Part II, p. 541 (1914).

[6] "Malleable Iron Drilling Data," *Journal*, Soc. Automotive Engrs., July, 1922.

[7] "The Cutting Properties of Tool Steel," *Journal*, Iron and Steel Institute (British), 1910-I, p. 206.

The shanks of the drills were provided with flat lands to permit secure chucking in the fixed spindle of the testing machine. An iron-constantan couple was introduced into each oil hole, the junction being just inside the end of the hole and near the cutting edge. The fact that the two couples read within a few degrees of each other whenever comparisons were made, shows that either indicates closely the temperature of the drill. Quite probably, the actual cutting or rubbing surface of the drill is a little hotter than the couple. This error is probably small, since the couple was within $\frac{3}{32}$ in. of these surfaces. No accurate calibration of the couples was attempted, since it was realized that the results would be comparative, and influenced to some extent by the thermal conductivity of the drill, the thermal capacity of the chuck, etc.; conditions which could be kept constant but not readily be given absolute values.

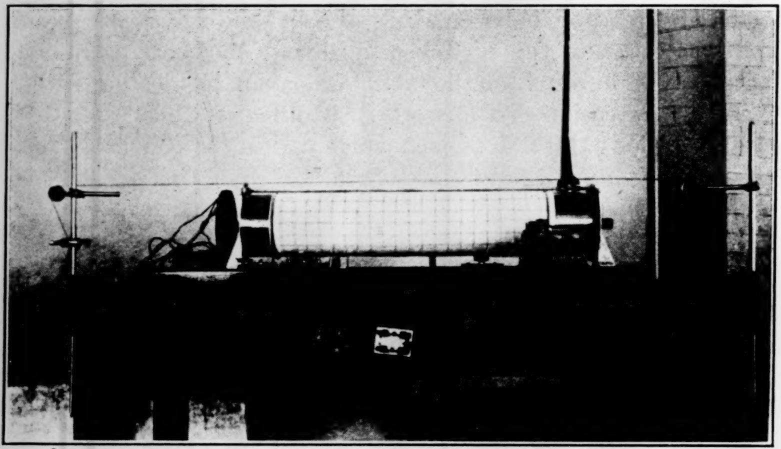

FIG. 1.—Semi-Autographic Potentiometer Recorder

The drill temperature was plotted against penetration by a special semi-autographic potentiometer recorder. This recorder was designed by the senior author and built under the junior author's supervision. Credit is due to H. V. Williams, of this company, for aid in the construction, to A. F. Gorton, of this laboratory, for the adjustment and calibration of the resistance coils, to Clifford S. Fuller, of this laboratory, for assistance throughout the investigation, and to The Leeds and Northrup Co. for material for the potentiometer wire and resistances.

The potentiometer is shown in Fig. 1, and consists essentially of a horizontal shaft carrying the potentiometer wire, wound in one turn on the rim of a hard rubber disk, and also the recorder drum, 15 in. in circumference and 24 in. long. A recording pen travels along the upper element of the recorder drum, being actuated by a string passing over suitable pulleys and connected to the movable head of the machine. The cord is kept tight by a weight at its free end, and actuates the pen by a spring clip which would slip if, through carelessness, connections were so made as to pull the pen against the potentiometer frame.

A galvanometer and the usual rheostats and switches for adjusting the current in the potentiometer circuit by comparison with a standard cell are mounted on the instrument. The resistances are so arranged that when paper ruled to 0.1-in. squares is used on the drum one division corresponds to 2° C. The range is thus 300° C., but by suitable resistances at the end of the wire the zero point of the drum can be made 0, 150, or 300° C.; so that temperatures up to 600° C. can be recorded. A further negligible additive correction of a little over 2° C. exists, due to the resistance of a flexible lead to the drum wire.

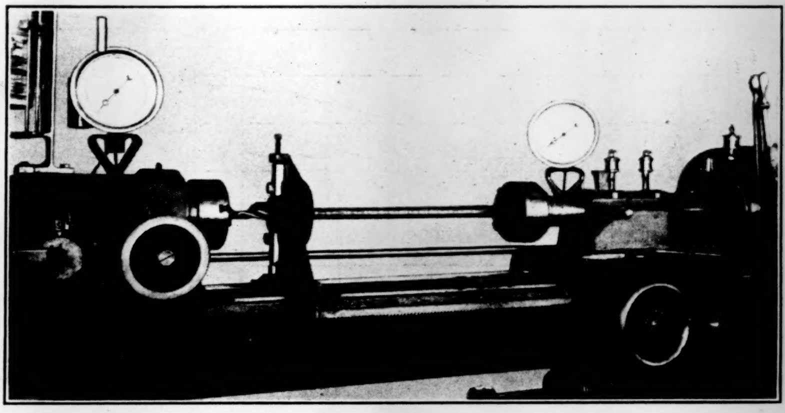

FIG. 2.—Illustrating Set-up of Drill and Specimen

SPECIMENS

The conditions require that the drill be stationary and short; hence, the specimen must rotate, and in order that the amount of penetration be sufficient to allow the temperature to come to equilibrium, a very long specimen, many times longer than the drill, is required. This result was accomplished by using a specimen 24 in. long, finished to 1 in. in diameter, rotated by the revolving spindle, and fed against the drill point through a ball bearing held in a steady rest fixed just in front of the drill point. A fiber bushing serves to minimize heat conductivity from the bearing to the specimen. The specimen has key-ways cut at opposite ends of a diameter and extending its full length. These key-ways are of such depth that their bottoms are a little over $\frac{3}{4}$ in. apart. As the drill, which is $\frac{3}{4}$ in. in diameter, penetrates, a few thousandths of an inch of metal remain. At axial intervals of 1 in., the diameter of the specimen is reduced by circumferential grooves to a few thousandths under $\frac{3}{4}$ in. Therefore, at 1-in. intervals of penetration, an annular ring is detached which can be split at the key-ways and removed. It is thus possible to secure a penetration of 19 in. in all, limited by the length of the machine, without reference to the drill length. The set up is shown in Fig. 2.

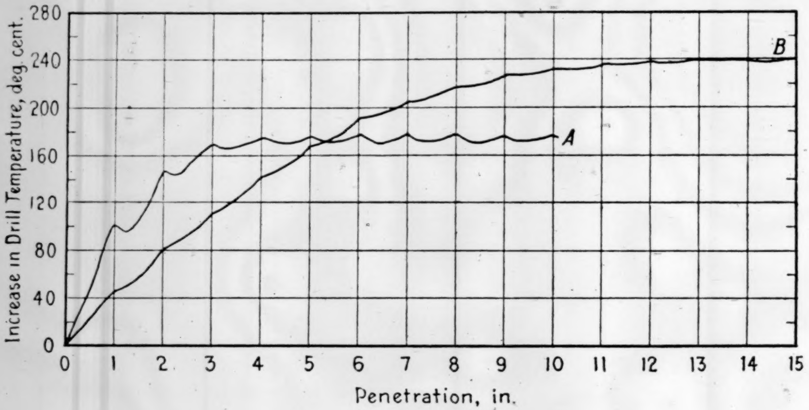

FIG. 3.—Plot of a Typical Potentiometer Record. (Not to scale)

EXPERIMENTAL PROCEDURE

The procedure consisted in reading, or recording, the torque and thrust of the standard drill, and following by the potentiometer its temperature until this became stationary. The potentiometer records are similar to Fig. 3. The intermittent saddle-shape depressions, especially visible at low speeds, are due to the changes in depth to which the drill is surrounded by the material.

Tests on each material were conducted at as many combinations of feeds of 0.0025, 0.005, 0.01, 0.02, and 0.04 in. per revolution and speeds of approximately 40, 80, 160, 320, 460, 640, and 760 r.p.m. as were within the power capacity of the machine, or the temperature range of the pyrometer.

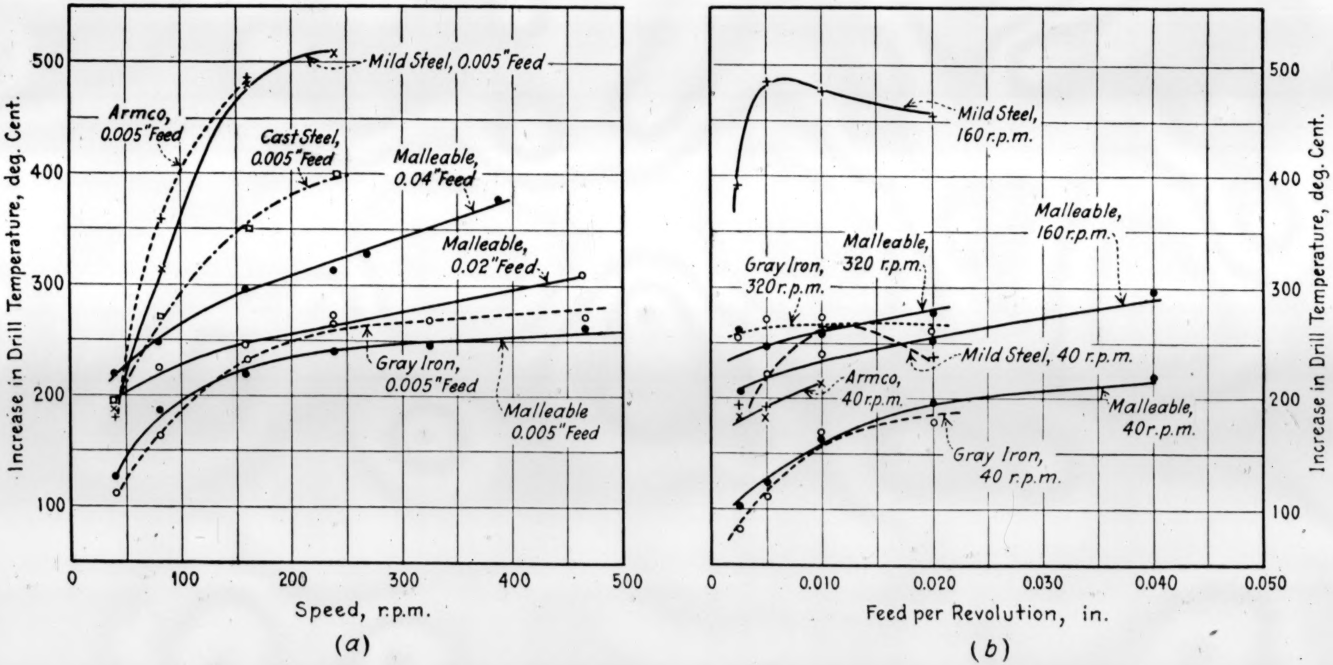

FIG. 4.—Typical Relations Between Drill Temperature and (*a*) Speed at Constant Feed per Revolution; (*b*) Feed per Revolution

The following materials were to be investigated:

Armco Iron;
Wrought Iron;
Hot-Rolled Machinery Steel;
Cold-Rolled Steel Shafting;
Annealed Cast Steel;
Annealed (Eutectoid) Drill Rod;
A Special Heat-Treated Steel;
Three Specimens of Malleable Cast Iron;
One Specimen of Gray Cast Iron.

The wrought iron proved unworkable in the test due to slag inclusions which dulled the drill. The drill rod was so hard that it could not be held in the chucks even against a light slow cut.

DATA

The observed data are shown in Table I. We have previously shown[1] that neither torque nor thrust is greatly affected by speed, and only in some of the tougher materials is it really necessary to record these constants at all speeds. The table, however, records these quantities for all the tests.

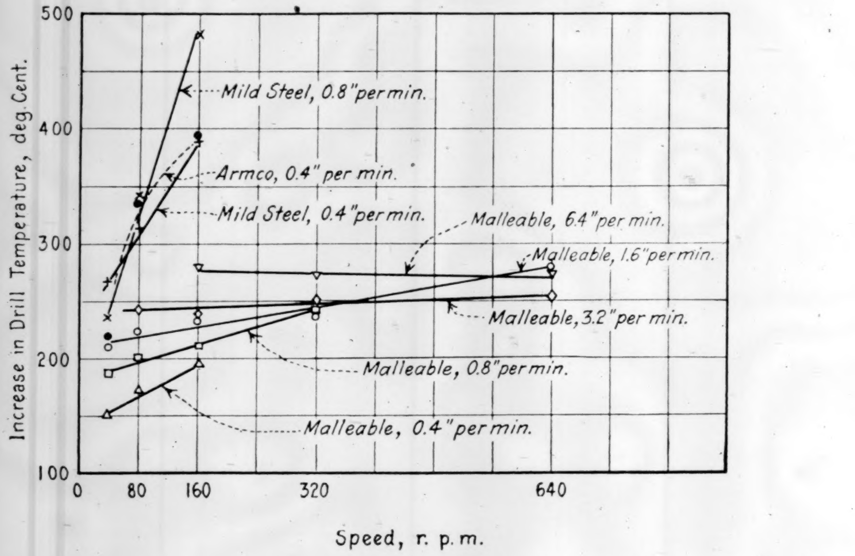

FIG. 5.—Drill Temperature at Constant Rate of Penetration as a Function of the Speed of Revolution

INTERPRETATION OF DATA

The tabular data may be used to determine the machining properties with respect to torque, thrust, temperature, etc., of the several materials under varying conditions of feed and speed. Some

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[1] *Loc. cit.*

TABLE I.—TEST DATA.

Feed per Revolution, in.	Speed, r.p.m.	Torque, in-lb.	Thrust, lb.	Temperature above Room Temperature, deg. Cent.
COLD-ROLLED STEEL. BRINELL NO. 191				
0.0025	238	70	450	382
	325	120	800	Over 600
0.005	238	200	1050	492
0.010	40	265	1350	262
	80	265	1250	304
	158	265	1450	372
	238	290	2050	552
0.020	40	400	1950	260
	80	415	2050	334
	158	400	2250	504
HOT-ROLLED MACHINERY STEEL. BRINELL NO. 109				
0.0025	40	120	350	186
	80	110	400	278
	158	110	350	388
	238	115	650	494
0.005	40	155	550	186
	80	165	650	312
	158	185	950	482
	238	195	950	510
0.010	40	270	1000	266
	80	270	1050	340
	158	320	1450	474
0.020	40	470	1700	238
	80	530	2200	404
	158	525	2300	454
SPECIAL HEAT TREATED STEEL. BRINELL NO. 223				
0.0025	40	105	350	180
	80	110	350	305
	158	110	350	406
0.005	40	175	650	240
	80	200	750	390
	158	225	1050	Over 600
0.010	40	320	1200	352
	80	390	1850	506
0.020	40	575	2450	362
ANNEALED CAST STEEL. BRINELL NO. 131				
0.0025	40	140	300	217
	80	120	300	246
	158	90	350	308
	238	75	400	356
0.005	40	150	700	192
	80	150	700	270
	158	150	700	350
	238	150	700	400
0.010	40	260	1200	218
	80	260	1100	306
	158	260	1100	370
GRAY IRON. BRINELL NO. 143				
0.0025	40	50	225	80
	80	55	225	132
	158	60	300	220
	238	55	275	260
	325	45	225	254
	464	52	275	302
	628	45	225	299
	766	45	225	322
0.005	40	85	400	108
	80	80	400	164
	158	85	450	232
	238	92	475	272
	325	80	425	268
	464	75	400	269
	628	[a]	[a]	[a]
0.010	40	160	500	168
	80	155	550	208
	158	135	500	216
	238	135	550	250
	325	140	550	270
	464	135	550	286
0.020	40	265	1200	177
	80	260	1200	216
	158	235	1150	246
	238	230	1050	248
	325	220	1000	258
	464	220	950	288
HIGH-CARBON MALLEABLE. BRINELL NO. 113				
0.0025	40	50	150	83
	80	45	125	128
	158	45	125	174
	238	45	150	206
	325	45	125	226
	464	45	150	250
	628	45	150	274
	766	45	150	276
0.005	40	75	250	98
	80	75	250	144
	158	75	275	202
	238	75	275	226
	325	80	275	235
	464	75	250	249
	628	75	250	258
	766	75	250	276
0.010	40	135	425	146
	80	135	475	190
	158	135	450	222
	238	135	500	226
	325	135	500	240
	464	135	500	253
	628	135	500	265
	766	135	500	278
0.020	40	250	1100	177
	80	245	1050	212
	158	235	1000	238
	238	245	1050	230
	325	235	1000	258
	464	240	950	286

[a] Drill failed.

TABLE I.—*Continued.*

Feed per Revolution, in.	Speed, r.p.m.	Torque, in-lb.	Thrust, lb.	Temperature above Room Temperature, deg. Cent.
Armco Iron. Brinell No. 89				
0.0025	40	130	300	180
	80	125	500	282
	158	120	600	390
0.005	40	160	600	180
	80	200	1000	360
	158	200	1100	484
0.010	40	230	1000	217
	80	330	1500	428
Certified Malleable "A". Brinell No. 118				
0.0025	40	55	200	78
	80	55	200	124
	158	55	200	182
	238	55	200	212
	325	55	200	234
	464	55	200	250
	628	55	200	256
	766	55	200	270
0.005	40	85	300	106
	80	85	300	154
	158	85	300	198
	238	85	300	218
	325	85	300	226
	464	85	300	240
	628	85	300	248
	766	85	300	258
0.010	40	140	700	140
	80	140	700	188
	158	140	700	222
	238	140	700	236
	325	140	700	238
	464	140	700	244
	628	140	700	264
	766	140	700	266
0.020	40	245	1500	180
	80	245	1500	216
	158	245	1500	228
	238	245	1500	248
	325	245	1500	268
	464	245	1500	300
0.040	40	475	2800	198
	80	475	2800	236
	158	475	2800	258
	238	475	2800	300

Feed per Revolution, in.	Speed, r.p.m.	Torque, in-lb.	Thrust, lb.	Temperature above Room Temperature, deg. Cent.
Certified Malleable "E". Brinell No. 112				
0.0025	40	55	200	100
	80	55	200	143
	158	55	200	204
	238	55	200	248
	325	55	200	260
	464	55	200	268
	628	55	200	296
	766	55	200	270
0.005	40	85	300	124
	80	85	300	186
	158	85	300	220
	238	85	300	240
	325	85	300	246
	464	85	300	260
	628	85	300	260
	766	85	300	276
0.010	40	155	600	162
	80	155	600	212
	158	155	600	238
	238	155	600	252
	325	155	600	260
	464	155	600	266
	628	155	600	288
	766	155	600	326
0.020	40	265	1300	196
	80	265	1300	224
	158	265	1300	248
	238	265	1300	265
	325	265	1300	276
	464	265	1300	310
	628	*a*	*a*	*a*
	766	*a*	*a*	*a*
0.040	40	550	2600	218
	80	550	2600	248
	158	550	2600	296
	238	550	2600	314
	266	550	2600	328
	388	550	2600	376

a Motor would not pull.

general conclusions may be reached more easily by a study of portions of the experimental data when reduced to graphical form.

In Fig. 4, the temperature of the drill at constant feed is plotted against speed, and also at constant speed against feed. A few typical cases only are plotted in order that the relations may be readily visualized. These are evidently somewhat complex, and incapable of being brought under any simple generalization.

In Fig. 5 the rise in temperature of the drill is plotted against revolutions per minute for constant rates of penetration ranging from 0.4 to 6.4 in. per minute on malleable cast iron (average of two materials of "Certified" grade). Only this material permitted of tests at sufficiently high cutting rates to give a complete series of graphs. Data on machinery or mild steel at two rates, and on Armco iron at one rate, are included for comparison.

It seems that there is an evident linear relation between rate of rotation and tool temperature, assuming a constant rate of penetration. It appears, also, that the temperature increases more rapidly with speed at low rates of linear penetration than at high, and that the tendency is for the temperature to become independent of speed at high rates of penetration per unit of time. Very possibly, similar conclusions would be reached with regard to the other materials had we a sufficient range of data. The tables will indicate that the other steels tested behave similarly to mild steel and Armco iron.

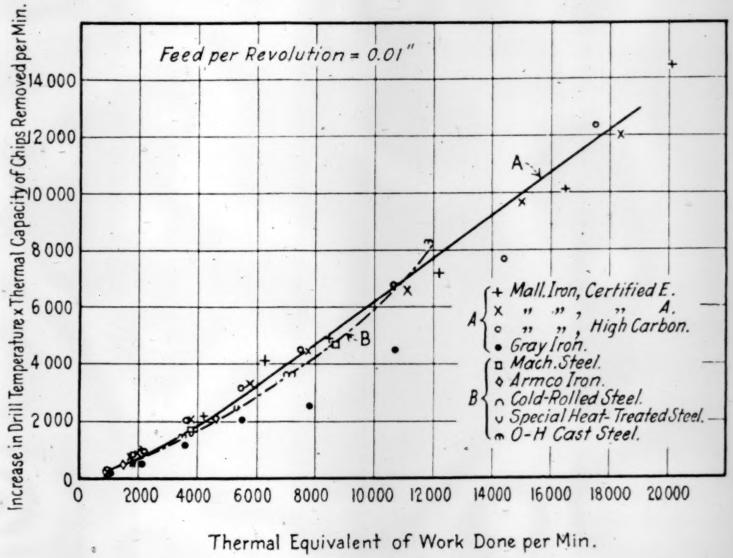

FIG. 6.—Relation between Temperature, Rate of Removal of Metal and Energy Input for all Materials, for Feed per Revolution of 0.01 in.

The work done upon the material, and measured by the dynamometer readings and speed counter of the machine, is expended in severing the metal, curling up the chip, and in overcoming friction between the drill, chips, and specimens. This work reappears as heat which is dissipated in the sensible heat of chips and specimen, and by conduction into the machine, and radiation from the specimen and its surroundings.

There is no assurance that the drill temperature is that of the chips; very probably the drill is a little colder than the face of the chip which rubs against it, and this face in turn is the hottest portion of the chip. In any event, since the chips constitute a constant proportion (nine-sixteenths) of the specimen heated, and the heat transmission to the machine is relatively small and constant, it seems logical to compare the product of drill temperature and thermal capacity of chips produced with the work done. The comparison is easiest if both are reduced to calories (small) per minute. While this appears to be a comparison with the total heat of the chips on the tacit assumption that chip and drill temperatures are identical, we would distinctly disclaim any belief that we have measured the actual sensible heat of the chips. The data are expressed in this form merely as a convenient means of comparison. This computation and comparison has been made for each of the tabular values. In the interest of brevity, the data for all the materials at a feed of 0.01 in. per revolution are shown graphically in Fig. 6. Here the abscissas are the heat equivalents of the work done per minute, and the ordinates the product of the thermal capacity of the chips produced per minute times the increase in drill temperature. It will be noted that, except for gray iron (where the results were somewhat erratic throughout), these very dissimilar metals yield surprisingly concordant results. The variation is, indeed, not far from the limits of error of the dynamometer readings. More particularly in the rolled materials, the recorded readings are correct averages of somewhat

TABLE II.—ENERGY REQUIRED TO REMOVE 1 CU. IN. OF MATERIAL

Values Expressed in 1000 in-lb.

Material	Brinell No.	Feed per Revolution, in.				
		0.0025	0.005	0.010	0.020	0.040
Armco Iron	89	714	536	402	...	...
Wrought Iron	93	*a*	*a*	*a*	*a*	*a*
Hot Rolled Machinery Steel	109	672	497	405	367	...
Certified Malleable "E"	112	315	244	221	191	202
High Carbon Malleable	113	256	215	193	173	...
Certified Malleable "A"	118	315	244	201	179	177
Annealed Cast Steel	131	607	432	374	...	...
Gray Iron	143	298	237	206	170	...
Cold Rolled Steel	191	*b*	*b*	394	350	...
Special Heat Treated Steel	223	588	599	511	417	...
Annealed (Eutectoid) Drill Rod	237	*a*	*a*	*a*	*a*	*a*

a Not machineable under test conditions.
b Drill ran out of center.

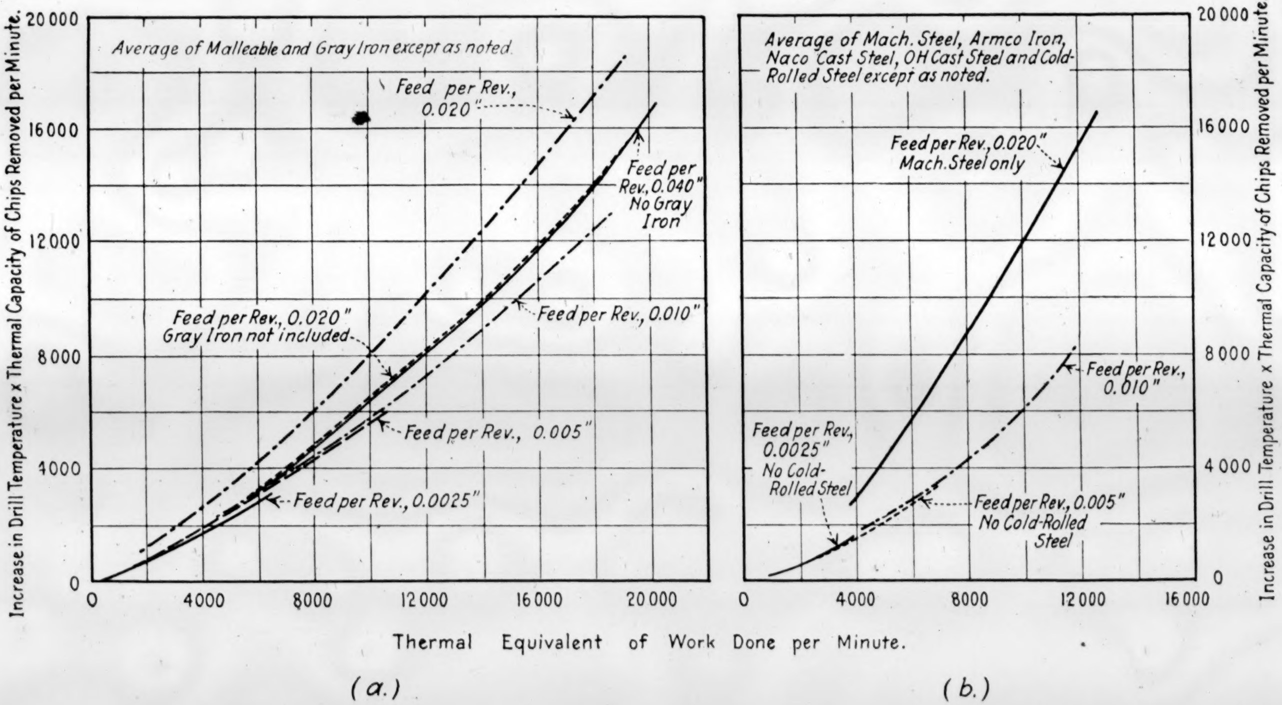

FIG. 7.—Average Relation between Temperature, Rate of Removal of Metal and Energy Input for (*a*) Malleable and Gray Cast Iron, and (*b*) Steel; for Constant Feed per Revolution

widely fluctuating values, while in the cast irons the dynamometer readings throughout a given run are very constant. The latter, therefore, furnish more reliable data for comparison than the former. Two lines are plotted in the figure, one representing the average of the data for malleable and gray iron, the other for the series of steels.

In Fig. 7 (*a*) the average data for the cast irons have been plotted for each feed used. At the highest feed, gray iron could not be cut. The data for a feed of 0.02 in. per revolution are given, including and excluding that erratic material.

Note that the ratio under consideration changes relatively little even under a wide range of feed and speed conditions. The curves for fine feeds are very nearly superimposed on the lower ends of the curves for coarser feeds.

In Fig. 7(*b*) the available data for the steels are similarly plotted. Here the curve at a feed of 0.02 in. per revolution departs considerably from any expected relationship. No explanation is at hand for the abnormal temperatures observed, and, unfortunately, we have been unable to machine other similar materials at this rate for comparison.

In passing, we would invite attention to the lack of relationship between cutting hardness and Brinell number. The malleable cast iron, Armco iron, and mild or machinery steel, all have substantially similar Brinell hardness, yet the resistance to cutting varies widely as shown in Table II.

We would also call attention to the increase in drill stress at the higher speeds for some of the steel series. May this be an expression of increased resistance to cutting due to the higher temperature of the metal? We hope to continue our investigation under conditions involving the artificial cooling of the tool and metal in order to determine this point.

CONCLUSIONS

The experiments herein described lead us to the conclusion that, given only constant dimensions of test specimens and drill, the drill temperature furnishes a fairly satisfactory and very cheap means of comparing the cutting resistances of different metals even through a rather wide range of materials and feeds.

A calibration curve could be fairly accurately established from dynamometer tests by which the energy input could be closely approximated for any known rate of removal of metal and drill temperature. This curve is not a straight line; hence, the temperature is not directly an accurate measure of the energy required to remove a unit volume of the material.

A cheap clamp for holding the drill and steady rest could be mounted on the carriage of any lathe and render it available for comparisons of cutting hardness by this method. We hope to adapt this method of investigation to the turning operation where the need of rigidity under heavy cuts renders an application of a dynamometer very difficult.

In the past, tool loads have been regarded as relatively of slight significance in machine-shop operation. It was thought sufficient to meet increased loads by increased power of the machine and strength of the tool. We have shown that the increased loads find direct expression in increased tool temperature, and hence are of primary importance in determining tool life.

We would point out further, that while we have not yet investigated the effect of cutting compounds, it may be assumed that these act primarily by removing heat from the system. Such a removal of heat can be beneficial only if the maximum temperature of the cutting tool is reduced. If only the chips are cooled as they leave contact with the tool, little good can be accomplished.

A general study of the life of tools of different steels under known loads at various temperatures is much to be desired.

TOPICAL DISCUSSION: IS IT DESIRABLE TO INCLUDE CHEMICAL REQUIREMENTS IN SPECIFICATIONS FOR CAST IRON?

For the past several years there has been a good deal of interest evidenced in certain of the Society's committees and at its annual meetings in the matter of including chemical requirements in specifications for cast iron. This subject was prominent in the discussion two years ago of the Tentative Specifications for Chilled Cast-Iron Wheels proposed by the Society's Committee A-3 on Cast Iron. It has accordingly seemed desirable to the Society's Committee on Papers and Publications to arrange for a topical discussion on the question "Is It Desirable to Include Chemical Requirements in Specifications for Cast Iron?" The committee has invited two members of the Society to open this discussion, Mr. Robert Job presenting the introductory argument for the affirmative, and Mr. Richard Moldenke, the introductory argument for the negative.

ARGUMENT FOR THE AFFIRMATIVE

By Robert Job[1]

In order to obtain good service from iron castings used under widely differing conditions, certain physical properties have been found essential, varying in degree from the hardness and toughness needed in chilled cast-iron car wheels to the soft ductile iron desired in gray-iron machineable castings.

Theoretically, physical tests could be made to determine the relative fitness of an iron casting for any given purpose—tests, for example, which would enable one to measure hardness, toughness, strength, ductility, resistance to shock, etc. Present knowledge as to physical tests is, however, often insufficient to determine definitely the meaning of these tests in terms of actual serviceability of the castings under practical working conditions, and, owing to this lack of knowledge, supplementary chemical tests have, by practical experience, been proved necessary to secure efficient service from the castings.

The testing of cast-iron car wheels is a case in point, for it should be possible by means of correctly regulated physical tests to measure definitely the service value of a wheel. At the present time, unfortunately this cannot be done, and it is well known by every large consumer of chilled cast-iron car wheels that the physical tests which are to-day used in routine inspection of wheels are insufficient to demonstrate the service which may be expected. Wheels may pass all of the usual physical tests, such as drop test, thermal test, etc., and still be far from satisfactory as to quality.

As a result of study of wheels which have failed in service, it has been observed that when the percentage of combined carbon exceeds a certain proportion, undue brittleness is present, and the wheel is liable to fail. Consequently, in addition to making the usual physical tests it is customary to determine the proportion of combined carbon and of certain other constituents. Objection may be made that this same information might be obtained by a simple determination of hardness or other physical tests made under definite conditions, and it is entirely possible that this may be true. In the present state of knowledge we know, however, that excess of combined carbon—say above one per cent or even less—is objectionable, but we do not know just what physical properties in the cast iron produce the same result. Here is an evident need of further study, but for the present we find the chemical determinations necessary.

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[1] Vice-President, Milton Hersey Co., Ltd., Montreal, Canada.

The steam cylinder of a locomotive affords another illustration. This iron is totally different from that in the chilled wheel, for medium hardness is needed, together with toughness, good resistance to shocks and to abrasive action. In the valuation of iron for this purpose, physical tests are extremely useful, but few consumers would have the hardihood to pin their faith merely to the physical tests, and as a result of practical experience it has been found desirable to hold the chemical composition within certain limits.

If we wish to consider still another case, let us take iron castings used for evaporating pans, or for grate bars. Here we need an iron with a relatively high melting point, and resistant to heat. Consequently, in such iron we limit the constituents that would tend to lower the melting point, and make sure that the requisite proportion of heat-resisting substances is present. Physical tests, including fusing point, could be made to give this information, but in this case the chemical analysis is simpler and more definite, making it easier also for the practical foundryman to meet the required properties.

These few instances taken from many which might be given, will serve to point our meaning, and to show that in the present state of knowledge chemical tests cannot be dispensed with in the valuation of iron castings. In making this statement we wish it to be clear that in some cases we consider that ample information is given by simple physical tests, and in such cases chemical tests would be superfluous and a waste of time and money. Consequently, in arranging the tests for any iron casting it is our practice to make the simplest arrangement of physical or of chemical tests—one, or both—which will ensure getting the properties proved by experience to be necessary to meet the conditions of service.

ARGUMENT FOR THE NEGATIVE

By Richard Moldenke[1]

Of recent years, as the consumer of iron castings is becoming familiar with the methods of foundry operation, there has come a marked tendency to include chemical requirements in the specifications for cast-iron products, evidently because of the success of such a procedure in the case of steel. The movement has gone so far that the new tentative specifications for chilled iron car wheels, as first reported, actually demanded the given analysis in full.

Cast iron, in contradistinction to steel, is an extremely non-homogeneous material, highly useful for its fine resistance to compression, ease in machining, cheapness and adaptability to simple melting and molding processes. Yet, while steel may be perfectly homogeneous throughout the piece, the surface and interior of a casting are widely different in their physical properties, as brought about by carbon condition changes due to the rate of heat abstraction during setting. Further, the subsequent heat treatment to which steel is amenable finds almost no counterpart in cast iron.

On the other hand, while it will be shown in what follows that the full chemical composition when specified in purchasing iron castings serves no useful purpose, it is right and proper, and on occasion extremely important, that one or more injurious elements always present in cast iron be limited to given maxima. The experience of the foundryman and the consumers of his product would go for naught if this limitation could not be made part of the specifications governing their transactions.

The first reason why the introduction of chemical requirements into the usual specifications for iron castings is undesirable is that wherever this has been tried in a general way it has failed of its purpose. One prominent engineering society, which went into the subject very thoroughly, found that to include all the compositions of castings coming under each separate line of work for which special specifications were to be prepared, would mean that the limits for the several elements in cast iron would have to be held so wide that no useful purpose would be served. The project was therefore abandoned. Another engineering society, through its specification committee and in conjunction with a similar committee of the foundrymen interested, has just come to the same conclusion, after studying an elaborate series of tests for which chemical data were available to check up with the physical results. They would not fit though showing general parallel tendencies. Perhaps the reason for this, in part, lies in the variations in practice at the several foundries. Procedure in founding iron is cruder and hence more widely fluctuating than in the finer and more exact process of steel making.

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[1] Consulting Metallurgist, Watchung, N. J.

The second reason why chemical compositions at given figures should not be required of foundrymen in specifications they are to work under, is that the history of the development in foundry practice has shown that present-day compositions of castings are considerably different from what was considered necessary some years back. Silicon is a good instance of this. With more knowledge on the bad effects of dissolved iron oxide in cast iron, and definite information on the progression of cupola melting, the same grades of soft iron castings are now produced with much lower silicons than formerly. Much of the trouble formerly attributed to sulfur is now known to have been caused by oxidation phenomena. Even phosphorus has come in for greater attention than formerly, though in the direction of limiting it more closely, for with the better irons that can now be made, the phenomena of segregation have become more acute, as the metal remains fluid longer. The general use of steel scrap in cupola mixtures, for cutting down the total carbon and thus strengthening the iron, has upset many notions on compositions as going with physical properties, and with this new variable introduced into the problem already complicated with many others, it is very nearly hopeless to get down to a series of constants such as could be used to build up a set of compositions that could be called standard.

None the less, the purchaser of castings, in the course of his experience with the product of the foundries he deals with, may develop a line of compositions which he finds gives him good results, and he naturally likes to apply this information for his own benefit. There is no reason whatever why such compositions should not be given in the specifications issued, as "Recommended Practice" but not as an essential requirement. The foundryman who works under such specifications will do his best to comply with them and get the physical properties demanded. But should he find himself unable to do this, he will be free to use his own experience in getting the physical properties required by using compositions of his own, without having his product rejected.

The cases where maxima for certain elements may be specified is somewhat different. Where it is known definitely that when a given maximum percentage of an element is exceeded, failure may result, it is the part of wisdom to apply this limit. The case of sulfur in cast-iron water pipe is an example. High sulfur here may cause breaking when the shock of water-hammer is applied. It is perfectly justifiable, therefore, to state in the specifications that sulfur shall not exceed a given percentage. Phosphorus, similarly, is known to give serious segregation troubles when sections exceed two to three inches in thickness. Hence, a limit is proper where the work will exceed these sections.

The conclusion derived from the above discussion is that it is unwise to add to the troubles of the foundryman by imposing chemical requirements upon him in addition to physical ones. Either one or the other should be imposed, but not both. If the consumer wants a given composition, the foundryman can furnish it, but then the consumer is responsible for the physical characteristics of the work. But if the consumer wants the castings for his service requirements, the foundryman should be unhindered in working out the problem, so long as the service requirement tests are met.

DISCUSSION ON CHEMICAL REQUIREMENTS FOR CAST IRON

Mr. Diller Mr. H. E. Diller[1] (*presented in written form*).—Every possible measure should be taken to insure that the purchaser of castings secures the proper metal. Physical tests, as now established, will not do this in all cases. The usefulness of the transverse and tension tests, as well as hardness tests, of cast iron is limited by the fact that metal poured at different temperatures is likely to have different strengths. Further, as the test bars usually are cast separately from the castings, no infallible method has been devised of insuring that both will be cast at the same temperature. Also, the thickness in section of the casting in a measure will govern the strength of the iron which must be compared to iron cast into a bar 1.25 in. in diameter. Another factor is that metal from one ladle may be of altogether a different composition from that in the previous ladle.

Thus the test bar will be only a general index of what may be expected of the metal in the castings. In some cases, this does not give the consumer sufficient assurance that the castings will meet his requirements. Therefore, he should, in certain cases, have the privilege of demanding that the metal come within certain defined limits of composition. This is particularly true in the cases mentioned by Mr. Job, where a highly specialized product is required. Certainly limits for combined carbon should be included in the specifications where service tests have indicated that beyond these limits danger is encountered.

Let the gray-iron industry make specifications as rigid as possible within the bounds of reason. It will benefit the industry and raise the standard of gray-iron castings. On the other hand, the chemical requirements should be as broad as it is possible to have them and still protect the interests of the purchaser. Thus, limits should not be set unless it is conclusively proved that outside of these limits an undesirable metal is produced. Certainly the wholesale inclusion of limits of composition should be avoided. A great number of cast-iron products require no chemical specifications, while a few such as car wheels demand them.

An illustration of too great zeal in specifying limits for the various elements is shown in the Society's present Specifications for

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[1] Metallurgical Editor, *The Foundry*, Penton Publishing Co., Cleveland, Ohio.

Mr. Diller Gray-Iron Castings (A 48 – 18) which require a sulfur content of less than 0.10 per cent. At the time these specifications were drawn up, this amount of sulfur was considered on the danger line, but more recently, advocates of higher sulfur have appeared and claim higher sulfur to be a distinct advantage to cast iron. It has been pointed out that car wheels that have stood years of service have contained much more than 0.10 per cent of sulfur. Thus, it is being shown that we know little about the effect of definite amounts of sulfur in gray iron. This was acknowledged by the committee which recently drew up tentative British specifications for cast iron. This committee omitted the inclusion of limits for sulfur in gray-iron castings because the limit at which sulfur becomes injurious to castings is still undetermined. On the other hand this committee provided that a limit for phosphorus might be agreed upon between the foundryman and the purchaser of castings. Obviously, this recognizes the effect of phosphorus on the hardness of gray iron and its machining properties.

Owing to the possibility of great variation in composition between the test bar and castings, it might be advantageous to include in the specifications limits of variation between the test bar submitted and the castings supplied. It must be remembered that the purchaser is not buying the test bar, but the casting, and he should have every means afforded for determining that the casting is like the test bar.

This subject is so broad that it gives leeway for wide expressions, when possibly if concrete cases were considered those entering into the discussion would be more in accord than is indicated by what is said in this discussion, In fact, it would be quite possible to subscribe to all the arguments put forth by both of the papers for and against, and still be consistent.

Mr. Wood MR. J. G. WOOD[1] (*presented in written form*).—Sensible specifications must result in greater uniformity in product than castings furnished without. The first mental reaction to furnishing castings under specification would be to have a tendency to cause suppliers to anticipate higher costs per pound, but, on the other hand, the uniformity and consequent value of such castings to the customer, resulting in less loss, etc., undoubtedly will be found to more than compensate for this. The net result is an economic gain.

The foundries and foundrymen have in the past been largely free from such chemical specifications, but such specification is in line with progress, it is certain that evolution in the industry requires closer specification for castings, and it would seem the natural and logical method would be to take up such chemical specification by

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[1] Manager of Works, International Harvester Co., Chicago, Ill.

Mr. Wood groups of typical uses and arrange for the chemical specification to fulfill the necessary physical properties covering a variety of sizes of castings and working conditions.

These specifications can be sensibly defined and qualified by not too broad tolerances, or maximum to meet existing general practice. Chemical specifications should, of course, deal more particularly with phosphorus, sulfur, manganese and silicon and define the carbon content only in a general way.

Because we have in the past failed to arrive at such specification is not a valid reason for us to give up, though with the large number of variables entering into this problem it seems elusive. Erratic results were found in early steel practice, but the variables have now been well cornered and results are eminently satisfactory.

Mr. Putnam MR. W. P. PUTNAM[1] (*presented in written form*).—While I have always felt that chemical specifications are desirable and necessary on some classes of gray-iron castings, I am of the opinion that on some other grades of castings it would work an injustice on the foundryman to impose both chemical and physical requirements. As it has been so well put, if physical requirements only are named it should not make any difference to the purchaser about the chemical composition of the material entering his castings. In other words, the foundryman should be given sufficient leeway to use his own judgment in producing the physical properties required in the castings by his customer. In producing such physical properties, however, it is necessary for the foundryman to know the effect produced by certain chemical constituents of the metal used in his foundry operations.

My experience for a number of years in handling foundry mixtures leads me to feel that we are rapidly approaching the time when it is going to be possible for the purchaser of gray-iron castings to not only specify the physical properties but to also give the maximum and minimum percentages of the different elements permissible without working any hardship upon the foundryman. I feel that this time is rapidly approaching and that definite chemical specifications can be made on certain classes of gray-iron castings that will greatly benefit not only the manufacturer but the purchaser of the castings.

I would incline strongly to the position taken by Mr. Job that chemical specifications should in a number of instances be included in the general specifications on cast iron.

Mr. Elliott MR. G. K. ELLIOTT[2] (*presented in written form*).—Upon reading the introductory arguments by Mr. Job and Mr. Moldenke, it struck

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[1] President, The Detroit Testing Laboratory.

[2] Chief Chemist and Metallurgist, The Lunkenheimer Co., Cincinnati, Ohio.

Mr. Elliott me that each, inferentially or directly, was admitting much that clearly is counter to his side of the question, which presents a condition speaking volumes for the probability of there being much virtue on both sides and suggesting the middle ground as probably the best.

It is certain that the iron foundryman must not be saddled with those unnecessary requirements which at best only lamely tell him what he already and more perfectly knows through experience. The good foundryman often is more familiar with testing and controlling the common, important mechanical properties of cast iron than is the engineer who fatuously imagines he can help him to the desired end by detailed chemical specifications. Broadly speaking in this connection I believe the iron foundryman should be untrammeled, especially in handling carbon and silicon and that his guide should be definite mechanical specifications covering such points as transverse strength, deflection, tensile strength, chill, hardness, etc., all of which are readily measured and controlled in the modern iron foundry. Of course I am mindful that there are exceptions, but generally speaking I favor leaving silicon and carbon to the judgment of the foundryman because with the introduction into the foundries of other furnaces than the cupola, such as the basic and the acid electric furnaces, air furnaces, etc., new conditions are arising which involve in some cases a decided change in the quantitative effect of silicon upon carbon, thereby profoundly altering its influence upon strength, hardness and other mechanical properties of cast iron. Except in rare instances, it seems advisable to leave silicon and carbon to the man who is most familiar with his melting process, his materials and local conditions, giving him merely an accurate description of the mechanical properties desired.

On the other hand there are a number of properties of cast iron that are unknown to the average foundryman, lying entirely beyond his horizon. Many of these center about sulfur and phosphorus. Dynamic tests of cast iron are not yet common enough for the ordinary foundryman to be thoroughly posted concerning the influence of these elements upon the resistance of iron castings to shock and vibration, and therefore in every case where castings are liable to be exposed to dynamically applied stresses sulfur and phosphorus content should be specified by carefully determined maxima. In the manufacture of valves and fittings for the modern high-pressure steam power plant this is especially important, and I give it as merely one example illustrating needed chemical requirements.

In conclusion and speaking only in a very broad sense, I believe that requirements for carbon and silicon should be imposed upon the

Mr. Elliott foundryman as little as possible—definite, complete mechanical specifications being preferable; also that carefully determined maxima often are imperative for sulfur and phosphorus.

Mr. Carson MR. H. Y. CARSON[1] (*presented in written form*).—In reading what both Mr. Moldenke and Mr. Job have said about the topic in question, I cannot see much difference of opinion between their views.

Perhaps, the real question involved is: Whether or not complete chemical requirements for cast iron should be put into specifications. If complete requirements, including the limits for the five elements—sulfur, silicon, manganese, phosphorus and the carbons—were to be included along with definite physical requirements, such a specification becomes confusing to say the least and imposes unfair conditions on the manufacturer.

Some cast-iron articles require closer limits than others, but finished castings may not be entirely judged by what the test bar shows. The test bar indicates the character of the metal entering the mold, but many other conditions of foundry practice may entirely change the conclusion that has been derived solely from the test bar readings. This is a subject that has been so thoroughly discussed in the American Foundrymen's Association that repetition of details would be uninteresting. Yet it is really surprising how many engineers believe they should obtain parallel physical results from finished castings and test bars poured from the same metal.

There is an undoubted need for specifications that will either more fully cover foundry practice or else give information for the benefit of the designing engineer such as will form the necessary factors of safety for wisely using the metal involved. Many of our present specifications can well be criticized on the basis of being too precise in certain details, while at the same time they are not comprehensive enough to cover practical requirements. In other words we have "strained at a gnat while swallowing a camel."

Mr. Force MR. H. J. FORCE.[2]—When it comes to cast-iron wheels, I fully agree with Mr. Job. A car wheel which is made to run over a railroad should be given very careful consideration as to its composition. In 1921 the railroads of the United States paid $106,000,000 in loss and damage claims, and a good part of that was due to broken cast-iron wheels. Last year there were about nine hundred broken cast-iron wheels reported to the Interstate Commerce Commission. Every one of them caused a serious wreck, with a number of lives lost and a number of persons injured. When I lie in the sleeping car at night

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[1] Research Engineer, American Cast Iron Pipe Co., Birmingham, Ala.

[2] Chemist and Engineer of Tests, Delaware, Lackawanna and Western Railroad Co.

Mr. Force

and go speeding along at fifty miles an hour, I often think what kind of wheel is on that freight train going by me on the other side; if it breaks into my train, I will attend no more meetings. That thing has happened, and not long ago an engineer was killed when a car from a foreign road broke into his train. So when we discuss a specification for cast-iron wheels we should give it very careful consideration.

Last week the American Railway Association at Chicago unanimously adopted a complete chemical specification for cast-iron wheels, and a few days prior to that the Association of Manufacturers of Chilled Car Wheels at their meeting in St. Louis unanimously recommended the adoption of that specification, both physically and chemically. The old specification of the Master Car Builders' Association did not call for a chemical composition but merely a physical test, which did not protect the railroads, while the new specification contains a more severe physical test and chemical requirements in addition. Now to discuss a few features of the chemical requirements. Often we are asked "Why specify combined carbon?" I can tell you why I specify it: the carbon in the steel wheel is always in a combined form, and we all agree that 0.85 per cent is the maximum limit; the combined carbon in the cast-iron wheel is in exactly the same form as in the steel wheel, and when it goes above 0.85 per cent it also commences to increase the brittleness of the wheel, so that the combined carbon in the cast-iron wheel should not be above 0.85 per cent. I have arrived at this conclusion not by guess work but by tests. We made up a large number of wheels, placed them in sand and poured around them a band of cast iron 2 in. thick; when the combined carbon is down around 0.65 per cent and other elements are normal, the wheel will show no signs of a crack for ten minutes, but a wheel of 0.90 to 0.95 per cent combined carbon may go two minutes but will probably not get to five, and it may crack in less than a minute. It is important to keep the sulfur low because sulfur has the property of causing high shrinkage. Manganese should be kept within proper limits to neutralize the sulfur. Phosphorus should not be too high because it makes the wheel brittle. The graphitic carbon should, I think, be about 3.0 per cent.

The practice Mr. Moldenke refers to of throwing old scrap steel into cast iron is a bad one. It causes more shrinkage, for the lower the total carbon, the higher the shrinkage. If a wheel is full of shrinkage strains, it will be difficult to anneal it properly.

Mr. Moldenke referred to segregation in cast iron. Cast iron does not segregate any more than steel. You can find three different kinds of composition in a piece of steel rail; but you will find little

Mr. Force difference in composition in cast iron if it is handled properly. Segregation in cast iron is usually due to the use of inferior scrap and iron being melted cold, trying to save coke. Iron of this kind will probably contain blowholes and cold shot and be badly segregated and is unfit to put into a car wheel.

In conclusion, I would say the results we are getting from cast-iron wheels made according to a strict chemical specification which the foundryman can meet and wants to meet are entirely satisfactory.

Mr. Moldenke MR. RICHARD MOLDENKE.—I am afraid Mr. Force has not read what I wrote, and I will read it again: "On the other hand, while it will be shown in what follows that the full chemical composition when specified in purchasing iron castings serves no useful purpose, it is right and proper, and on occasion extremely important, that one or more injurious elements always present in cast iron be limited to given maxima. The experience of the foundryman and the consumers of his product would go for naught if this limitation could not be made part of the specifications governing their transactions." I agree with Mr. Force in this: if you can find any one or several factors that ought to be definite in any particular line of casting, specify them. In general, however, I do not like to see chemical specifications.

Mr. Job MR. ROBERT JOB.—When I read Mr. Moldenke's argument yesterday, I was struck by the fact that while he states that the chemical composition serves no useful purpose, he also states in the same paragraph that it is extremely important that one or more injurious elements always present in cast iron be limited to given maxima. Thus, evidently our two arguments are not far apart in their essentials, since both state that the chemical requirements for cast iron are necessary. Evidently there is no negative side to the question! In the "good old days" pig iron was bought and sold by looking at it, and certain kinds were regarded as medicine for certain other kinds. To-day we have graduated from this and by the aid of analysis we know the reasons for many of the results which in those days were unknown, and it is hardly necessary to state that we have no disposition to throw away the knowledge thus gained. We must, however, in drawing up specifications, be certain that there is a definite reason for each chemical and physical requirement.

In connection with the remarks made in reference to car wheels, I might mention that for over twelve years it has been the standard practice of the Lehigh Valley Railroad to have a definite composition in its cast-iron car wheels. The combined carbon has been limited to 0.75 per cent. It was fixed at that because after tests of a great many broken wheels, it was found that 0.75 per cent appeared to be

Mr. Job

close to the danger limit beyond which it was not desirable to go. At a committee meeting a year ago when the question first came up as to the proportion of combined carbon, I said that it was my opinion that we might take 0.85 per cent as the absolute maximum, and my feeling is that that would be satisfactory, but I should certainly object most strenuously as a result of this experience, to going beyond that figure of 0.85 per cent in combined carbon. I may say further that in all those dozen years in the use of these wheels with the definite composition, we have found not a single one, so far as I know, which failed in service causing an accident. That was in most marked contrast to the practice which we found in other wheels which caused trouble and in which, in many cases, the combined carbon was found as high as 1.50 per cent, with phosphorus sometimes beyond all reason, and sometimes with manganese extremely high.

Mr. Moldenke

MR. MOLDENKE.—I should like to amplify one statement of mine. When I say "specifying the composition serves no useful purpose," I mean the entire composition; and while in my estimation specifying the entire composition serves no useful purpose, some specific items of the composition are very useful to have.

Mr. Onderdonk

MR. J. R. ONDERDONK.[1]—I am sort of between the positions taken by Mr. Job and Mr. Moldenke, having followed up some million or more wheels in service and a good many failures, and having started a chemical composition specification for wheels five or six years ago. We had a limit on manganese, but none on sulfur other than that it should be in a certain relation to the manganese. The physical tests consisted of the ordinary drop test and the thermal test, but a great deal in testing wheels to those specifications depends on how the samples are taken. We have had more serious wrecks with wheels with extremely low chills, I might say, than with almost any other defect in wheels. Coming over the road, wheels with $\frac{1}{8}$ to $\frac{3}{16}$ in. chill would fail before the car got off the road. Nothing has been said about the depth of the chill. If you test wheels and simply pick them out indiscriminately and do not determine the chill, you are liable to get either a very low chill or one that is very high.

A good many years ago we had wheels that apparently met the chemical specifications as suggested by Mr. Force, but the wheels failed. This was not due to the chemical composition, but to bad foundry practice. There is another point that comes up. I do not know how the foundry practice can be controlled unless you put it up to the manufacturer in the form of a guarantee. By having the

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[1] Engineer of Tests, Baltimore and Ohio Railroad.

Mr. Onderdonk guarantee, the foundryman knows he is "up against" something and he is more careful in his foundry practice.

Mr. Force said something about using steel scrap in a wheel. Mr. Moldenke thought it was pretty good if put in right and properly. I agree with Mr. Moldenke. One manufacturer who makes a great many wheels puts steel scrap in them, and although I have hunted for failures of that particular make of wheel, I have been unable to find them on our road, which speaks very well for that particular wheel. A wheel when poured is at a much higher temperature than the ordinary casting, and the steel melting at a higher point can readily be introduced without segregation. In the ordinary casting we have found that in attempting to add steel, the temperature is not high enough and the steel segregates, causing a pretty bad casting.

Mr. Thackray MR. G. E. THACKRAY.[1]—It might be of interest to know that some years ago Mr. S. A. Ford, then the chemist for the Edgar Thomson Steel Works, and the speaker made some careful investigations of cast iron with the idea of discovering the inter-molecular combinations of the various elements. Cast iron has a much larger proportion of metalloids, such as carbon, silicon, sulfur and phosphorus, than most of the other metals with which we operate, and certain of the qualities of cast iron depend not only on one element, or many, but on inter-molecular combinations of various elements. We discovered this by making a solution of cast iron in chlorine gas, dissolving the iron chloride and then investigating the shell or skeleton of metalloids remaining. We found, among other things, that a silicide of carbon had something to do with the chilling properties, but on account of Mr. Ford's death and other changes, the work was never completely carried out. We did, however, change the mixtures used for car wheels and malleable irons and substituted certain coke irons, with good technical results and commercial advantage.

My reason for speaking of that matter at this time is to suggest that we should not look to any one element or group of elements, but should attempt to discover the true inwardness of the various combinations as they may exist in cast iron. In the light of our modern knowledge which Mr. Moldenke doubtless possesses, I should like to ask him how far the foundrymen have gone into that particular question.

Mr. Moldenke MR. MOLDENKE.—I think there are any number of people working on this point, but the data are still not ready to bring together. I tried to bring together the relation of the physical strength with the chemical composition and found that it was hopeless. The same

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[1] Special Engineer, Bethlehem Steel Co., Inc.

Mr. Moldenke

strength was shown by iron with 1.50 per cent silicon as with 2 per cent silicon. Cast iron is steel plus graphite. The steel portion contains ten times as much phosphorus and five or six times as much sulfur as ordinary steel, and yet you expect good results from it. We ought to go very much more into physical tests, and foundrymen ought to go into their foundry practice and try to standardize it. What Mr. Thackray speaks of is the subject of a lot of investigation, and I hope some day we will arrive at some worth-while results.

This very question about the correlation of the elements is receiving a little setback by the introduction of nickel and chrome in cast iron and malleable practice, when we add a slight amount of steel to our mixtures. The slightest amount of chrome seems to ruin the iron for malleable purposes. This is a new point coming in—chrome-nickel added to our scrap heaps is overturning much of what has been considered settled.

Mr. Swan

MR. H. B. SWAN.[1]—I think there is one point that has not been touched on. There is considerable difference between specifications as written by the A.S.T.M. as the result of the work of the chemists and perhaps research work of the Society, and the ordinary specifications as drawn up by an engineer for castings for some particular purpose. I do not think it does any harm to include chemical specifications which have been shown to be of value through service and research along with the physical specifications; but at the same time when we specify sulfur and phosphorus, manganese and silicon, we have only done part of the job. We are just beginning to find out that other elements such as oxygen, nitrogen, and oxides of iron, etc., have important effects. And then another point: we are not always so interested in how much carbon may be present as in the microstructure of the carbon. There is a difference in the condition of the graphite which makes a great deal of difference in physical properties. It was my privilege some years ago to do a little work for the late J. E. Johnson on the effects of oxygen on iron. He did some very remarkable work; he was able to change the structure of coke iron to almost the same as charcoal iron and to get strengths and physical properties that paralleled charcoal iron; so I think that when we start to put in chemical specifications, unless we are in a position to really cover the whole job in the right manner, we might better leave them out. Even though they be up to specifications, castings may be absolutely spoiled by poor foundry practice in the regulation of melting.

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[1] Cadillac Motor Car Company, Detroit, Mich.

ENDURANCE PROPERTIES OF STEEL: THEIR RELATION TO OTHER PHYSICAL PROPERTIES AND TO CHEMICAL COMPOSITION[1]

BY D. J. MCADAM, JR.[2]

SCOPE OF WORK

This paper gives results of an investigation of endurance properties of steel at the U. S. Naval Engineering Experiment Station. The investigation here described is a continuation of the previously described[3] investigation in which carbon and alloy steels of various compositions were tested. In continuing this work, a great variety of carbon and alloy steels of various compositions, heat-treated in various ways, have been utilized. Static tension and torsion and impact tests were made on each kind of material. The tests of specimens of the rotating cantilever type as previously described have been continued. Alternating torsion tests and impact-endurance tests have also been made.

TESTING APPARATUS

Rotating Cantilever Testing Machines.—The machine for testing specimens of the rotating cantilever type has been previously described.[4] Additional units of this type of machine have been built. The speed of the newer units is about 1800 r.p.m.

Alternating Torsion Testing Machines.—With the same material a series of tests has been made, using the alternating torsion testing machines developed at this station.[5, 6, 7] In the high-speed machine the inertia of a fly wheel causes alternating stresses in the specimen. This machine has been improved so as to run at a speed of over 2100 r.p.m. An additional machine has also been constructed. The low-speed machine, in which the stress is applied and measured by springs, has also been used in order to compare the results obtained on this machine with the results obtained with the machines of the inertia type.

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[1] This paper is presented by permission of the Secretary of the Navy.

[2] Metallurgist, U. S. Naval Engineering Experiment Station, Annapolis, Md.

[3] D. J. McAdam, Jr. Discussion of Paper, by Moore, Kommers and Jasper on "Fatigue of Metals," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 288 (1922).

[4] D. J. McAdam, Jr., "Endurance of Steel under Repeated Stresses," *Chemical and Metallurgical Engineering*, December 14, 1921.

[5] D. J. McAdam, Jr., "Endurance and Impact Tests of Metals," *Proceedings*, Am. Soc. Testing Mats., Vol. XVI, Part II, p. 292 (1916).

[6] D. J. McAdam, Jr., "An Alternating Torsion Testing Machine," *Proceedings*, Am. Soc. Testing Mats., Vol. XVII, Part II, p. 599 (1917).

[7] D. J. McAdam, Jr., "A High-Speed Alternating Torsion Testing Machine," *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part II, p. 366 (1920).

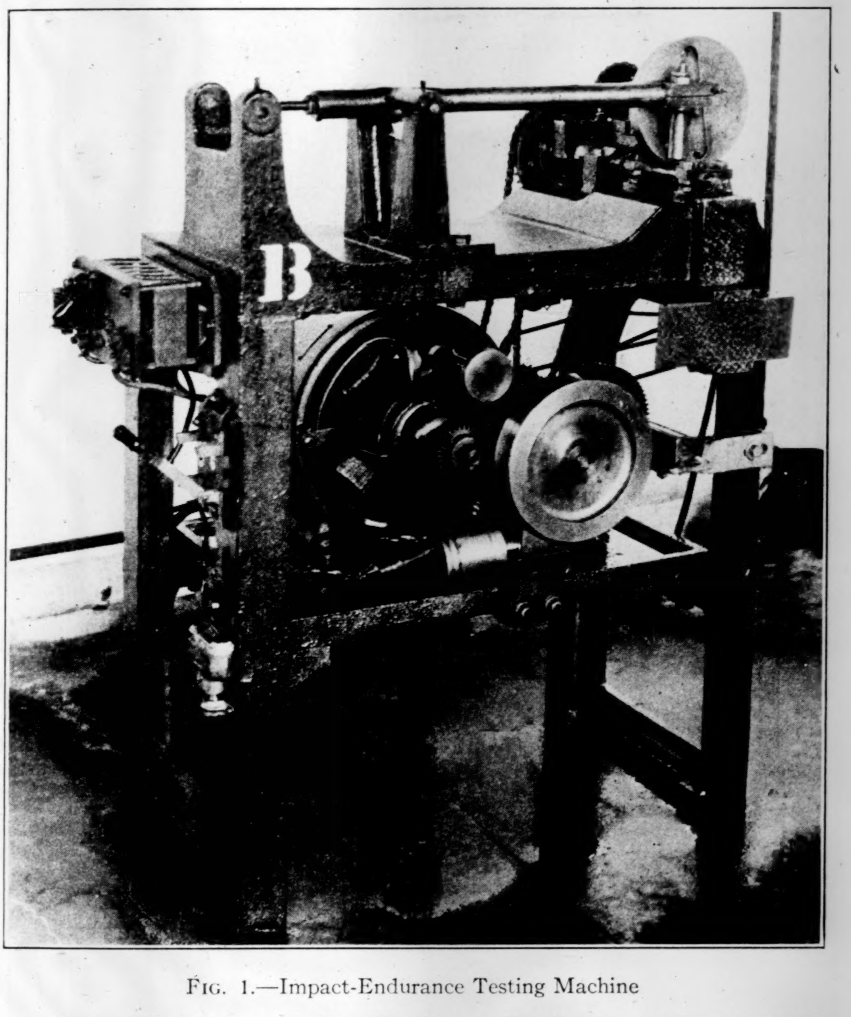

Fig. 1.—Impact-Endurance Testing Machine

Impact-Endurance Testing Machine.—For impact-endurance testing, a machine was developed several years ago at the Naval Engineering Experiment Station. The essential construction of the machine has been previously described.[1] Since then, however, the machine has been somewhat improved.

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[1] D. J. McAdam, Jr., "An Impact-Endurance Testing Machine," *Journal*, Am. Soc. Naval Eng., November 1917.

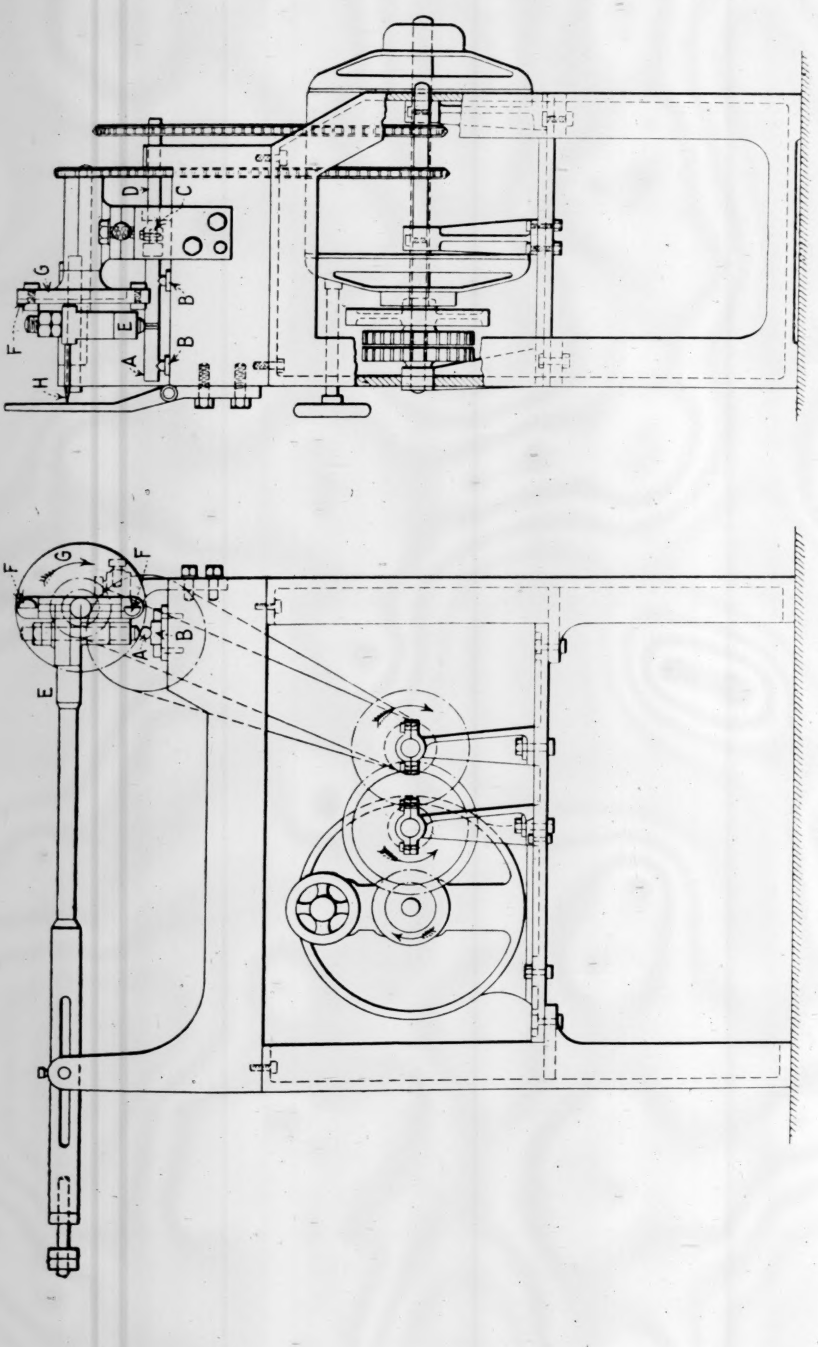
H
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FIG. 2.—Diagrammatic Sketch of Impact-Endurance Testing Machine

Fig. 1 is a photograph of the machine. Fig. 2 is a sketch illustrating its construction and principles. As shown in Fig. 2, the cylindrical notched specimen *A* rests on the supports *B*. It is attached by a flexible coupling to a mechanism *C* which in turn is attached to a driving shaft *D*. As a result of this mechanism; the specimen has alternate periods of rest and sudden rotation through 90 deg. The hammer *E* is alternately lifted and released by the cams *F* attached to the rotating disk *G*. The action of the cams is so timed that the hammer strikes the specimen once during each of its above mentioned periods of rest. The specimen is thus subjected, during each revolution, to four hammer blows struck just over the notch.

Table I.—Chemical Composition of Bar Material (Rounds)
All Values in Per Cent

Material Designation	C	Mn	P	S	Si	Ni	Cr	V	Mo
N	0.023	0.037	0.002	0.031	0.005	.....	.....	.....	.....
O, OZ	0.24	0.45	0.009	0.051	0.007	.....	.....	.....	.....
T	0.38	0.57	0.033	0.048	0.04	.....	.....	.....	.....
S	0.81	0.32	0.012	0.031	0.16	.....	.....	.....	.....
R	0.97	0.31	0.019	0.020	0.13	0.18	.....	.....	.....
P	0.42	0.70	0.017	0.016	0.12	3.60	.....	.....	.....
Q	0.31	0.64	0.026	0.028	0.13	3.35	.....	.....	.....
Xa	0.39	0.49	0.034	0.035	0.195	.....	0.76	.....	0.18
Xb	0.31	0.44	0.034	0.035	0.22	.....	0.85	.....	0.20
Xc	0.50	0.48	0.030	0.039	0.24	.....	1.03	.....	0.19
Y	0.55	.....	.....	.....	.....	.....	0.99	0.19	.....
Z	0.49	.....	.....	.....	.....	1.75	0.99	.....	.....

The hammer is so constructed that its center of percussion is immediately above the specimen. The weight of the hammer is adjustable. The cams are adjustable so that the height of fall of the hammer can be varied.

In order to prevent irregular rebound of the hammer from the cams and consequent irregular height of fall, it is essential that the speed of revolution of the disk *G* be so adjusted that the time of a half revolution is equal to the natural period of fall of the hammer. In thus adjusting the speed, a diagram of the path of fall of the hammer can be obtained by means of a lightly held pencil *H* attached to the hammer.

As the height of fall decreases, the frequency of hammer blows increases. In the tests at the Naval Engineering Experiment Station, the frequency varies from about 90 to 600 blows per minute. By varying the energy of hammer blow, as described above, a series of tests can be made on each material and diagrams can be made in which the energy of blow is plotted against the number of blows necessary to cause failure.

MATERIAL

Nearly all the material for this investigation was received in the form of round bars $\frac{7}{8}$ to $1\frac{1}{4}$ in. in diameter. The chemical composition and designation of each kind of bar material are given in Table I. Shafts of battleships, submarines and aeroplanes, were also investigated. A description of these shafts and their chemical compositions are given in Table II.

TABLE II.—DESIGNATION AND CHEMICAL COMPOSITION OF CRANK-SHAFT AND PROPELLER-SHAFT MATERIAL

Shaft Designation	Type of Vessel	Type of Shaft	Diameter of Journal, in.	Diameter of Bore, in.	Chemical Composition, per cent							
					C	Mn	P	S	Si	Ni	Cr	V
ZA	Aeroplane	Crank	....	....	0.46	0.82	0.015	0.020	....	1.41	0.81	....
1	Submarine	Crank	9.45	4.31	0.45	0.56	0.016	0.030	0.13	0.20	1.00	0.16
2	Battleship	Propeller	O. D. $16\frac{3}{8}$	$9\frac{3}{8}$	0.39	0.61	0.024	0.019	0.14	3.06	....	....
3	Battleship	Propeller	O. D. $16\frac{3}{8}$	$9\frac{3}{8}$	0.34	0.65	0.032	0.027	0.15	3.37	....	....
4	Submarine	Crank	8	3	0.25	1.18	0.031	0.04	0.25	....	....	....
6	Submarine	Crank	8	3	0.38	0.59	0.020	0.017	0.16	....	....	....
9	Submarine	Crank	8	4.3	0.41	0.54	0.017	0.024	0.14	....	....	....

Note.—Shafts ZA, 1, 4 and 6 had never seen service. Shafts 2, 3 and 9 are failed shafts.

HEAT TREATMENT

In Table III are stated the heat treatments to which the bar material was subjected. For example, material Q-8 as recorded in Table III, has the composition Q as recorded in Table I, and has received the heat treatment designated in Table III.

The bars were turned down, if necessary, to a diameter of 1 in. and cut to the required length before treatment. An electric muffle furnace was used and the specimens were supported so that there was an air space between them and the bottom of the furnace. The temperature on the outside of the set of bars being treated was found to differ by not more than 5° F. from that in the midst of the set.

STATIC AND IMPACT TEST RESULTS

Tension Test Results.—Table IV gives the averages of tension test results on bar material. For convenience, the results from material of

TABLE III.—HEAT TREATMENT OF BAR MATERIAL

Material Designation	Heat Treatment					
	Heated to — deg. Fahr.	Held	Cooled in	Reheated to — deg. Fahr.	Held	Cooled in
N-8	1650	1 hr.	Water			
O-2	1600	½ hr.	Oil	900	½ hr.	Furnace
O-3	"	"	"	1100	"	"
O-4	"	"	"	1300	"	"
O-5	"	"	Furnace			
O-6	"	"	Air			
O-7	"	"	Oil	700	½ hr.	Furnace
O-8	1650	1 hr.	Water	1250	1 hr.	"
O-9	"	"	"	1000	"	"
O-10	"	"	"	800	"	"
P-2	1450	½ hr.	Furnace			
P-2₂	"	1 hr.	"			
P-3	"	½ hr.	Oil	1150	½ hr.	Furnace
P-5	"	"	"	900	"	"
P-5₂	"	1 hr.	"	900	1 hr.	"
P-6	"	"	Water	1100	"	"
P-7	"	"	"	1000	"	"
P-8	"	"	"	900	"	"
Q-3	1475	½ hr.	Oil	1200	½ hr.	Furnace
Q-5	"	1 hr.	Furnace			
Q-5₁	"	½ hr.	"			
Q-6	"	"	Oil	1150	"	"
O-7	"	"	"	1050	"	"
Q-8	"	"	"	950	"	"
Q-10	"	"	Air			
Q-11	1650	2 hr.	Oil	1475	"	"
Q-12	1475	1 hr.	"	900	"	"
Q-13	"	"	"	1100	1 hr.	"
Q-14	"	"	Water	"	"	"
Q-15	"	"	"	900	"	"
Q-16	"	"	"	1200	"	"
Q-17	1550	"	"	900	"	"
R-1	1450	1 hr.	Furnace			
S-1	1450	½ hr.	Furnace			
S-2	1475	2 hr.	Air			
T-1	1550	½ hr.	Furnace			
T-1₂	"	1 hr.	"			
T-2	"	½ hr.	Air			
T-3	1700	2 hr.	Oil	1550	½ hr.	Furnace
T-4	1550	½ hr.	"	1250	"	"
T-5	"	"	"	1000	"	"
T-6	"	"	"	800	"	"
T-7	1800	5 hr.	"	1550	1 hr.	"
T-8	1550	½ hr.	Water	800	½ hr.	"
T-8₂	"	1 hr.	"	"	1 hr.	"
T-9	"	"	"	1250	"	"
T-10	"	"	"	1000	"	"
T-11	"	"	"	1100	"	"

TABLE III.—HEAT TREATMENT OF BAR MATERIAL—*Continued*

Material Designation	Heat Treatment					
	Heated to —deg. Fahr.	Held	Cooled in	Reheated to —deg. Fahr.	Held	Cooled in
Xa-1	1600	1 hr.	Furnace			
Xa-2	"	"	Water	900	1 hr	Furnace
Xa-3	"	"	"	1100	"	"
Xb-1	1600	1 hr.	Furnace			
Xb-2	"	"	Water	900	1 hr.	Furnace
Xb-3	"	"	"	1100	"	"
Xc-1	1600	1 hr.	Furnace			
Xc-2	"	"	Water	900	1 hr.	Furnace
Xc-3	"	"	"	1100	"	"
Y-1	1650	1 hr.	Furnace			
Y-2	"	"	Water	900	1 hr.	Furnace
Y-3	"	"	"	1100	"	"
Z-1	1550	1 hr.	Furnace			
Z-2	"	"	Water	900		
Z-3	"	"	"	1100		

the same composition are grouped together and arranged in definite order. For each material the sequence is as follows: Water quenched, arranged in order from lowest to highest temperature of draw; oil quenched, arranged in order from lowest to highest temperature of draw; and air cooled, furnace cooled or annealed. The same sequence is used in the graphs of endurance results to be described later.

Table V gives the averages of longitudinal tension test results for the shafting described in Table II.

Torsion Test Results.—Tables VI and VII give the torsion test results for bar material and shafts, respectively. The sequence of torsion results for each kind of material is the same as the sequence of tension test results.

Charpy Impact Test Results.—The Charpy test results for bar material are given in Table VIII; the results for shafts are given in Table IX. The sequence of Charpy results for each kind of material is the same as the sequence of tension and torsion test results.

ROTATING CANTILEVER ENDURANCE TESTS

Types of Specimen.—The specimen used in most of the rotating cantilever endurance tests is essentially the same as that described in a previous publication.[1] This specimen, designated Type C in Fig. 3,

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[1] D. J. McAdam, Jr., "Endurance of Steel under Repeated Stresses," *Chemical and Metallurgical Engineering*, December 14, 1921.

TABLE IV.—AVERAGE RESULTS OF STATIC TENSION TESTS ON BAR MATERIAL

Each Value is Average of Two Specimens, except as Noted.

Material Designation	Cooled in	Drawn at — deg. Fahr.	Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Proportional Limit lb. per sq. in.	Elongation in 2 in., per cent	Reduction of Area, per cent
N-8	Water	....	46 700	29 700	28 700	38.0	71.0
O-10	Water	800	78 820	46 110	44 360	21.5	66.5
O-9	"	1000	79 160	48 750	47 000	28.5	67.4
O-8	"	1250	67 100	45 610	44 610	37.5	71.1
O-7	Oil	700	68 500	50 000	48 500	37.0	68.7
O-2	"	900	67 500	48 500	47 500	35.6	68.2
O-3	"	1100	66 000	42 250	39 250	38.5	69.2
O-4	"	1300	60 300	40 250	39 120	41.0	72.5
O-1	As Received		60 400	38 200	37 600	39.0	63.7
O-6[a]	Air	....	60 750	42 000	40 000	39.0	63.0
O-5	Furnace	....	55 120	33 620	31 250	38.7	60.6
T-8$_2$	Water	800	96 620	63 500	63 000	24.2	56.4
T-8	"	800	85 250	58 000	58 000	27.2	58.7
T-10[a]	"	1000	97 370	66 000	65 000	26.0	61.1
T-11	"	1100	86 250	55 000	54 500	26.5	61.5
T-9	"	1250	82 000	58 500	58 000	29.7	65.7
T-6[a]	Oil	800	91 500	60 000	60 000	29.0	58.0
T-5[a]	"	1000	85 250	55 000	55 000	31.0	60.5
T-4	"	1250	84 010	55 250	55 000	30.5	64.8
T-2	Air	....	77 490	49 500	48 500	32.7	60.2
T-1[a]	Furnace	....	70 000	39 500	38 500	32.0	50.0
T-1$_2$	"	....	69 370	38 000	37 500	33.2	51.3
T-3[a]	"	....	72 000	40 000	39 000	30.5	47.0
T-7	"	....	70 000	33 250	32 750	31.2	48.5
S-2$_2$	Air	....	119 300	41 000	40 000	15.0	23.7
S-2	"	....	110 000	41 000	40 000	15.0	23.7
S-1	Furnace	....	89 750	34 000	32 250	22.5	34.4
R-1$_2$	Furnace	....	91 890	41 400	40 900	20.7	45.3
R-1	"	....	87 500	42 500	42 500	25.5	37.5
P-8[a]	Water	900	154 200	142 000	139 000	15.0	55.0
P-7	"	1000	149 250	139 150	137 500	18.0	55.0
P-6	"	1100	132 250	116 000	115 500	18.7	54.7
P-5$_2$	Oil	900	118 000	92 500	90 500	23.2	50.7
P-3	"	1150	123 000	61 750	60 500	21.7	55.1
P-2$_2$	Furnace	....	113 630	70 330	69 510	22.0	43.5
Q-17	Water	900	145 750	128 500	127 000	17.2	58.7
Q-15$_2$	"	900	145 880	130 500	129 000	18.0	60.5
Q-17$_2$	"	900	138 750	126 000	126 000	18.7	64.0
Q-15[b]	"	900	142 900	126 300	124 000	18.3	61.2
Q-14	"	1100	115 900	99 000	96 750	24.0	64.5
Q-16$_3$[d]	"	1200	112 000	96 100	95 500	25.6	68.7
Q-16[b]	"	1200	99 100	83 800	83 000	26.6	68.0
Q-16$_2$[c]	"	1200	107 500	90 100	92 375	25.1	65.2
Q-12[b]	Oil	900	117 500	83 700	82 700	22.0	60.5
Q-8	"	950	154 000	130 000	125 500	16.5	48.5
Q-7	"	1050	158 500	134 500	124 500	16.0	42.0
Q-13[b]	"	1100	97 600	70 000	69 000	27.5	68.5
Q-6[b]	"	1150	115 500	89 500	85 500	22.5	60.0
Q-3	"	1200	97 500	80 000	79 000	26.0	67.5
Q-10	Air	....	91 050	50 000	49 500	29.5	55.0
Q-11	Furnace	....	85 500	57 500	57 500	30.3	56.3
Q-5$_3$[d]	"	....	90 800	58 100	56 800	30.0	60.0
Q-5$_2$[c]	"	....	80 600	51 250	50 250	31.0	58.0
Q-5$_1$	"	....	86 450	53 550	51 550	34.5	59.0
Q-5	"	....	103 900	53 500	50 500	22.5	44.5
Xa-2	Water	900	121 570	86 220	85 220	12.7	55.3
Xa-3	"	1100	136 630	108 500	107 500	17.3	61.3
Xa-1	Furnace	....	85 750	48 250	47 250	29.0	49.3
Xb-2	Water	900	164 750	124 250	122 500	13.3	52.3
Xb-3	"	1100	139 880	116 500	115 500	16.0	62.3
Xb-1	Furnace	....	82 430	43 130	42 100	27.5	51.7
Xc-2	Water	900	172 000	128 500	127 500	12.3	41.7
Xc-3	"	1100	157 000	129 000	128 000	16.3	55.5
Xc-1	Furnace	....	111 350	50 850	49 900	21.0	41.0
Y-2	Water	900	201 000	165 000	164 400	13.3	53.5
Y-2$_2$	"	900	147 740	105 770	104 770	14.8	42.8
Y-3	"	1100	164 400	130 500	129 500	15.5	50.5
Y-1	Furnace	....	101 750	51 500	50 500	25.5	48.5
Z-2	Water	900	164 500	128 000	127 500	14.5	52.5
Z-3	"	1100	144 250	116 000	115 000	18.0	60.0
Z-1	Furnace	....	117 750	56 500	55 500	20.0	44.5

[a] Values reported are for one specimen.

[b] Values reported are average of 3 specimens.

[c] Values reported are average of 4 specimens.

[d] Values reported are average of 5 specimens.

is so designed that when loaded at the outer end the stress is nearly uniform over about $1\frac{1}{2}$ in. of the specimen's length. In order that the stress throughout the length of a cantilever of circular cross-section may be entirely uniform, the longitudinal axial section must have the form of a cubic parabola. In the specimen actually used the outer surface is conical and of such taper that it would be tangent to an enclosed cubic paraboloid of revolution in the plane M, $\frac{3}{4}$ in. from the outer end of the fillet as shown in Fig. 3. This is the plane of maximum stress. For about $\frac{3}{4}$ in. on each side of this plane, however, the conical surface differs so slightly from that of an enclosed cubic paraboloid of revolution that the stress does not vary more than about $1\frac{1}{2}$ per cent from uniformity.

TABLE V.—AVERAGE RESULTS OF STATIC TENSION TESTS ON CRANK-SHAFT AND PROPELLER-SHAFT MATERIAL

Each Value is Average of Two Specimens, except as Noted.

Shaft Designation	Specimen		Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Proportional Limit, lb. per sq. in.	Elonga- in 2 in., per cent	Reduction of Area, per cent
	Location[1]	Direction					
ZA[a]		Longitudinal	140 000	120 560	....	19.9	58.4
1	Prolongation	Longitudinal	103 380	68 500	65 500	23.8	57.8
		Tangential	99 000	69 000	65 500	15.0	25.0
	Block	Longitudinal[a]	114 410	89 000	87 750	23.5	60.0
		Breadth	109 680	58 800	52 790	15.0	31.9
		Thickness	110 800	88 950	88 950	17.8	39.0
2		Longitudinal	95 000	48 000	46 500	26.2	49.5
		Tangential	65 870	48 500	47 500	3.0	5.7
		Radial	93 000	50 000	49 000	18.5	30.5
3		Longitudinal	84 870	43 000	43 000	29.5	55.8
		Tangential	85 400	45 090	42 950	26.2	34.9
		Radial	81 810	42 000	38 500	19.7	30.0
4	Prolongation	Longitudinal	75 940	43 860	43 860	36.2	66.7
		Tangential	74 620	39 900	39 900	31.0	51.6
		Radial	72 620	39 000	39 000	27.0	43.5
	Block	Longitudinal	74 630	40 310	39 310	35.0	66.0
		Breadth	74 440	40 600	39 600	35.5	60.9
		Thickness	75 750	41 500	40 000	30.5	49.1
6	Prolongation	Longitudinal	77 310	38 000	37 000	28.0	53.1
		Tangential	77 120	35 500	35 500	26.7	50.0
		Radial	77 180	35 910	35 910	27.0	45.3
	Block	Longitudinal	83 950	43 100	40 600	29.7	56.0
		Breadth	82 500	39 000	38 000	23.0	34.8
		Thickness	76 580	36 270	35 270	26.0	43.1
9[b]	Web	Longitudinal	79 810	48 220	46 970	30.8	58.7
		Breadth	79 770	48 060	46 940	27.8	49.8
		Thickness	76 660	45 240	43 990	20.3	33.1

"Prolongation" designates material from the test end prolongation of the shaft. "Block" designates material from the crank-throw test block, about midway of the shaft.

[a] Values reported are average of 4 specimens.

[b] Values reported are average of 8 specimens.

After a Type C specimen is broken, the unstressed end can be machined and used for either a tension test or an alternating torsion test. It is believed, therefore, that this cantilever specimen serves

TABLE VI.—RESULTS OF STATIC TORSION TESTS ON BAR MATERIAL

Individual Determinations for Each Material.

Material Designation	Cooled in	Drawn at — deg. Fahr.	Nominal Torsional Strength, lb. per sq. in.	Proportional Limit, lb. per sq. in.	Angle of Twist per Linear Inch at Proportional Limit, min.	Angle of Twist per Linear Inch at Break, deg.
N-8	Water	....	40 700	14 400	12	998
O-10	Water	800	60 200	38 600	32	237
O-9	"	1000	59 300	36 200	30	336
O-8	"	1250	54 300	24 200	22	433
O-7	Oil	700	56 600	25 360	22	323
O-2	"	900	60 500	30 190	25	433
O-3	"	1100	53 440	22 940	20	327
O-4	"	1300	52 300	22 940	23	383
O-1	As Received		53 435	25 355	19	260
O-6	Air	....	48 730	24 150	50	260
O-5	Furnace	....	46 640	14 490	12	360
T-8_2	Water	800	67 480	36 510	35	221
T-8	"	800	63 390	33 810	27	256
T-10	"	1000	60 450	33 810	38	237
T-11	"	1100	64 300	33 810	28	217
T-9	"	1250	62 490	33 810	28	293
T-6	Oil	800	57 730	21 730	17	161
T-5	"	1000	65 210	31 390	27	235
T-4	"	1250	56 150	28 980	22	168
T-2	Air	....	67 020	36 220	29	210
T-1	Furnace	....	60 000	33 810	25	307
T-1_2	"	....	54 300	21 730	18	180
T-3	"	....	54 790	24 150	19	160
T-7	"	....	52 980	19 320	16	168
S-2_2	Air	....	74 270	26 560	18	78
S-2	"	....	72 000	26 560	22	88
S-1	Furnace	....	70 645	20 530	20	147
R-1	Furnace	....	69 900	31 400	29	236
P-8	Water	900	91 370	72 140	59	170
P-7	"	1000	93 300	86 900	72	145
P-6	"	1100	86 940	79 690	67	153
P-5_2	Oil	900	77 000	53 000	66	163
P-3	"	1150	81 060	60 370	55	197
P-2_2	Furnace	....	77 890	41 400	35	182
Q-17	Water	900	99 000	91 000	69	135
Q-17_2	"	900	88 000	79 695	62	247
Q-15	"	900	88 750	84 500	72	220
Q-14	"	1100	77 890	62 790	52	240
Q-16	"	1200	69 700	50 700	43	264
Q-12	Oil	900	77 000	58 000	48	255
Q-8	"	950	96 000	90 560	82	78
Q-7	"	1050	111 400	89 350	85	151
Q-13	"	1100	71 400	48 300	40	251
Q-6	"	1150	77 890	54 330	50	170
Q-3	"	1200	67 920	45 880	37	360
Q-10	Air	....	67 300	33 800	30	202
Q-11	Furnace	....	60 000	33 800	28	172
Q-5_2	"	....	61 500	33 800	28	227
Q-5_1	"	....	62 000	31 400	27	212
Xa-2	Water	900	101 890	99 015	82	4
Xa-3	"	1100	73 053	55 545	46	140
Xa-1	Furnace	....	65 190	31 830	26	236
Xb-2	Water	900	104 150	94 180	77	107
Xb-3	"	1100	86 040	72 450	60	115
Xb-1	Furnace	....	62 950	26 560	19	110
Xc-2	Water	900	125 510	111 960	81	83
Xc-3	"	1100	103 370	81 950	66	103
Xc-1	Furnace	....	76 700	36 960	30	172
Y-2	Water	900	124 080	103 840	80	28
Y-3	"	1100	110 490	106 260	87	97
Y-1	Furnace	....	76 980	33 810	28	128
Z-2	Water	900	95 287	79 695	67	183
Z-3	"	1100	82 420	55 540	46	167
Z-1	Furnace	....	78 498	33 810	28	220

TABLE VII.—RESULTS OF STATIC TORSION TESTS ON CRANK-SHAFT AND PROPELLER-SHAFT MATERIAL

Individual Determinations for Each Material.

Shaft Designation	Specimen: Location[1]	Specimen: Direction	Nominal Torsional Strength, lb. per sq. in.	Proportional Limit, lb. per sq. in.	Angle of Twist per Linear Inch at Proportional Limit, min., deg.	Angle of Twist per Linear Inch at Break, deg.
2		Longitudinal	75 200	36 200	25	**218**
		Tangential	67 020	38 640	30	**162**
3		Longitudinal	62 950	21 740	24	**213**
		Tangential	61 680	24 150	17	**117**
4	Prolongation	Longitudinal	61 360	28 980	22	**343**
6	Prolongation	Longitudinal	54 340	21 740	18	**266**
	Block	Thickness	62 950	24 150	17	**197**
9	Web	Breadth	56 150	21 740	18	**220**
		Thickness	60 680	26 550	22	**228**

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[1] "Prolongation" designates material from the test end prolongation of the shaft. "Block" designates material from the crank-throw test block, about midway of the shaft.

TABLE VIII.—AVERAGE RESULTS OF CHARPY IMPACT TESTS ON BAR MATERIAL

Each Value is Average of Two Specimens except as Noted.

Material Designation	Cooled in	Drawn at —deg. Fahr.	Charpy Test, ft-lb.
N-8	Water	....	44.6
O-10	Water	800	37.0
O-9	"	1000	38.3
O-8[a]	"	1250	47.7
O-7	Oil	700	48.0
O-2	"	900	50.9
O-3[a]	"	1100	56.8
O-4	"	1300	59.2
O-1	As Received		23.6
O-6	Air	....	38.8
O-5[b]	Furnace	....	13.5
T-8$_2$	Water	800	21.5
T-8[b]	"	800	31.7
T-10	"	1000	24.7
T-11	"	1100	24.1
T-9	"	1250	30.8
T-6	Oil	800	27.8
T-5	"	1000	25.2
T-4	"	1250	13.6
T-2	Air	....	20.9
T-1	Furnace	....	18.8
T-1$_2$	"	....	6.7
T-3	"	....	8.4
T-7[b]	"	....	14.1
S-2$_2$	Air	....	4.2
S-2	"	....	4.8
S-1	Furnace	....	2.9
R-1$_2$[c]	Furnace	....	....
R-1	"	....	5.6
P-8	Water	900	17.6
P-7	"	1000	26.1
P-6	"	1100	25.8
P-5$_2$	Oil	900	29.8
P-3	"	1150	32.3
P-2$_1$	Furnace	....	17.9
Q-15$_2$	Water	900	34.9
Q-15	"	900	29.8
Q-14	"	1100	37.5
Q-16$_3$	"	1200	43.9
Q-16	"	1200	47.6
Q-16$_2$	"	1200	41.3
Q-12	Oil	900	35.5
Q-8	"	950	20.5
Q-7	"	1050	15.0
Q-13	"	1100	48.4
Q-6	"	1150	34.5
Q-3	"	1200	54.2
Q-10	Air	....	33.6
Q-11	Furnace	....	22.1
Q-5$_3$	"	....	34.4
Q-5$_2$	"	....	36.4
Q-5$_1$[b]	"	....	27.9
Q-5	"	....	12.0
Xa-2	Water	900	11.4
Xa-3	"	1100	10.5
Xa-1	Furnace	....	8.1
Xb-2	Water	900	18.0
Xb-3	"	1100	12.5
Xb-1	Furnace	....	12.5
Xc-2	Water	900	12.2
Xc-3	"	1100	20.7
Xc-1	Furnace	....	4.8
Y-2	Water	900	9.8
Y-3	"	1100	14.2
Y-1	Furnace	....	16.0
Z-2	Water	900	16.5
Z-3	"	1100	29.4
Z-1	Furnace	....	16.7

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[a] Average of 3 specimens.

[b] Average of 4 specimens.

[c] Value to be supplied.

every purpose of an endurance test as well as the beam type of specimen and has the advantage of being shorter and of larger test diameter for the same weight of load.

Specimens *M* and *N* shown in Fig. 3 were used in some special experiments to determine the comparative effects of abrupt changes of section in material of various physical properties.

Table IX.—Average Results of Charpy Impact Tests on Crank-Shaft and Propeller-Shaft Material

Each Value is Average of Two Specimens, except as Noted.

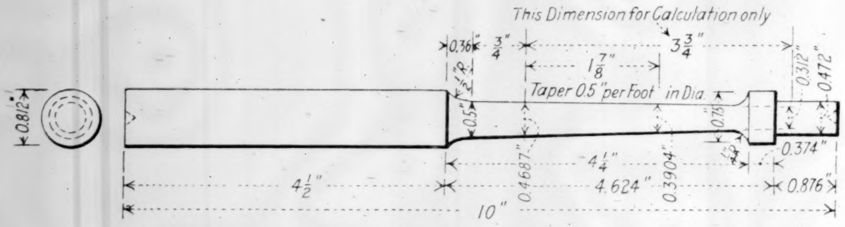
Shaft Designation	Specimen		Direction of Impact	Charpy Test, ft-lb.
	Location[1]	Direction		
1	Prolongation	Longitudinal	Tangential	25.3
			Radial	36.9
		Tangential	Longitudinal	13.1
			Radial	13.9
		Radial	Longitudinal	15.0
			Tangential	12.5
	Block	Longitudinal	Breadth	27.9
			Thickness	25.0
		Breadth	Longitudinal	10.6
			Thickness	10.8
		Thickness	Longitudinal	18.7
			Breadth	18.3
2		Longitudinal	Tangential	11.1
			Radial	12.0
		Tangential	Longitudinal	13.6
			Radial	13.2
		Radial	Longitudinal	15.0
			Tangential	15.7
3		Longitudinal	Tangential	21.6
			Radial	21.5
		Tangential	Longitudinal	16.8
			Radial	13.9
		Radial	Longitudinal	13.4
			Tangential	14.4
4	Prolongation	Longitudinal	Tangential	44.6
			Radial	41.4
		Tangential	Longitudinal	19.4
			Radial	22.8
		Radial	Longitudinal	22.7
			Tangential	27.1
	Block	Longitudinal	Breadth	36.4
			Thickness	34.8
		Breadth	Longitudinal	28.7
			Thickness	29.4
		Thickness	Longitudinal	21.3
			Breadth	22.6
6	Prolongation	Longitudinal	Tangential	9.2
			Radial	9.3
		Tangential	Longitudinal	10.3
			Radial	21.3
		Radial	Longitudinal	9.3
			Tangential	12.8
	Block	Longitudinal	Breadth	8.4
			Thickness	7.3
		Breadth	Longitudinal	5.8
			Thickness	7.3
		Thickness	Longitudinal	6.6
			Breadth	7.5
9[a]	Web	Longitudinal	Breadth	24.4
			Thickness	24.3
		Breadth	Longitudinal	18.6
			Thickness	18.0
		Thickness	Longitudinal	15.6
			Breadth	15.0

[a] Values reported are average of 4 specimens.

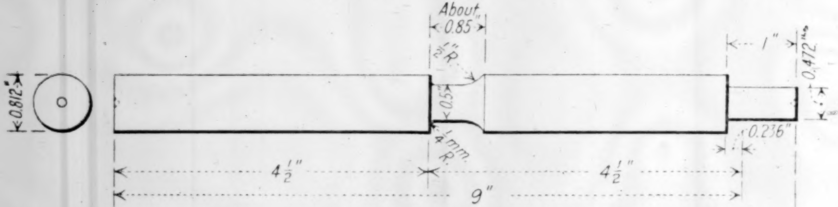
[1] "Prolongation" designates material from the test end prolongation of the shaft. "Block" designates material from the crank-throw test block, about midway of the shaft.

Types of Finish.—Endurance test specimens, preparatory to polishing, were smooth turned to within 0.010 in. of finished diameter. From this point, the finishing process will be designated as a "polish" although few of the specimens were actually polished. A number of types of "polish" were used, seven of which are referred to in this paper. In the earlier types, the revolving specimen was pressed against emery cloth or crocus cloth of various degrees of fineness and for varying lengths of time. Such a type of finish was described in a previous publication. In that publication, however, an error was made in describing the emery cloth as No. 1 instead of No. 000.

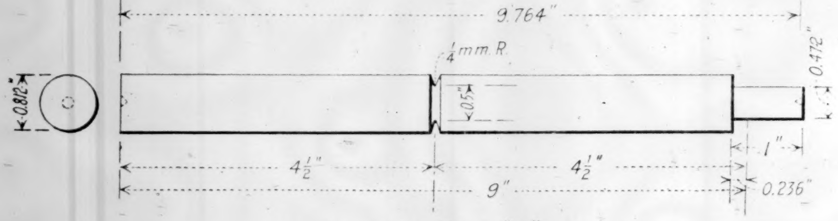
In types Nos. 6 to 11, inclusive, however, alternate longitudinal and circumferential polishing was used, the last polish being longitudinal. Circumferential polishing was done by the edge of a cloth-covered relatively thin revolving disk fed longitudinally along the specimen. Longitudinal polishing was done by a rapidly revolving cloth-covered cylinder fed longitudinally along the specimen. Emery, alundum or alumina powders are sprayed on these polishing wheels. Polishing of the specimen by this method, which is rapid, is continued until there are no visible circumferential scratches. The types of polish are described below:


This Dimension for Calculation only
0.36"
3/4"
3 3/4"
1 7/8"
1/2" R
Taper 0.5" per Foot in Dia.
0.312"
0.472"
0.812"
0.5"
0.75"
0.4687"
0.3904"
0.374"
4 1/2"
4 1/4"
4.624"
0.876"
10"

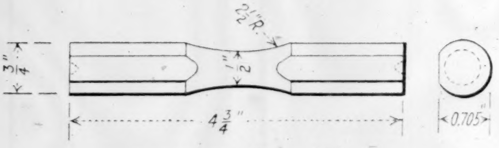
Specimen Type "C".


About 0.85"
3/4" R
1"
0.472"
0.812"
0.5"
1/4 mm R
0.236"
4 1/2"
4 1/2"
9"

Specimen Type "M".


9.764"
1/4 mm. R
0.472"
0.812"
0.5"
1"
4 1/2"
4 1/2"
9"
0.236"

Specimen Type "N".


2 1/2" R
3/4"
1/2"
4 3/4"
0.705"

Specimen Type "F".

FIG. 3.—Test Specimens

Polish types Nos. 2 and 3 are practically the same. Polish No. 2 is the designation given at the time of some of the older tests. The designation was changed to polish No. 3 when improvement was made in the skill of the polisher.

There were seven types of finish as follows:

Types Nos. 2 and 3.—Finishing tool, $\frac{1}{64}$-in. radius. Speed of lathe, 150 r.p.m. Feed, 80 threads per inch. No. 000 emery cloth for 10 minutes; No. 000 crocus cloth for 5 minutes; both circumferential.

Types Nos. 5, 6, 7, 9 and 11.—Finishing tool, $\frac{1}{2}$ in. radius. Speed of lathe, 90 r.p.m. Feed 130 threads per inch.

Type No. 5.—No. 000 emery cloth, circumferentially, 15 minutes; No. 000 crocus cloth, circumferentially, 5 minutes.

Type No. 6.—No. 000 emery cloth, longitudinally, 3 minutes; flour of emery, circumferentially, 3 minutes; No. 000 crocus cloth, longitudinally, 3 minutes.

Type No. 7.—Same as type No. 6, followed by levigated alumina 3 minutes. (Alundum instead of levigated alumina was intended.)

Type No. 9.—No. 000 emery cloth, longitudinally, 3 minutes; flour of emery, circumferentially, 3 minutes; flour of emery, longitudinally, 3 minutes.

Type No. 11.—Same as type No. 9, followed by alundum powder circumferentially, 3 minutes; alundum powder, longitudinally, 3 minutes; levigated alumina, circumferentially, 3 minutes; levigated alumina, longitudinally, 3 minutes.

General Description of Test.—The speeds of various units of cantilever endurance testing machines range from 1200 to 1800 r.p.m. The weights which cause the test stresses are suspended by springs. The machines are so arranged that the load can be applied after the machine reaches full speed and is automatically removed if the speed slackens below a certain point or if the machine stops. This provision was found necessary. As far as possible, a test on any specimen was not interrupted until the specimen broke or was removed.

Graphic Summary of Results.—The tabular results of endurance tests are not included, since it is believed that the results are better presented graphically. Results obtained with various kinds of steel are summarized graphically in Figs. 4 to 12, inclusive. In these graphs, semi-logarithmic plotting has been used. Usually each figure represents material of only one composition heat-treated in various ways. In a few figures there are shown comparative results obtained with various kinds of material, such as shaft material. In each figure the graphs are arranged in a definite order which is the same for all figures. This order, reading from the top to the bottom of a figure, is as follows: Water-quenched material arranged in order, beginning with that which has been subjected to the lowest drawing temperature and ending with that which has been subjected to the highest;

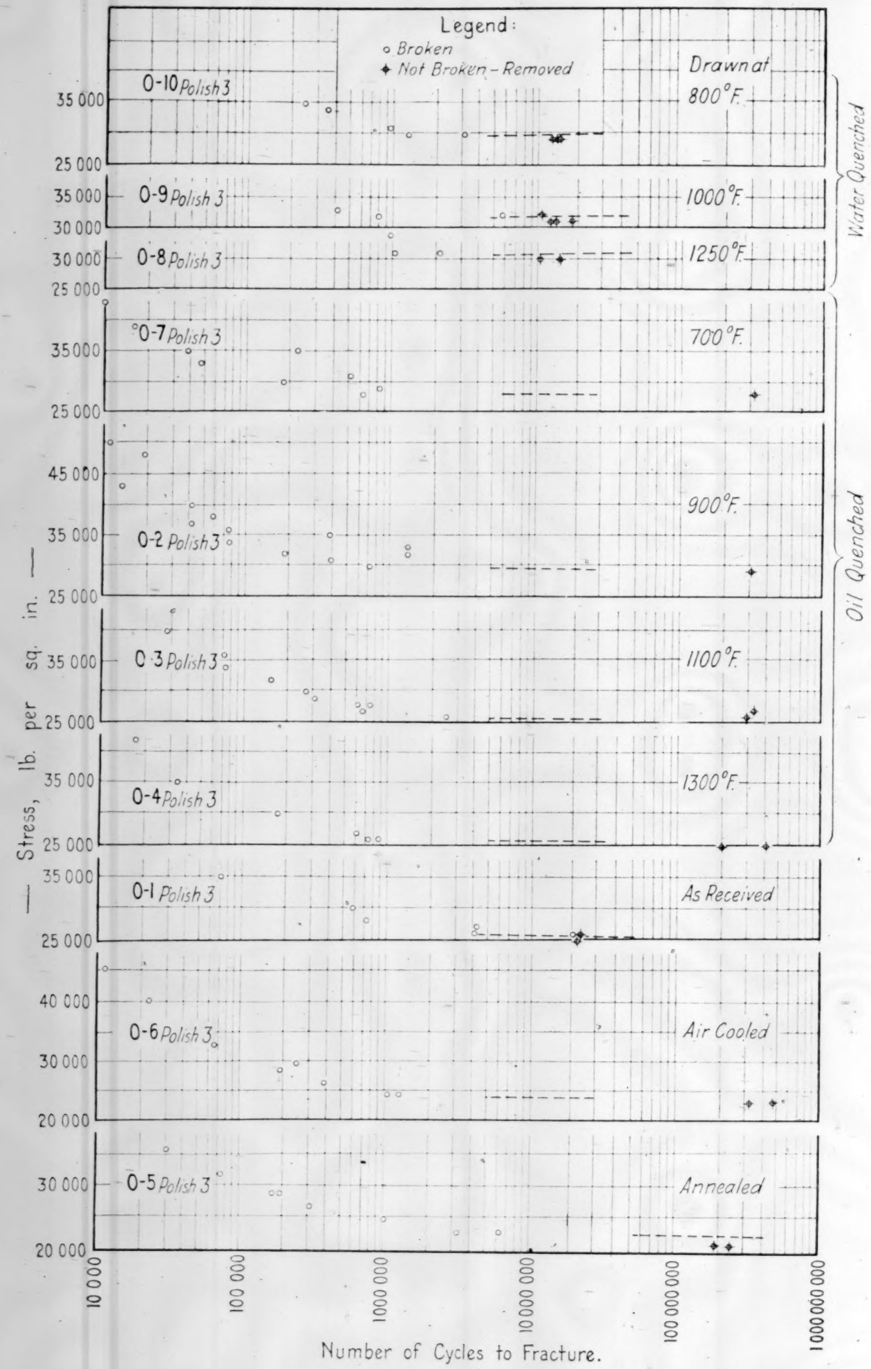


FIG. 4.—Results of Rotating Cantilever Endurance Tests. Material O

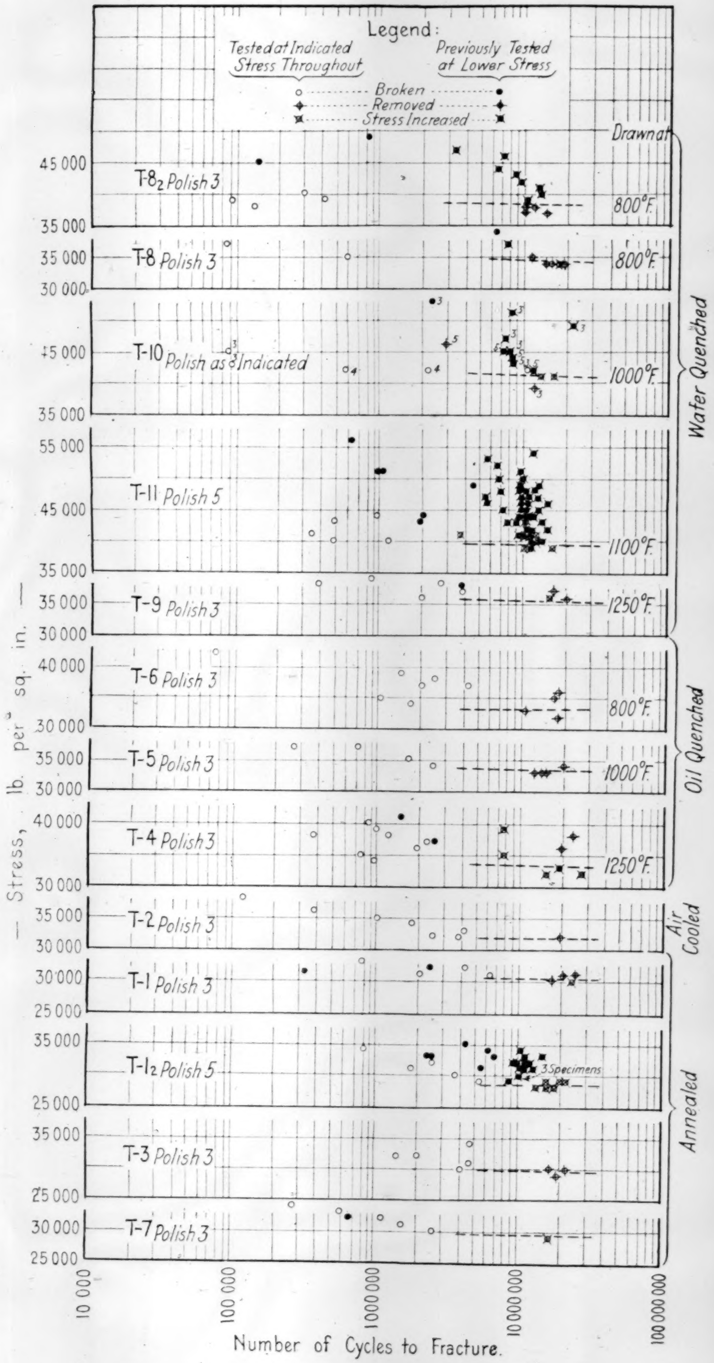


Fig. 5.—Results of Rotating Cantilever Endurance Tests. Material T

oil-quenched material, in the same order as water-quenched material; air-cooled material; furnace-cooled material.

Since the slope of the idealized line representing stress-cycle relations is only slight for abscissas corresponding to more than ten million cycles, it was considered desirable, in order to save time, to stop nearly all the tests at about ten million cycles. No attempt has been made to draw idealized lines to represent the stress-cycle relationships. In many cases such curved lines could readily be drawn through the plotted points representing individual results. The important part of each graph, however, is the nearly horizontal portion representing the lower stresses. For this reason, short horizontal lines have been drawn at the estimated points where the idealized graph cut the ordinate corresponding to ten million cycles. The value represented by such an ordinate has been designated in certain tables and graphs yet to be discussed as the "Endurance Limit for 10,000,000 Cycles."

Description of Figs. 4 to 12, Inclusive.—Fig. 4 summarizes the rotating cantilever endurance tests of material O, a carbon steel containing about 0.24 per cent carbon. In the most regular of the graphs here shown it is quite evident that the idealized line representing the endurance properties of such material is a continuous curve whose slope gradually decreases with decrease in stress until it becomes nearly horizontal. The "endurance limit for 10,000,000 cycles" varies from 22,500 lb. per sq. in. in annealed material to 29,500 lb. per sq. in. in material that has been water-quenched and drawn at 800° F.

Fig. 5 gives a summary of similar experiments on material T, which is carbon steel with about 0.38 per cent of carbon. The "endurance limit" of this material varies according to the heat treatment from about 29,500 to 41,500 lb. per sq. in. During the endurance tests on this material some special experiments were made to determine the effect on the "endurance limit" of previously subjecting the specimen to a number of reversals of stress of lower intensity. The results obtained with these specimens were compared with results obtained with similar material not previously so treated. For example, about one-half of the specimens of material T-11 were subjected to over ten million cycles of stress of 39,000 or 40,000 lb. per sq. in. The stress in each specimen was then raised by 1000 or 2000 lb. per sq. in. and the test was continued for about ten million more cycles when the stress was again raised. This was continued until the specimen broke. Each time that the stress was raised the counting of cycles of raised stress was started at zero and the results were so plotted. In these graphs the meaning of the various marks are as follows:

An open circle indicates specimen tested throughout and broke at the indicated stress;

An open circle crossed by diagonal lines indicates specimen subjected to the indicated number of cycles at the indicated stress, the stress then increased;

An open circle crossed by a vertical and a horizontal line indicates specimen removed after being subjected to the indicated number of cycles at the indicated stress;

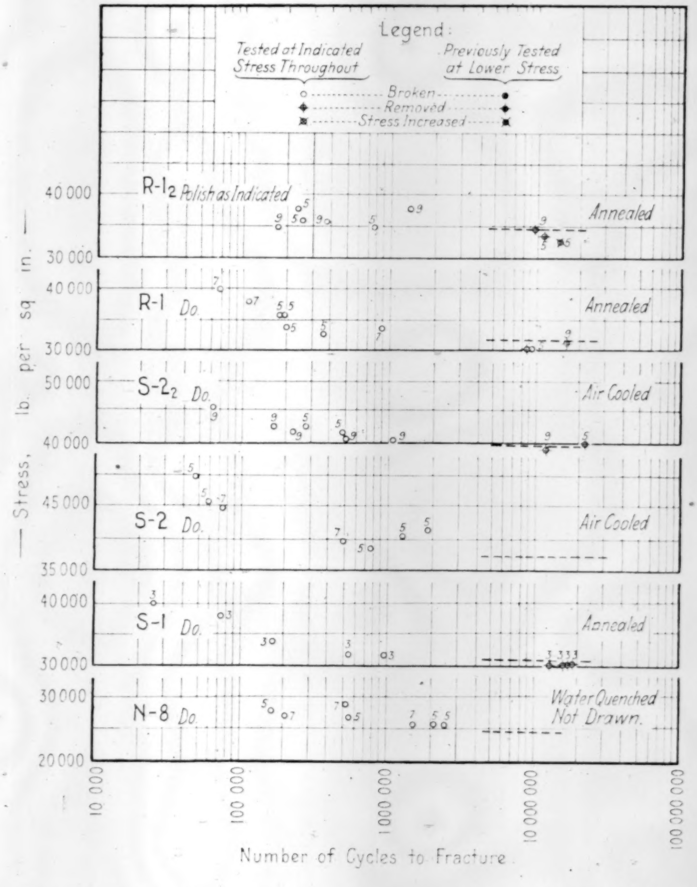


FIG. 6.—Results of Rotating Cantilever Endurance Tests. Materials N, S, and R

A circle blacked in indicates specimen broke at the indicated stress and number of cycles; previously tested at lower stress;

A circle blacked in crossed by diagonal lines indicates specimen after previous test at lower stress was subjected to the indicated stress and number of cycles; stress was then raised;

A circle blacked in crossed by a vertical and a horizontal line indicates specimen after previous test at lower stress was subjected to the indicated stress and number of cycles; specimen was then removed.

Examination of these graphs makes it evident that the endurance limit of this material has been raised by the previous testing at stresses below this limit. This conclusion agrees with conclusions expressed by H. F. Moore.

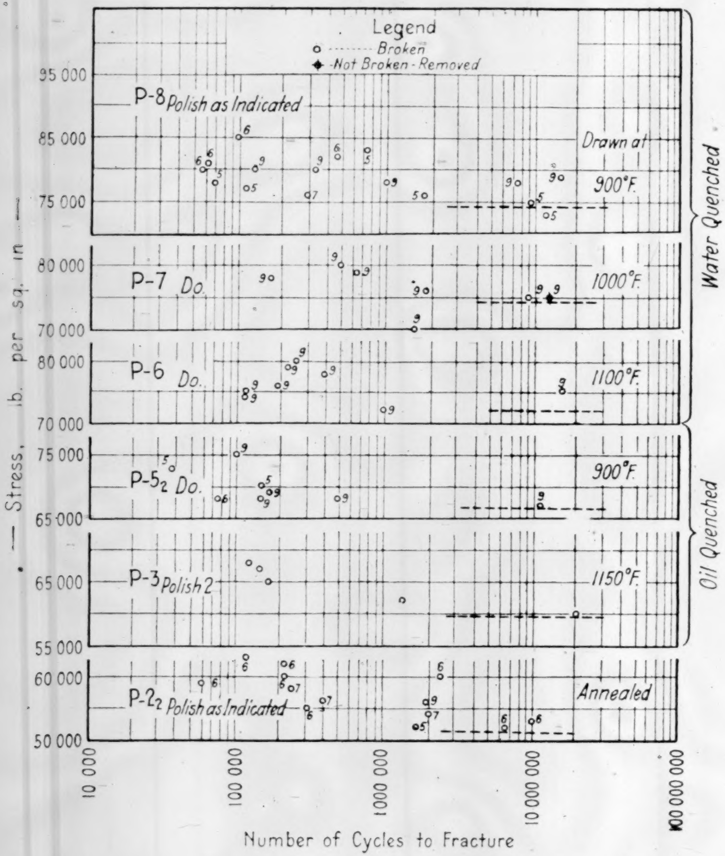


FIG. 7.—Results of Rotating Cantilever Endurance Tests. Material P

Fig. 6 summarizes results obtained with steels of high carbon content, materials R and S. For comparison there are also included results obtained with water-quenched ingot iron, material N-8. It will be noted that for these samples of annealed high-carbon steel the endurance limit for 10,000,000 cycles is not much greater than that of medium-carbon steel and is only about one-third greater than that of ingot iron. It will also be noted that the different types of polish used have given practically identical results.

Figs. 7 and 8 (*a*) and (*b*) give results obtained with nickel steels, materials P and Q. As shown in Fig. 7, the endurance limit of material P increases with increase in the rate of cooling of the steel from above the critical range and with decrease in the drawing temperature. As shown in Fig. 8 (*a*) and (*b*) there are irregularities in the relationship of the endurance limit of material Q to the drawing temperature of heat treatment. This is especially noticeable in the oil-quenched material. Since even the five annealing treatments as shown in Fig. 8 (*b*) gave endurance limits ranging from 41,500 to 50,000 lb. per sq. in., it is believed that the irregularities are due to differences in com-

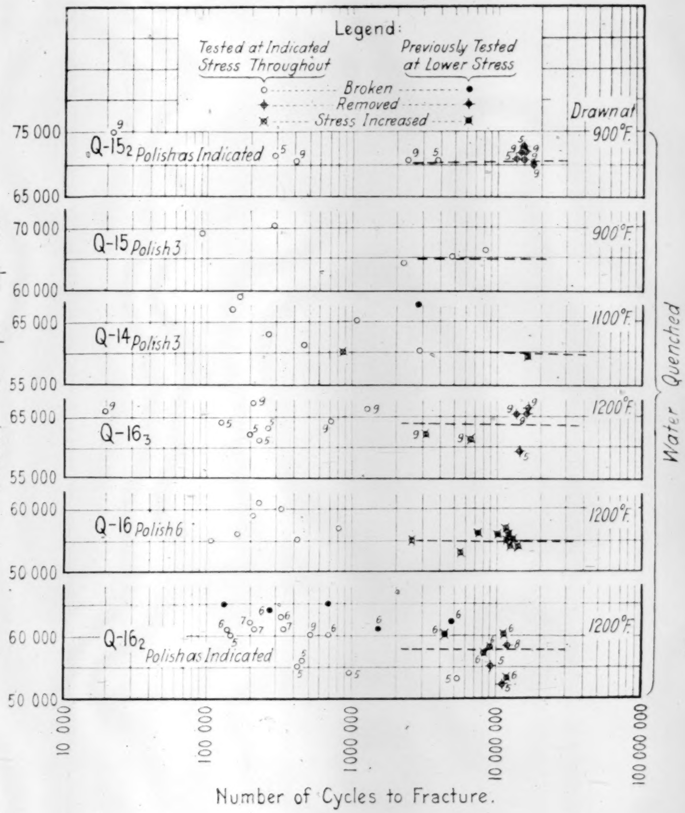


Fig. 8 (*a*).—Results of Rotating Cantilever Endurance Tests. Material Q, Water Quenched

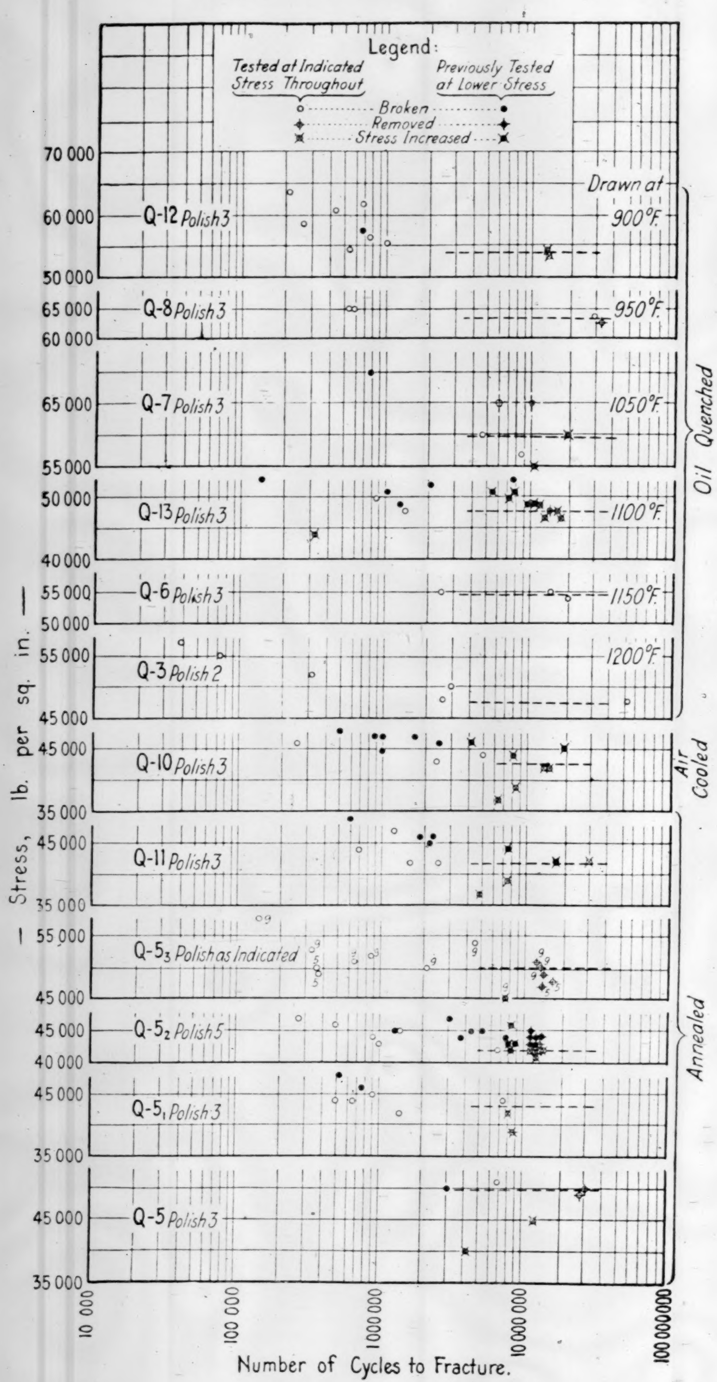


FIG. 8 (*b*).—Results of Rotating Cantilever Endurance Tests. Material Q, Oil Quenched, Air Cooled and Annealed

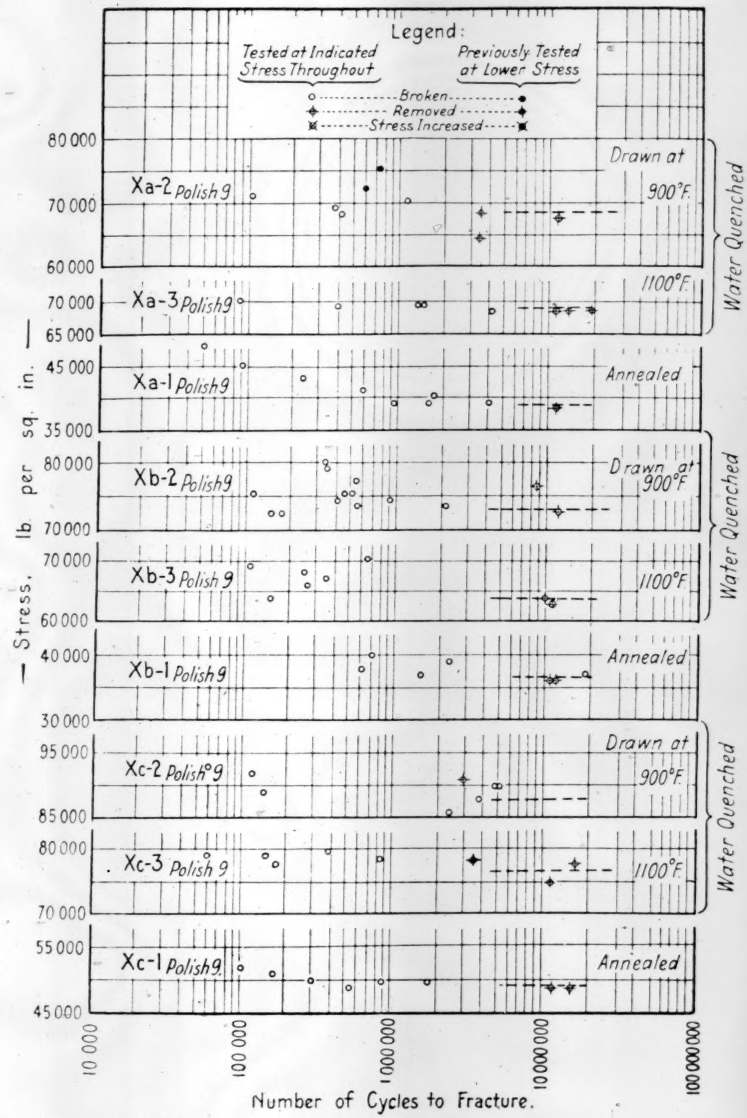


FIG. 9.—Results of Rotating Cantilever Endurance Tests.
Materials Xa, Xb and Xc

position of different bars supposed to be of the same composition. These bars, obtained on requisition from a navy yard, are possibly not all from the same heat. The chemical composition of each bar will therefore, be determined. Whatever the cause of the apparent exceptions, however, the relationship of endurance limit to heat-treatment is in general the same for material Q as for material P.

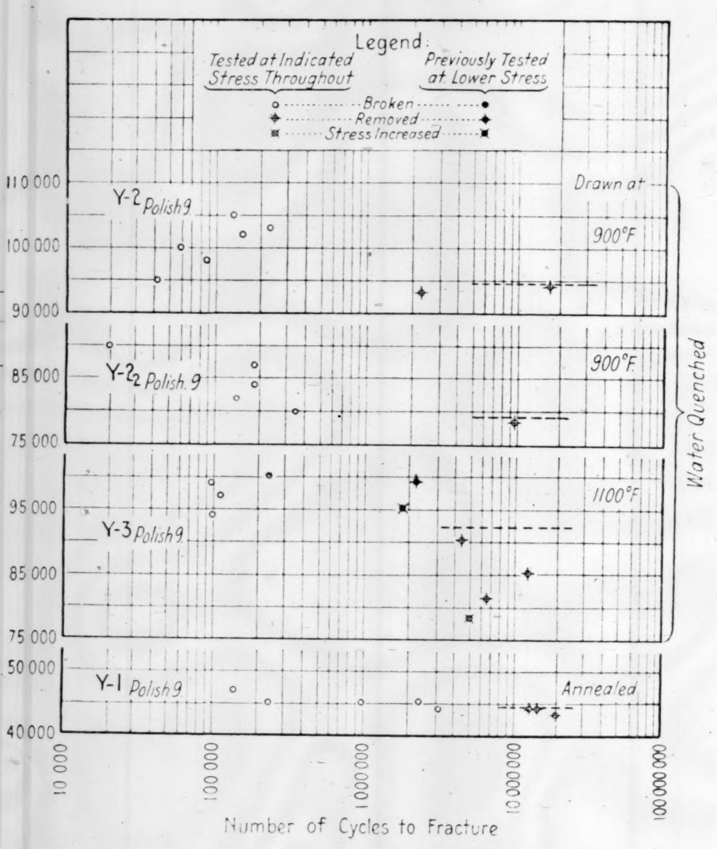


Fig. 10.—Results of Rotating Cantilever Endurance Tests. Material Y

As in the case of materials R and S, the various types of "polish" used have given practically identical results. The strengthening effect of previously applied alternating stresses below the endurance limit is apparently less in this material than in low-carbon steels.

Fig. 9 gives results of tests on chrome-molybdenum steels of three different compositions, materials Xa, Xb, and Xc. As shown in this figure, the relationship of endurance limit to heat treatment is similar to that obtained with carbon and nickel steels.

Fig. 10 summarizes results obtained with chrome-vanadium steel, material Y. There is great discrepancy between the results obtained with materials Y-2 and $Y\text{-}2_2$. The reason for this is being investigated. It should be noted that the discrepancy between the endurance limits of materials Y-2 and $Y\text{-}2_2$ is accompanied by a proportional discrepancy in tensile properties.

Fig. 11, showing results obtained with chrome-nickel steel, material Z, requires no special description.

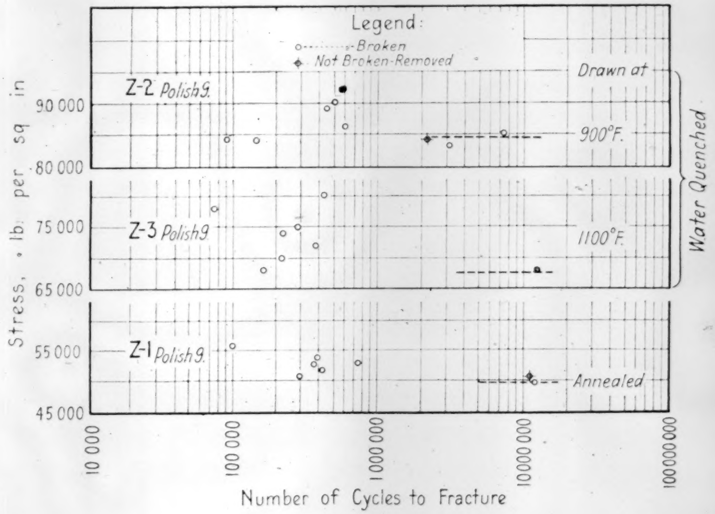


Fig. 11.—Results of Rotating Cantilever Endurance Tests. Material Z

Fig. 12 summarizes results obtained with shafting of battleships, submarines and aeroplanes. The results obtained with battleship and submarine shafts are of especial interest, since there is need for investigation of the relationship in large shafts of endurance properties to other physical properties in both longitudinal and transverse directions. A description of the shaft represented by each designation in Fig. 12 is given in Table II. Material ZA was obtained from test-end prolongations of aeroplane crank shafts. Since these shafts were manufactured in one lot, the results have been included in one graph.

Shafts Nos. 2 and 3 are battleship shafts of nickel steel. To show the influence of "fiber" in such material the results obtained with tangential and longitudinal specimens from the same shaft have

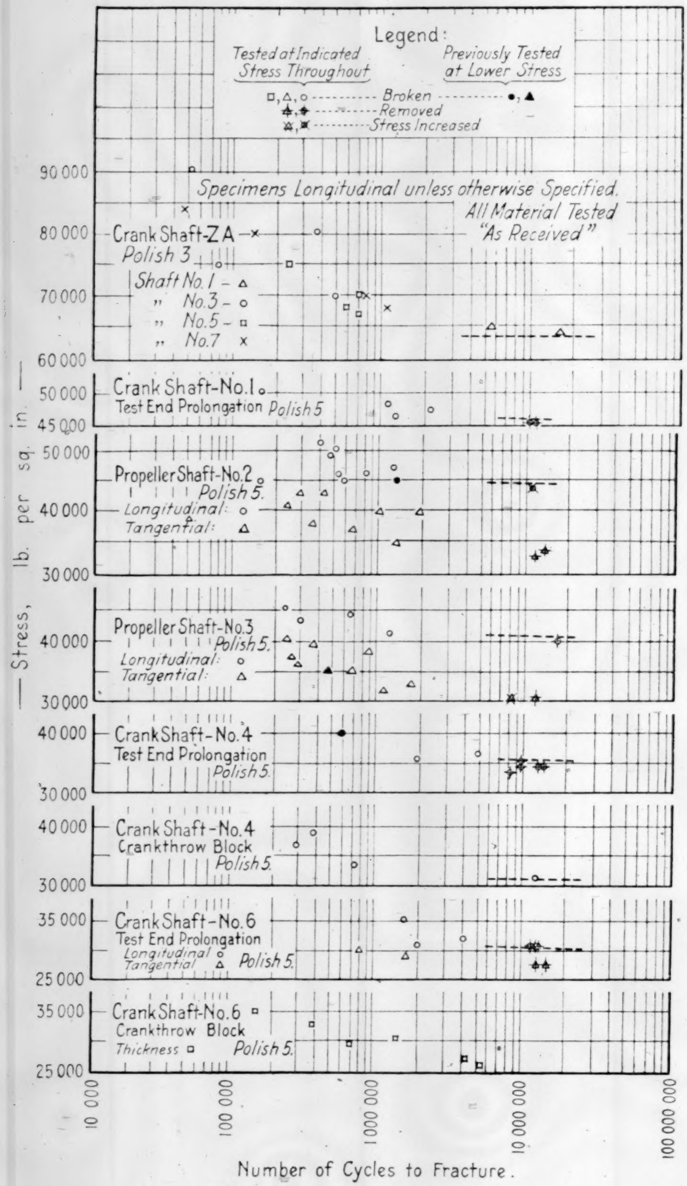


FIG. 12.—Results of Rotating Cantilever Endurance Tests. Crank-Shaft and Propeller-Shaft Material

been included in the same graph. The indicated endurance limits are for longitudinal specimens alone. The test end prolongation of crank shaft No. 4 showed a higher endurance limit than the crank throw block. This might have been expected, since the former had been forged down nearly to the size of the finished journal, while the latter was cut from between two crank webs where the cross-section of the forged slab was much greater.

ALTERNATING TORSION TESTS

Types of Specimen and Finish.—The specimen used in most of the alternating torsion tests is shown as Type F in Fig. 3. In this specimen, at a distance of about 0.1 in. from the mid-length, the stress is only about 2.2 per cent less than the maximum. It is believed, therefore, that the stress in this specimen is sufficiently uniform to give satisfactory results.

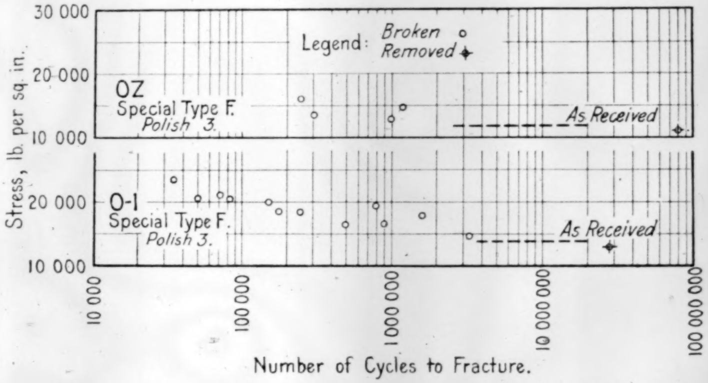


FIG. 13.—Results of Alternating Torsion Endurance Tests. Material O. Specimen as indicated. Speed 2140 r.p.m.

Specimens J and K were used in a few special experiments to determine the comparative effects of abrupt changes of section in material of various physical properties. They were notched specimens, the angles between the sides of the notch being 60 deg. and the radii at the bottom of the notch being $\frac{1}{2}$ and $\frac{1}{4}$ mm., respectively.

The various types of polish and designations are the same as those used on rotating cantilever endurance test specimens.

General Description of Test.—Most of the results have been obtained with the high-speed alternating torsion machine of the inertia type. In this machine the stress can be regulated by adjustment of one or more of three factors: speed, radius of crank-pin motion, and inertia of fly wheel. Since it is inconvenient to make frequent change of the adjustment of the speed regulator, the desired stress was usually obtained by adjusting the radius of crank-pin motion and by varying the inertia of the fly wheel by addition of rings of known moment of inertia.

In the use of this machine it was soon found that great care was necessary to avoid critical speeds. Since the critical speed varies with the type of specimen and inertia of the fly wheel, it was necessary to determine experimentally the critical speeds for each combination. Most of the endurance test results were obtained at a speed of about 2140 r. p. m.

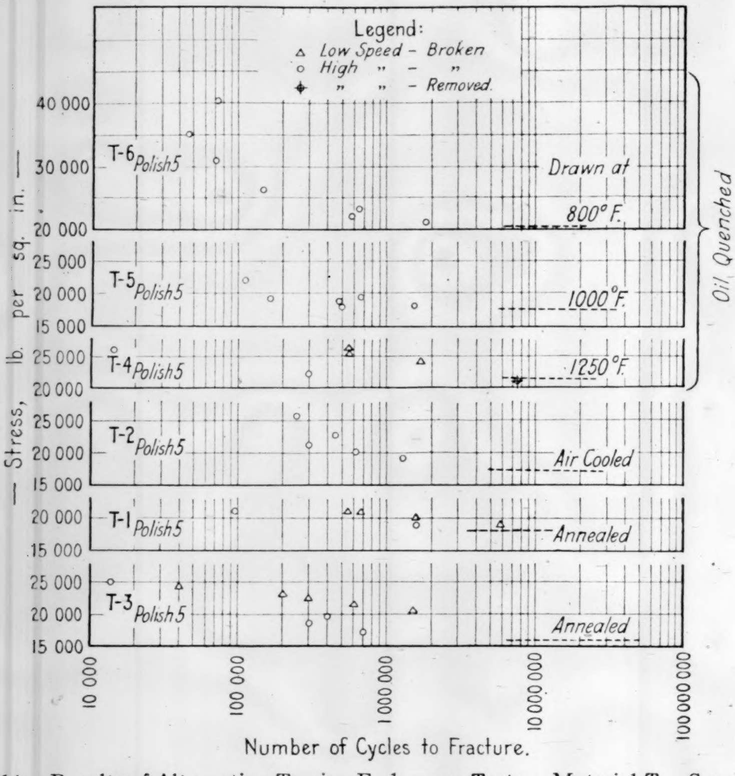


FIG. 14.—Results of Alternating Torsion Endurance Tests. Material T. Specimen Type F. Speed as indicated

Mention should be made of the fact that the inertia machine does not produce in the specimen exactly equal positive and negative stresses. The difference between the numerical values of the positive and negative stresses depends on the ratio of the length of the radius of crank-pin motion to the length of the connecting rod. The ratio of the positive to the negative stress is equal to $\frac{n-1}{n+1}$. The value designated n is obtained by dividing the length of the connecting rod by the length of the radius of crank-pin motion. In the alternating torsion tests with the inertia machine, the value of n was so large that the difference between the positive and negative stresses amounted to only a few per cent. Since experiments at the Naval Experiment Station and elsewhere have shown that the stress range varies slowly if at all as it begins to depart from the condition of complete reversal, it is evident that the stress values obtained with this machine may be assumed with sufficient accuracy to represent equal positive and negative stresses.

The low-speed alternating torsion machine in which the stress is applied and measured by springs, has been used to obtain a few results for comparison with those obtained with the high-speed machine. It was found that, while the results obtained with either type of machine are consistent among themselves, the results obtained with the low-speed machine do not agree with the results obtained with the high-speed machine. Tests with the inertia machine have been made on the same material at speeds varying from 2140 to 725 r. p. m. These experiments, involving an eight-fold increase of the moment of inertia of fly wheel as the speed was reduced, have given practically the same value for the endurance limit of the material. These results, however, are apparently from 10 to 20 per cent lower than the results obtained on the same material with the spring type of machine. Additional experiments on the inertia machine are to be made at still lower speeds to determine the relationship between results obtained with the two types.

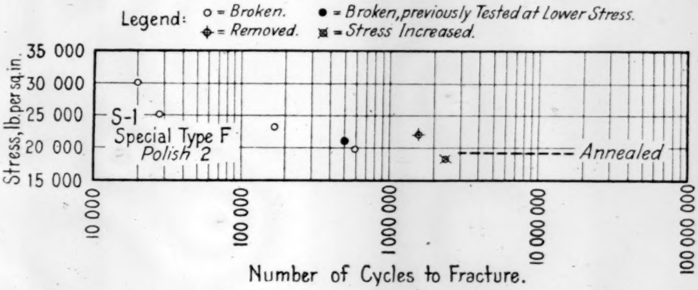


Fig. 15.—Results of Alternating Torsion Endurance Tests. Material S. Specimen Type F. Speed 2140 r.p.m.

While it is possible, therefore, that a constant correction will have to be applied to the alternating torsion results recorded in this paper, it is considered desirable to publish them at this time since they show the relative endurance limits, at least, of various kinds of steel.

Graphic Summary of Results.—The results of alternating torsion tests on practically the same material that was subjected to rotating cantilever tests are summarized graphically in Figs. 13 to 18, inclusive. The method of plotting and the arrangement of graphs are the same as in the graphs of cantilever endurance results.

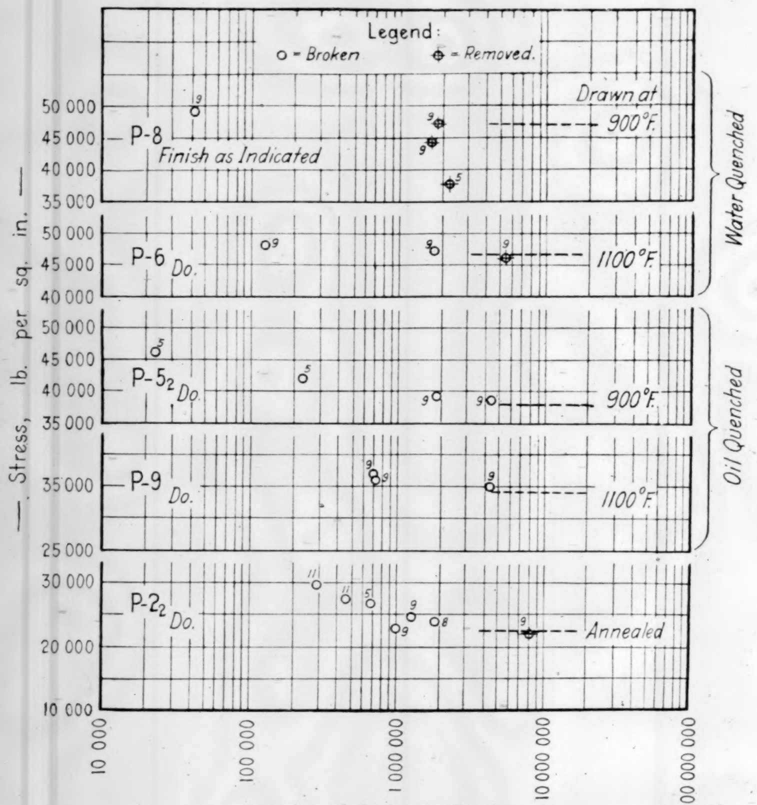


Fig. 16.—Results of Alternating Torsion Endurance Tests. Material P. Specimen Type F. Speed 2140 r.p.m.

Study of these graphs will show that in general they are of the same type as those representing results of cantilever endurance tests. The apparently anomalous relationships between results of certain heat treatments also correspond in the two kinds of endurance tests.

In Fig. 18 transverse as well as longitudinal results of some of the shafts are included to illustrate the effect of "fiber." The indicated endurance limits, however, are those obtained with longitudinal specimens.

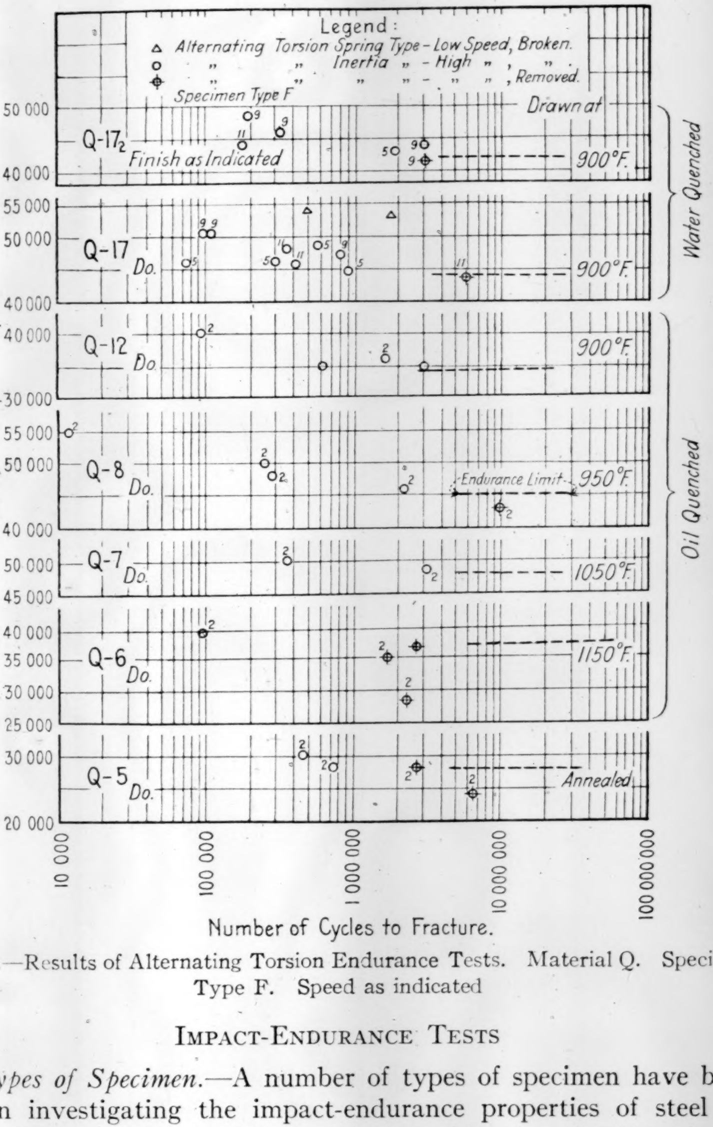


FIG. 17.—Results of Alternating Torsion Endurance Tests. Material Q. Specimen Type F. Speed as indicated

IMPACT-ENDURANCE TESTS

Types of Specimen.—A number of types of specimen have been used in investigating the impact-endurance properties of steel by means of the machine developed at the Naval Engineering Experiment Station. The specimens used have been of various diameters and have

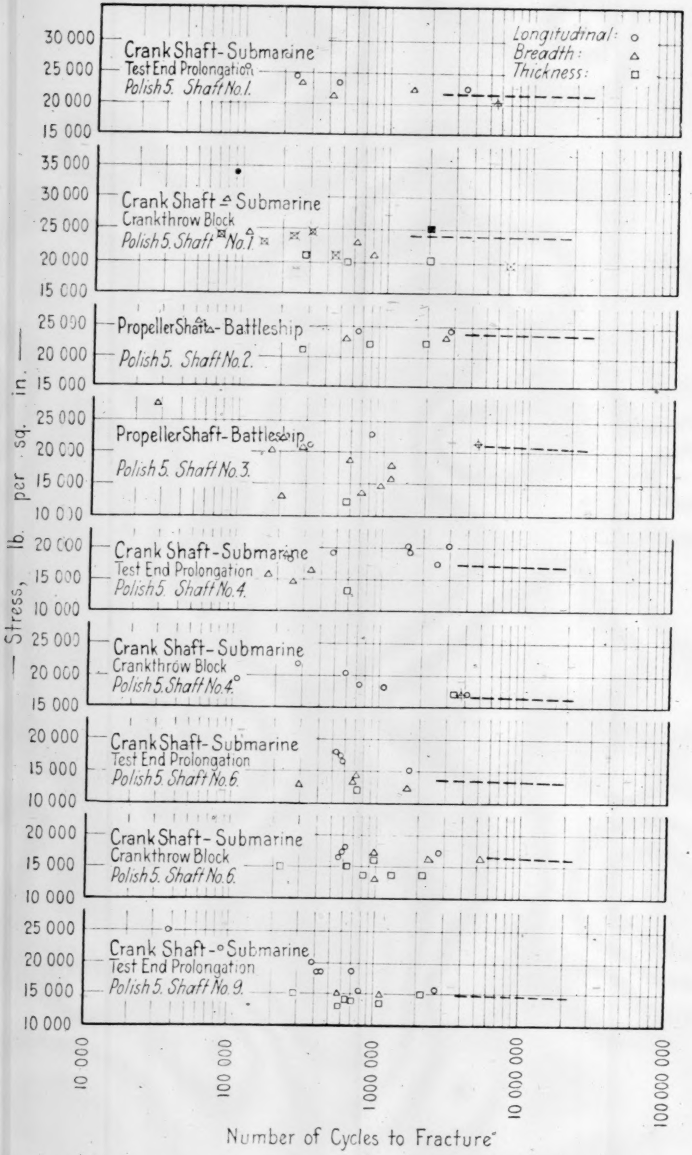


Fig 18.—Results of Alternating Torsion Endurance Tests. Crank-Shaft and Propeller-Shaft Material. Specimen Type F. Speed 2140 r.p.m.

had notches of various shapes and depths and with various radii at the bottom. The results obtained with two of these types have been selected to illustrate the impact-endurance properties of steel. The two types may be described as follows:

Specimen A—Diameter, 0.5 in.; diameter at bottom of notch, 0.4 in.; notch sides parallel; radius at bottom, $\frac{1}{16}$ in. Specimen B—Diameter, 0.75 in.; diameter at bottom of notch, 0.6 in.; angle between sides of notch, 60 deg., radius at bottom of notch, $\frac{1}{4}$ mm.

General Description of Impact-Endurance Test.—The object in view was to investigate for various kinds of steel the relationship between energy of blow and the number of blows necessary to cause fracture. For this purpose, a number of specimens of each kind of material were tested with various amounts of energy of blow. For making such a series of tests there were available two hammers differing considerably in weight; to each of these hammers several additional weights could be attached. By selecting suitable weights of hammer and heights of drop the energy of blow could be varied over a wide range. The number of blows necessary to cause fracture could be thus varied from about 500 to many millions. It was considered desirable, however, to investigate the impact-endurance properties of steel over an even wider range of energy of blow than that available with the above described machine. Arrangements were made, therefore, to increase the energy of blow so as to cause fracture after still fewer blows and even at a single blow. In this way, the impact-endurance properties of steel could be connected directly with behavior of the metal under single impact.

To cause fracture in specimens of Types A and B at a single blow, a Charpy machine was adapted for the purpose by inserting dies of special form so that a cylindrical specimen could be broken with little distortion of the surface. With this modified Charpy machine the energy absorbed in fracture could be determined in the usual manner. Specimens of various kinds of steels were tested in this manner. Other specimens of the same kinds of steel were broken by slow bending in suitable dies while an automatic diagram was being made from which could be calculated the energy necessary to cause fracture.

Energy of blow intermediate between that necessary to cause fracture at single impact and that available in the impact-endurance machine was secured by lifting the heaviest hammer of the impact-endurance machine by hand to a temporary platform and releasing it at proper intervals to fall on the rotating specimen.

Graphic Summary of Results.—Results of experiments on various kinds of steel with specimens of Type A are shown graphically in Fig.

19. Results of experiments on two kinds of steel with specimens of Type B are shown in Fig. 20. In these graphs logarithmic ordinates as well as abscissa are used. This is made necessary by the wide range of energy of blow.

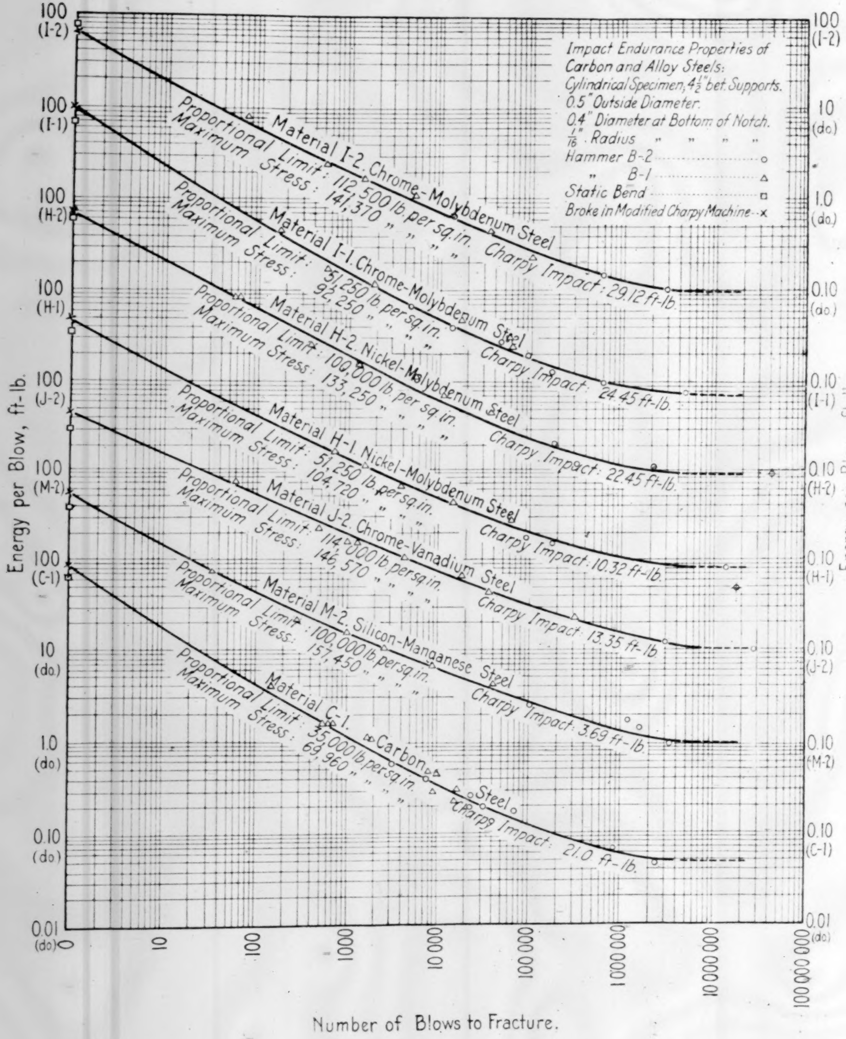


FIG. 19.—Impact-Endurance Properties of Carbon and Alloy Steels

In Fig. 19 are shown impact-endurance graphs of various carbon and alloy steels whose endurance properties have been discussed in a previous article.[1] The designations used in the graph are those of the article referred to. On each ordinate corresponding to single impact the amount of energy absorbed in slow bending and the amount absorbed in the modified Charpy machine have been recorded. The graphs have been drawn through the latter points. At the abscissa representing ten million blows a horizontal broken line has been drawn for each graph. The ordinate of this line represents for specimen A the "impact-endurance limit for 10,000,000 cycles."

In each of the graphs, the portion between the ordinates corresponding to a single blow and to about 100,000 blows is nearly straight.

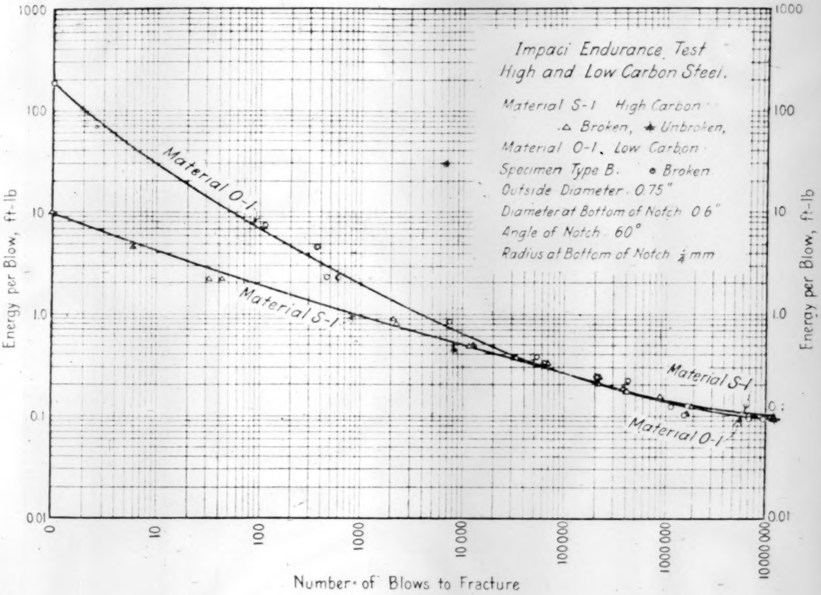


FIG. 20.—Impact-Endurance Properties of a High and a Low-Carbon Steel

At about the latter ordinate, each line begins to curve. Somewhere between the ordinates corresponding to 1,000,000 and 10,000,000 blows, the curvature is practically completed and each graph becomes nearly horizontal.

It might be expected that the ordinates at the extreme left of the various graphs would be roughly proportional to the Charpy values. That this is not the case is probably due to the usual wide variation of numerical values obtained with the deeply notched specimen used in the Charpy test. In general, however, as shown in columns Nos. 2 and 3 of Table X, low or high Charpy values are accompanied respectively by low or high "modified Charpy" values. It is of interest to compare the "impact-endurance limits for 10,000,000 cycles" with the rotating cantilever "endurance limits for 100,000,000 cycles" as recorded in the previously published paper.[1] This comparison is shown in columns Nos. 4 and 5 of Table X. It will be noted that, as shown in column No. 6, the ratio of the endurance limits as obtained in the two different kinds of test is fairly constant. The constancy is surprising when it is considered that the endurance limits as obtained from Fig. 19 (which were located in this figure before the idea of comparison with cantilever endurance limits suggested itself) are based on comparatively few results obtained with a type of specimen that would be expected to give variable results.

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[1] D. J. McAdam, Jr., "Endurance of Steel under Repeated Stresses," *Chemical and Metallurgical Engineering*, December 14, 1921.

Table X.—Impact-Endurance Properties of Carbon and Alloy Steels

Material Designation	Modified Charpy Values, ft-lb	Charpy Values, ft-lb.	Impact Endurance Limits, ft-lb.	Rotating Cantilever Endurance Limits, lb. per sq. in.	Ratio of Values of Column 5 to Column 4
I-2	61.5	29.12	0.10	67 500	670 000
I-1	92.6	24.45	0.075	47 000	630 000
H-2	68.0	22.45	0.10	57 000	570 000
H-1	46.3	10.32	0.085	46 500	550 000
J-2	45.6	13.35	0.11	67 000	610 000
M-2	56.4	3.69	0.098	62 000	630 000
C-1	93.2	21.0	0.048	30 000	620 000

Evidently, therefore, the ordinate at the extreme left of the impact-endurance graph depends on the impact properties and the ordinates at the extreme right depend on the endurance properties of the metal. Between these two extremes the influence of the impact properties decreases and that of the endurance properties increases with increase in the number of blows necessary to cause fracture.

The notch in the Type A specimen, with which the results summarized in Fig. 19 were obtained, has a larger radius at the bottom than does the notch in a standard impact specimen. For this reason it was considered desirable to obtain a series of results with the Type B specimen. Impact-endurance tests were, therefore, made with Type B specimen on low and high-carbon steels. The results are illustrated graphically in Fig. 20. As shown in this figure the graph representing the properties of high-carbon steel crosses the graph representing the properties of low-carbon steel. At the extreme left the ordinates representing the two kinds of steel are roughly proportional to the Charpy values and at the extreme right the ordinates are roughly proportional to the rotating cantilever "endurance limits for 10,000,000 cycles." Fig. 20, therefore, leads to the same conclusions as those expressed after discussion of Fig. 19.

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[1] D. J. McAdam, Jr., "Endurance of Steel under Repeated Stresses," *Chemical and Metallurgical Engineering*, December 14, 1921.

The Stanton Impact-Endurance Test.—It appears to be the usual practice in making the Stanton test to use an arbitrarily selected energy of blow and to compare samples of steel by comparing the number of blows endured before fracture. As illustrated by Figs. 19 and 20, the meaning of such a test will depend on the energy of blow selected and on the type of specimen. In most recorded Stanton results, the relationship of blow to specimen is such that the specimen is broken after not more than a few thousand blows. Under such conditions, as shown in Fig. 20, the impact properties of the metal are predominant, and the endurance properties have very little influence. Since in a test made in this way the position of the results on the impact-endurance graph varies with the properties of the material, it is evident that such a test can have little value in determining either a single property or a combination of properties of the metal.

RELATIONSHIP OF ENDURANCE LIMITS TO TENSILE AND TORSIONAL STRENGTH

In Tables XI and XII are assembled the endurance limits as obtained from the graphs of Figs. 4 to 18, inclusive, and in parallel columns the average values of tensile and torsional strength. In other parallel columns are recorded the following calculated ratios: Ratio of torsional strength to tensile strength; ratio of alternating torsion endurance limit to tension-compression endurance limit; ratio of tension-compression endurance limit to tensile strength; ratio of alternating torsion endurance limit to tensile strength; and ratio of alternating torsion endurance limit to torsional strength.

Relationship of Tension-Compression Endurance Limit to Tensile Strength.—In column No. 10 of Tables XI and XII are recorded the ratios of tension-compression endurance limit to tensile strength. The extreme variation in these ratios is from 0.339 to 0.564. In comparing the ratios for material of the same chemical composition it will be noticed that the effect of heat treatment on the ratio is for some kinds of material negligible while for other kinds of material it is considerable. To facilitate comparison of the various kinds of steel

TABLE XI.—ENDURANCE LIMITS FOR 10,000,000 CYCLES; THEIR RELATIONSHIP TO TENSILE STRENGTH AND TORSIONAL STRENGTH. BAR MATERIAL

Material Designation	Cooled in	Drawn at — deg. Fahr.	Average Tensile Strength, lb. per sq. in.	Average Torsional Strength, lb. per sq. in.	Endurance Limit for 10,000,000 Cycles, lb. per sq. in.		Ratio of Values				
					Rotating Cantilever	Alternating Torsion	Column 5 to Column 4	Column 7 to Column 6	Column 6 to Column 4	Column 7 to Column 4	Column 7 to Column 5
1	2	3	4	5	6	7	8	9	10	11	12
N-8	Water	....	46 700	40 700	25 000	......	0.872	.....	0.535	.....	.....
O-10	Water	800	78 820	60 200	29 500	......	0.764	.....	0.374	.....	.....
O-9	"	1000	79 160	59 300	31 500	......	0.749	.....	0.398	.....	.....
O-8	"	1250	67 100	54 300	30 500	......	0.809	.....	0.455	.....	.....
O-7	Oil	700	68 500	56 600	27 500	......	0.826	.....	0.401	.....	.....
O-2	"	900	67 500	60 500	29 500	......	0.896	.....	0.437	.....	.....
O-3	"	1100	66 000	53 440	25 500	......	0.810	.....	0.386	.....	.....
O-4	"	1300	60 300	52 300	25 500	......	0.867	.....	0.423	.....	.....
O-1	As Received		60 400	53 430	25 500	14 000	0.885	0.549	0.422	0.232	0.262
O-6	Air	....	60 750	48 730	24 500	......	0.802	.....	0.403	.....	.....
O-5	Furnace	....	55 120	46 640	22 500	......	0.846	.....	8.408	.....	.....
Average of O Material							0.825	0.549	0.411	0.232	0.262
T-8$_2$	Water	800	96 620	67 480	38 500	......	0.698	.....	0.398	.....	.....
T-8	"	800	85 250	63 390	34 500	......	0.744	.....	0.405	.....	.....
T-10	"	1000	97 370	60 450	41 500	......	0.621	.....	0.426	.....	.....
T-11	"	1100	86 250	64 300	39 500	......	0.746	.....	0.458	.....	.....
T-9	"	1250	82 000	62 490	35 500	......	0.767	.....	0.433	.....	.....
T-6	Oil	800	91 500	57 730	33 500	20 500	0.631	0.612	0.366	0.224	0.355
T-5	"	1000	85 250	65 210	33 500	16 500	0.765	0.493	0.393	0.194	0.253
T-4	"	1250	84 010	56 150	33 500	21 500	0.668	0.642	0.423	0.256	0.383
T-2	Air	....	77 490	67 020	32 000	17 500	0.865	0.547	0.413	0.226	0.261
T-1	Furnace	....	70 000	60 000	30 500	18 000	0.857	0.590	0.436	0.257	0.300
T-1$_2$	"	....	69 370	54 300	28 500	......	0.783	.....	0.411	.....	.....
T-3	"	....	72 000	54 790	29 500	16 000	0.761	0.545	0.410	0.222	0.292
T-7	"	....	70 000	52 980	29 500	......	0.757	.....	0.421	.....	.....
Average of T Material							0.743	0.571	0.415	0.230	0.307
S-2$_2$	Air	....	119 300	74 270	40 500	......	0.623	.....	0.339	.....	.....
S-2	"	....	110 000	72 000	38 500	......	0.655	.....	0.350	.....	.....
S-1	Furnace	....	89 750	70 645	31 500	19 000	0.787	0.603	0.351	0.212	0.269
R-1$_2$	Furnace	....	91 890	......	34 500	......	.....	.....	0.375	.....	.....
R-1	"	....	87 500	69 900	32 500	......	0.799	.....	0.371	.....	.....
Average of S and R Material							0.732	0.603	0.357	0.212	0.269
P-8	Water	900	154 200	91 370	74 000	47 000	0.593	0.635	0.480	0.305	0.514
P-7	"	1000	149 250	93 300	74 500	......	0.625	.....	0.499	.....	.....
P-6	"	1100	132 250	86 940	72 000	46 500	0.657	0.648	0.544	0.351	0.534
P-5$_2$	Oil	900	118 000	77 000	66 500	38 000	0.653	0.571	0.564	0.322	0.494
P-3	"	1150	123 000	81 060	59 500	......	0.659	.....	0.484	.....	.....
P-2$_2$	Furnace	....	113 630	77 890	51 000	22 500	0.685	0.441	0.449	0.198	0.298
Average of P Material							0.645	0.574	0.503	0.294	0.460
Q-17	Water	900	145 750	99 000	......	44 000	0.679	.....	.....	0.302	0.444
Q-15$_2$	"	900	145 880	......	69 500	......	.....	.....	0.476	.....	.....
Q-17$_2$	"	900	138 750	88 000	......	42 000	0.634	.....	.....	0.303	0.477
Q-15	"	900	142 900	88 750	64 500	......	0.621	.....	0.451	.....	.....
Q-14	"	1100	115 900	77 890	59 000	......	0.672	.....	0.509	.....	.....
Q-16$_3$	"	1200	112 000	......	63 000	......	.....	.....	0.563	.....	.....
Q-16	"	1200	99 100	69 700	55 000	......	0.703	.....	0.555	.....	.....
Q-16$_2$	"	1200	107 500	......	57 000	......	.....	.....	0.530	.....	.....
Q-12	Oil	900	117 500	77 000	54 500	35 000	0.655	0.642	0.464	0.298	0.455
Q-8	"	950	154 000	96 000	63 500	45 000	0.623	0.709	0.412	0.292	0.469
Q-7	"	1050	155 500	111 400	59 500	48 000	0.703	0.807	0.375	0.303	0.421
Q-13	"	1100	97 600	71 400	48 000	......	0.732	.....	0.492	.....	.....
Q-6	"	1150	115 500	77 890	54 000	37 500	0.674	0.694	0.468	0.325	0.481
Q-3	"	1200	97 500	67 920	47 500	......	0.697	.....	0.487	.....	.....
Q-10	Air	....	91 050	67 300	42 500	......	0.739	.....	0.467	.....	.....
Q-11	Furnace	....	85 500	60 000	41 500	......	0.702	.....	0.485	.....	.....
Q-5$_3$	"	....	90 800	......	50 000	......	.....	.....	0.551	.....	.....
Q-5$_2$	"	....	80 600	61 500	42 000	......	0.763	.....	0.521	.....	.....
Q-5$_1$	"	....	86 450	62 000	43 000	......	0.717	.....	0.497	.....	.....
Q-5	"	....	103 900	......	49 500	28 000	.....	0.566	0.476	0.269	.....
Average of Q Material							0.688	0.684	0.488	0.285	0.458

Table XI.—Endurance Limits for 10,000,000 Cycles; Their Relationship to Tensile Strength and Torsional Strength. Bar Material (*Continued*)

Material Designation	Cooled in	Drawn at — deg. Fahr.	Average Tensile Strength, lb. per sq. in.	Average Torsional Strength, lb. per sq. in.	Endurance Limit for 10,000,000 Cycles, lb. per sq. in.		Ratio of Values				
					Rotating Cantilever	Alternating Torsion	Column 5 to Column 4	Column 7 to Column 6	Column 6 to Column 4	Column 7 to Column 4	Column 7 to Column 5
1	2	3	4	5	6	7	8	9	10	11	12
Xa-2....	Water....	900	121 570	......	68 000	......	.....	.....	0.559	.....	.....
Xa-3....	"	1100	136 630	73 050	68 500	......	0.535	.....	0.501	.....	.....
Xa-1....	Furnace...	....	85 750	......	38 500	......	.....	.....	0.449	.....	.....
Average of Xa Material							0.535	.....	0.503	.....	.....
Xb-2....	Water....	900	164 750	104 150	72 500	......	0.632	.....	0.440	.....	.....
Xb-3....	"	1100	139 880	86 040	63 500	......	0.615	.....	0.454	.....	.....
Xb-1....	Furnace...	....	82 430	62 950	36 500	......	0.764	.....	0.443	.....	.....
Average of Xb Material							0.670	.....	0.446	.....	.....
Xc-2....	Water....	900	172 000	125 510	88 000	......	0.730	.....	0.511	.....	.....
Xc-3....	"	1100	157 000	103 370	77 000	......	0.658	.....	0.490	.....	.....
Xc-1....	Furnace...	....	111 350	76 700	49 500	......	0.689	.....	0.445	.....	.....
Average of Xc Material							0.692	.....	0.482	.....	.....
Y-2.....	Water....	900	201 000	124 080	94 500	......	0.617	.....	0.470	.....	.....
Y-2$_2$.....	"	900	147 740	......	78 500	......	.....	.....	0.531	.....	.....
Y-3.....	"	1100	164 400	110 490	92 000	......	0.672	.....	0.560	.....	.....
Y-1.....	Furnace...	....	101 750	76 980	44 500	......	0.757	.....	0.437	.....	.....
Average of Y Material							0.682	.....	0.500	.....	.....
Z-2.....	Water....	900	164 500	95 290	84 500	......	0.579	.....	0.514	.....	.....
Z-3.....	"	1100	144 250	82 420	72 000	......	0.571	.....	0.499	.....	.....
Z-1.....	Furnace...	....	117 750	78 500	50 000	......	0.667	.....	0.425	.....	.....
Average of Z Material							0.606	.....	0.479	.....	.....

as regards chemical composition, however, the ratios for each composition have been averaged.

In comparing these average ratios it will be seen that the effect of chemical composition on the ratio of tension-compression endurance limit to tensile strength is not negligible. For high-carbon steel the average ratio is about 0.35, while for low and medium-carbon steels it is about 0.41 and for $3\frac{1}{2}$-per-cent nickel steel it is about 0.50. The effect of chemical composition and heat treatment on the relationship of tension-compression endurance limit to tensile strength can best be studied by plotting, as in Fig. 21, the various values of tensile strength as abscissa and the corresponding values of endurance limit as ordinates. If from points thus plotted straight lines are drawn to the origin of coordinates, the tangents of the angles of slope of the lines represent the various ratios that are being studied. For example, in Fig. 21 (*a*), three lines have been drawn through the approximate centers of three groups of points representing respectively ingot iron, mild steel and high-carbon steel. The direction of each line is such that if prolonged it would pass through the origin of coordinates. The slopes of these three lines indicate that the ratio under discussion, which may be called the "tension-compression endurance ratio," is higher for ingot iron than for mild steel, and higher for mild steel than for high-carbon steel.

TABLE XII.—ENDURANCE LIMITS FOR 10,000,000 CYCLES; THEIR RELATIONSHIP TO TENSILE STRENGTH AND TORSIONAL STRENGTH. CRANK-SHAFT AND PROPELLER-SHAFT MATERIAL

Shaft Designation	Specimen		Average Tensile Strength, lb. per sq. in.	Average Torsional Strength, lb. per sq. in.	Endurance Limit for 10,000,000 Cycles, lb. per sq. in.		Ratio of Values				
	Location[1]	Direction			Rotating Cantilever	Alternating Torsion	Column 5 to Column 4	Column 7 to Column 6	Column 6 to Column 4	Column 7 to Column 4	Column 7 to Column 5
1	2	3	4	5	6	7	8	9	10	11	12
ZA...	....	....	140 000	......	63 500	......	.....	.....	0.454	.....	.....
1...	Prolongation...	Longitudinal	103 380	......	45 500	21 500	.....	0.473	0.440	0.208	.....
		Tangential..	99 000	......	......	......	.....	.....	.....	.....	.....
	Block..	Longitudinal	114 410	......	......	24 000	.....	.....	.....	0.210	.....
		Breadth....	109 680	......	......	......	.....	.....	.....	.....	.....
		Thickness...	110 800	......	......	......	.....	.....	.....	.....	.....
2.....	....	Longitudinal	95 000	75 200	44 500	23 500	0.792	0.528	0.468	0.247	0.313
		Tangential..	65 870	67 020	34 500	......	1.017	.....	0.524	.....	.....
		Radial......	93 000	......	......	......	.....	.....	.....	.....	.....
3.....	....	Longitudinal	84 870	62 950	40 500	20 500	0.742	0.506	0.477	0.242	0.326
		Tangential..	85 400	61 680	31 500	......	0.722	.....	0.369	.....	.....
		Radial......	81 810	......	......	......	.....	.....	.....	.....	.....
4...	Prolongation...	Longitudinal	75 940	61 360	35 500	16 500	0.808	0.465	0.467	0.217	0.269
		Tangential..	74 620	......	......	......	.....	.....	.....	.....	.....
		Radial.....	72 620	......	......	......	.....	.....	.....	.....	.....
	Block..	Longitudinal	74 630	......	31 500	16 500	.....	0.523	0.422	0.221	.....
		Breadth....	74 440	......	......	......	.....	.....	.....	.....	.....
		Thickness...	75 750	......	......	......	.....	.....	.....	.....	.....
6...	Prolongation..	Longitudinal	77 310	54 340	30 500	13 500	0.703	0.442	0.394	0.175	0.248
		Tangential.	77 120	......	28 500	......	.....	.....	0.370	.....	.....
		Radial.....	77 180	......	......	......	.....	.....	.....	.....	.....
	Block..	Longitudinal	83 950	......	......	16 500	.....	.....	.....	0.197	.....
		Breadth....	82 500	......	......	......	.....	.....	.....	.....	.....
		Thickness...	76 580	63 950	26 500	......	0.835	.....	0.346	.....	.....
9.....	Web...	Longitudinal	79 810	......	......	14 500	.....	.....	.....	0.182	.....
		Breadth....	79 770	56 150	......	......	0.704	.....	.....	.....	.....
		Thickness...	76 660	60 680	......	......	0.792	.....	.....	.....	.....

[1] "Prolongation" designates material from the test end prolongation of the shaft. "Block" designates material from the crank-throw test block, about midway of the shaft.

The slopes and relative positions of the similarly drawn lines of Fig. 21(*b*) indicate that the endurance ratio for nickel steels is higher than for medium-carbon steels. The distribution of the points on each side of the line representing nickel steels would seem to indicate that for such material the tension-compression endurance ratio is nearly independent of heat treatment.

Through the groups of points in Fig. 21(*d*), only one line has been drawn extending toward the origin of coordinates. The slope

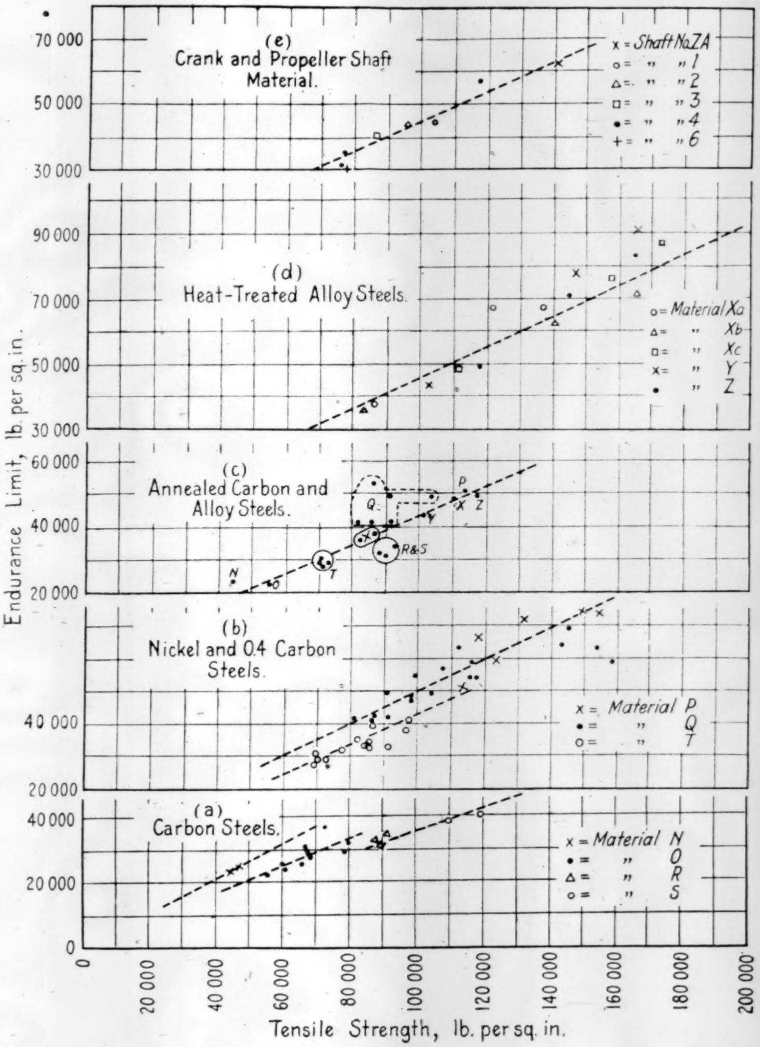

FIG. 21.—Relationship of Tension-Compression Endurance Limit for 10,000,000 Cycles to Tensile Strength

of this line is slightly less than the line in Fig. 21(*b*) representing nickel steels. If for each of the five kinds of material, however, a separate line were drawn, such a line for materials Xa, Xc, Z and Y should apparently be given a slight curvature with upward concavity. This would seem to indicate that when so heat-treated as to produce high tensile strength, there is little difference between the tension-compression endurance ratios of chrome-molybdenum, chrome-vanadium and "low" nickel-chrome steels.

For annealed carbon and alloy steels endurance ratio relationships are illustrated in Fig. 21(*c*). The points representing materials O, T, X, Y and Z are so situated that a straight line starting at the origin of coordinates may be so drawn as to pass through or just above all these points. The points representing ingot iron and high-cabon steel are well above and below this line, respectively. Of the seven points representing 3½-per-cent nickel steels, five are located well above the line and two are above but close to the line. The accumulated evidence presented by this figure, therefore, would seem to indicate that in the annealed condition the endurance ratio of 3½-per-cent nickel steel is higher than that of chrome-molybdenum, chrome-vanadium, low nickel-chrome and all carbon steels except ingot iron. The endurance ratio for annealed high nickel-chrome steel has not yet been determined.

Relationships between endurance ratios of large steel forgings are illustrated in Fig. 21(*e*). With the exception of shafts ZA, all of the material here represented was taken from submarine or battleship shafts. Of these larger shafts, Nos. 1 and 6 were water-quenched and tempered, No. 4 was given accelerated air cooling followed by tempering, Nos. 2 and 3 were probably annealed. Since the effect of heat treatment on such large forgings is much less than in the 1-in. round bars, it would be expected that in Fig. 21(*e*) to some extent as in Fig. 21(*c*) the effect of chemical composition would be noticeable. This is found to be the case. Shafts Nos. 2 and 3 of annealed 3½-per-cent nickel steel show a slightly higher endurance ratio than the shafts of chrome-vanadium or carbon steels and even the low nickel-chrome water-quenched aeroplane shafts.

Relationship of Alternating Torsion Endurance Limit to Tensile and Torsional Strength.—In columns Nos. 11 and 12 of Tables XI and XII are recorded the ratios of alternating torsion endurance limits to tensile and torsional strengths, respectively. Since the torsional properties of a rolled or forged object are not necessarily well indicated by a tension test, the torsional endurance limit should logically be compared with the results of a static torsion rather than with the results of a static tension test. Since a torsion test is rarely made in inspection of material, however, and since the round bars used in this work had all presumably received about the same degree of mechanical work, it seems desirable to compare the torsional endurance limits with results of static tension as well as static torsion tests.

Examination of the results in column No. 11 will show that the ratio of torsional endurance limit to tensile strength is fairly constant for the carbon steels studied and averages about 0.22. For the 3½-per-cent nickel steels, the ratio apparently varies somewhat with heat treatment but averages about 0.28. These relationships are better shown graphically as in Fig. 22(*a*). As there shown, the plotted results for carbon and nickel steels fall on or near two rather widely separated lines. With the exception of one point representing an annealed nickel steel, all the points on or near the lower line represent low, medium or high-carbon steels. All the points on the upper line represent nickel steels. One point between the two lines represents an annealed nickel steel. The upper and lower lines represent respectively values of 0.3 and 0.22 for the ratio of torsional endurance limit to tensile strength.

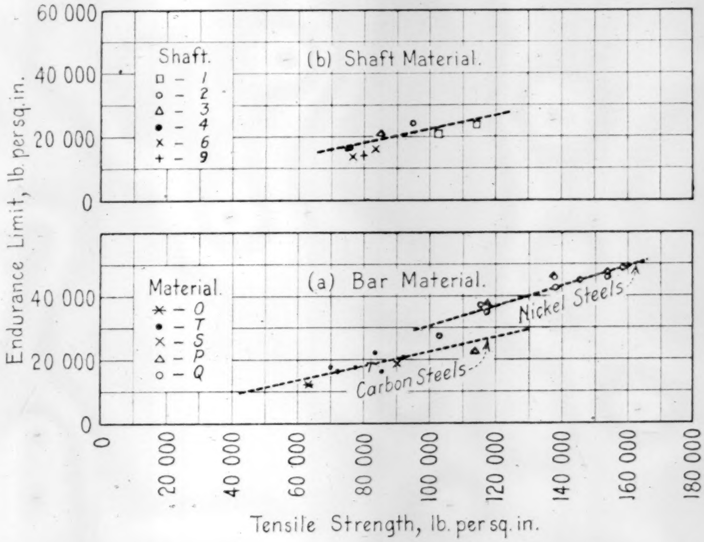

FIG. 22.—Relationship of Alternating Torsion Endurance Limit for 10,000,000 Cycles to Tensile Strength

In Fig. 22(*b*) are illustrated similar relationships for large forgings. The one line drawn in this graph represents a value of 0.22 for the ratio of torsional endurance limit to tensile strength. This is about the same as for carbon-steel bar material. Four lines, however, could have been drawn through the origin of coordinates so as to classify these battleship and submarine shafts in four groups. In order of decreasing ratio of endurance limit to tensile strength represented by them, these four lines would pass through points representing battleship shafts of annealed $3\frac{1}{2}$-per-cent nickel steel, submarine shaft of air-cooled carbon steel containing $1\frac{1}{4}$ per cent manganese, submarine shaft of water-quenched and tempered chrome-vanadium steel, submarine shafts of carbon steel. The influence of chemical composition on this endurance ratio, therefore, appears to be even more important than on the ratio of tension-compression endurance limit to tensile strength as illustrated in Fig. 21(*e*).

As recorded in column No. 12 of Tables XI and XII, the ratio of torsional endurance limit to torsional strength—which may be called the "alternating torsion endurance ratio"—averages about 0.28 for the carbon steels studied and about 0.45 for the nickel steels. As shown graphically in Fig. 23, the two lines representing the torsional endurance ratios for carbon and nickel steels respectively are widely separated, more widely than the corresponding graphs shown in Fig. 22(*a*). The same sample of annealed nickel steel that gave a low point in Fig. 22(*a*) is represented by a low point in Fig. 23. With this exception, the various individual results represented in Fig. 23 follow as closely as could be expected the two widely separated lines.

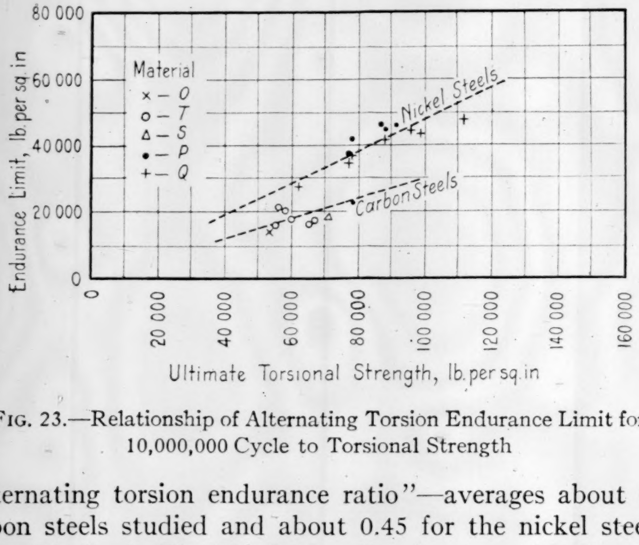

FIG. 23.—Relationship of Alternating Torsion Endurance Limit for 10,000,000 Cycle to Torsional Strength

A possible correction of the same percentage amount for its recorded torsional endurance limits may make necessary a corresponding correction of the torsional endurance ratios. The relationship between the various ratios, however, will not be affected. Until the possibility of this percentage correction has been further investigated no discussion will be given of the relation between the values in columns Nos. 8 and 9 of Tables XI and XII.

Endurance Ratios as Affected by Chemical Composition and Microstructure.—Study of the endurance ratio relationships as shown on Figs. 21 to 23, inclusive, leads to interesting tentative conclusions in regard to the influence of chemical composition and microstructure on the above discussed endurance ratios. The endurance ratio of ingot iron is higher than that of any other pearlitic carbon steel. The ratio decreases with increase in percentage of pearlite. Experiments not here recorded, with annealed steels of about 0.1 per cent carbon, indicate that not only the endurance ratio but even the endurance limit is lowered by the first addition of cementite to ferrite. Evidently these particles of cementite serve as starting points for cracks. With further increase in the quantity of cementite distributed throughout the ferrite, the hard particles hinder somewhat the extension of the cracks which start in the ferrite, and thus raise the endurance limit above that of pure ferrite. The raising of the endurance limit by increase of cementite percentage, however, does not keep pace with the increase of tensile strength.

Addition of nickel to annealed carbon steel apparently strengthens the ferrite with the result that the endurance ratio is raised. This fact would seem to indicate that an investigation of the endurance properties of the carbonless iron-nickel alloys would be of interest. The full effect of nickel on the endurance ratio, however, is obtained only by quenching and tempering.

In the presence of other alloying constituents such as chromium, molybdenum, and vanadium, the endurance ratio can be raised by heat treatment until it approaches that of ingot iron. Even in the presence of these alloying constituents, however, it appears evident that the full effect of quenching treatment in raising the endurance ratio will not be secured unless the carbon percentage is kept above a certain minimum. For example, material Xb containing 0.31 per cent of carbon apparently has a lower endurance ratio in the quenched-and-tempered condition than has material Xa containing 0.40 per cent of carbon, although the former contains more chromium and molybdenum than the latter.

Quenching and tempering treatment applied to low and medium-carbon steels decreases the size of the carbide particles and thus raises the endurance limit. By such treatment of the lower carbon steels the endurance ratio is not greatly increased. With increase in the carbon content, however, quenching and tempering increases the endurance ratio as well as the endurance limit.

The fact that small percentages of a hard constituent such as cementite lower not only the endurance ratio but also the endurance limit, makes it probable that non-metallic inclusions in steel have a decided effect in lowering the endurance ratio. Confirmatory evidence is presented in graphs of Figs. 12 and 18. As shown in these graphs, the endurance limits of specimens taken in a transverse direction are usually considerably lower than those of longitudinal specimens. Since the tensile strength in the two directions usually does not differ greatly, this means that the transverse endurance ratio is considerably lower than the longitudinal. Although it is probable that inferiority of the transverse endurance ratio is due to the unfavorable orientation of inclusions in the transverse specimen, it indicates that inclusions in steel have a noticeable effect and that the effect on the torsional endurance ratio is probably considerable.

Relationship of Endurance Limits to Tensile and Torsional Proportional Limit

In Fig. 24, the various values of the tension-compression endurance limit are plotted against the corresponding values of the tensile proportional limit. In Fig. 25 values of the alternating torsion endurance limit are plotted against the corresponding values of the torsional proportional limits. To represent the relationships thus disclosed, idealized graphs have been drawn. A comparison of these graphs with those of Figs. 21 to 23, inclusive, reveals a striking difference between the two sets. Instead of the straight line relationship so conspicuous in Figs. 21 to 23, a curvilinear relationship is prominent in Figs. 24 and 25. Most of the curved lines leave the origin of coordinates at an angle of about 45 deg. with the horizontal, thus indicating that for the lower values of the proportional limit the endurance limit is nearly equal to the proportional limit. With increase in the value of the proportional limit the ratio of endurance limit to proportional limit decreases. At the highest values of the proportional limit the endurance limit is about half the proportional limit. As shown in Figs. 24(*a*) and 25, the ratio of endurance limit to proportional limit is conspicuously lower in low and medium-carbon steels than in nickel steels of the same strength. This fact is the more noteworthy when it is remembered that the elastic ratio is higher for nickel steels than for carbon steels. For the other alloy steels and for high-carbon steels the ratio is approximately the same as for nickel steel. In Fig. 24(*c*) the only fact of interest is the relatively high ratio of endurance limit to proportional limit found in the two battleship shafts of annealed $3\frac{1}{2}$-per-cent nickel steel.

Relationship of Endurance Limit to Impact Value

A comparison of endurance limit values given in Tables XI and XII with the corresponding Charpy impact values given in Tables VIII and IX shows that there is no definite relationship between the two sets of values. In other words, the endurance limit as determined with a specimen free from abrupt changes of section is independent of the energy required to fracture a notched specimen. Since in many machinery parts, such as a crank shaft, the changes of section are

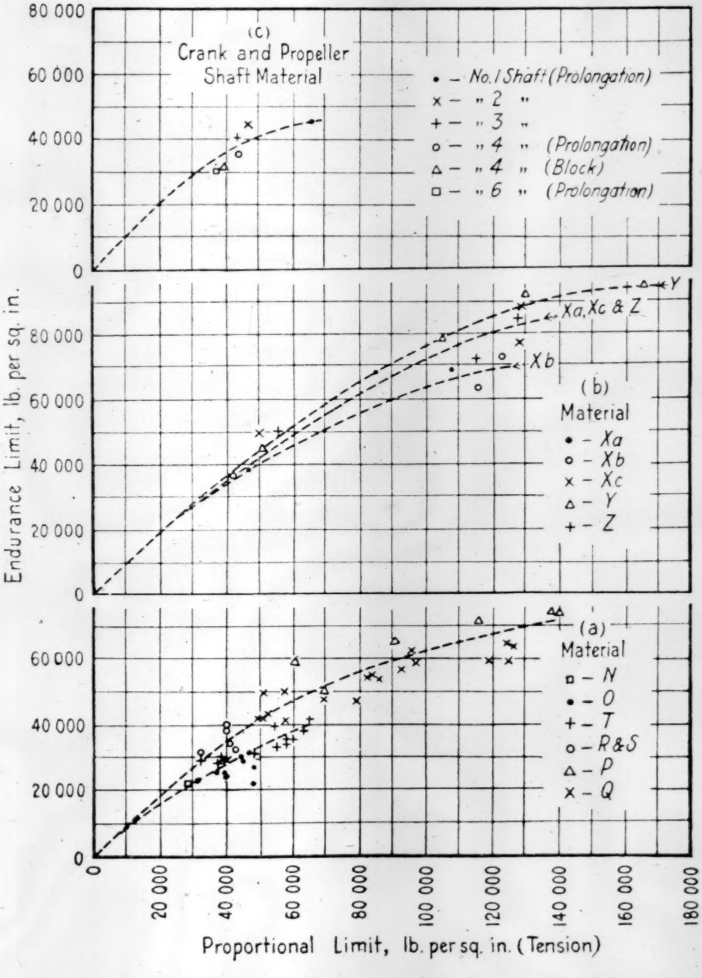

Fig. 24.—Relationship of Tension-Compression Endurance Limit for 10,000,000 Cycles to Tensile Proportional Limit

rather abrupt, the question arose whether or not the endurance limit of such a part is entirely independent of the energy necessary to fracture a notched specimen under impact. Light is thrown on this subject by the impact-endurance tests which are summarized in Figs. 19 and 20 and have been discussed above. The impact-endurance experiments summarized in Fig. 19 showed conclusively that unless the energy of impact is so great as to cause fracture after less than about one million blows, the endurance of a notched bar depends not on the impact properties but on the "endurance limit" of the steel. As shown by results summarized in Fig. 20, notched bars of high-carbon steel having a Charpy value of only 2.9 proved superior in endurance to similar bars of low-carbon steel having a Charpy value of 23.6, provided the energy of hammer blow was not so great as to cause fracture after less than about a million blows. The fatigue-resisting quality of a machinery part, therefore, whether or not it contains abrupt change of section, is independent of the impact value of the metal.

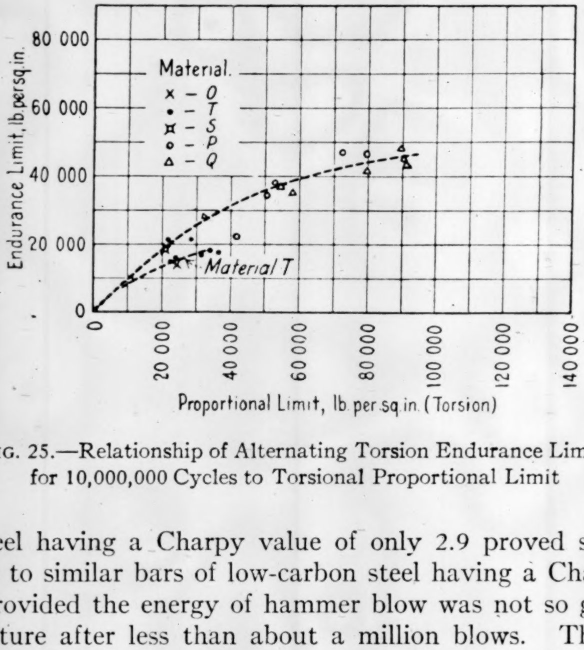

FIG. 25.—Relationship of Alternating Torsion Endurance Limit for 10,000,000 Cycles to Torsional Proportional Limit

These conclusions are apparently contrary to the general opinion that a notch or groove on the surface affects the endurance of a hard material more than that of a soft material. To test the conclusion still further, however, endurance tests have been made on rotating cantilever specimens of types M and N and on alternating torsion specimens of types J and K. With such specimens, tests were made on materials O-1, S-1, Q-5 and Q-7. The nominal endurance limits obtained with these notched specimens, while they were less than ones half the endurance limits obtained with unnotched specimens, type-C and F, were found to be proportional to the endurance limits obtained with unnotched specimens. The endurance quality of a machinery part, therefore, whether or not it contains abrupt changes of section, depends on the endurance limit as determined by either rotating cantilever or alternating torsion test.

THE ENDURANCE RANGE OF STEEL

In the endurance tests above described, the specimens have been subjected to cycles of stress ranging between equal or nearly equal positive and negative values. Under such conditions values for "endurance limit" have been obtained. Many machinery parts, however, are subjected in service to cycles of stress in which complete reversal of stress does not occur. In many such parts the stress cycle may even consist of a range between two positive stress values. It is important, therefore, that data be available from which may be calculated the limiting ranges of stress for various values of the stresses at the ends of the range. The limiting range may be designated the "endurance range."

Many experiments have been made by various investigators to determine the endurance range of steel under various conditions of incomplete reversal of stress. These experiments have led to theoretical conclusions that are of value. In the discussion of their results, however, it appears to the writer that the experimenters have lost sight of the practical conclusions to be drawn and have made their discussions needlessly complex from a practical point of view. This is due to the fact that proper distinction has not been made between fatigue failures and other kinds of failure. A fatigue failure is always a complete or partial fracture. A machinery part may, however, fail without fracture. A crank shaft, for example, if stressed beyond its elastic limit has failed though it may not have fractured. In investigation of the endurance range of machinery parts, therefore, it is not necessary that one of the limits of this range be pushed beyond the elastic limit.

For investigation of the length of the endurance range in various positions within and just beyond the edge of the elastic range, some experiments have been made at the Naval Experiment Station with the previously described alternating torsion machine of the spring type. For these experiments, various steels were heat-treated so as to have proportional limits nearly twice their endurance limits. With such steels it was possible to vary considerably the position of the endurance range within the elastic range. As a result of these experiments, the conclusion has been reached that the variation in the endurance range within the elastic range is slight. Even when the position of the endurance range is such that its stress range oscillates between zero and a positive value, it is apparently not more than about 10 per cent less than when the stress oscillates between equal positive and negative values. These experiments have led to the conclusion, therefore, that within the elastic range, the endurance range may be assumed to be practically twice the endurance limit. A study of the published results of other investigations of this subject, if rightly interpreted, would, it is believed, lead to the same conclusions.

Evidently, therefore, the endurance range is definitely related to the tensile strength rather than to the elastic limit. Nevertheless, the general fatigue-resisting quality of any steel is greatly dependent on its elastic limit. In a steel of low elastic ratio, the full range could be secured only by complete or nearly complete reversal of stress per cycle. In ingot iron the endurance range under rapid alternations of stress even exceeds the elastic range. In a steel of high elastic ratio, however, the position of the endurance range within the elastic range may be greatly varied. When, therefore, a machinery part in service is to be subject to repeated rather than alternating stresses, a high elastic ratio may be of even more importance than a high endurance range. Fortunately, however, a high elastic ratio is usually accompanied by a high endurance range.

SELECTION OF MATERIAL FOR FATIGUE-RESISTANT MACHINERY PARTS

When a machinery part subject to repeated or alternating stress is to be made as light as possible, the proper design of the part involves not only intelligent attention to form and dimensions but also intelligent selection of material. In the proper selection of material, use can be made of the information presented above in regard to the relationship of endurance limit to other physical properties.

For fatigue resistance, the endurance limit should be made as high as possible. This involves making the tensile strength as high as is possible consistent with the ductility required in the machinery part in question. In other words, the ductility should be at the lowest limit consistent with the use to which the material is to be put. Too much stress has in many cases been laid on ductility. In many machinery parts in which an "elongation" of 16 per cent would be ample, it has frequently been the practice to aim at an elongation of 30 or 35 per cent so as to get material with high Charpy value supposedly indicating "shock-resisting" qualities. For machinery parts such as bolts, which even though threaded should be able to withstand slight deformation without fracture, ductility is of much importance and the Charpy test is of some value. To minimize the disastrous effects of collisions or other accidents, it may be desirable that certain machinery parts have enough ductility to bend somewhat without fracture. Even in such parts, however, the quality of first importance is resistance to fatigue failure, which is always a fracture in which the most ductile material behaves as if it were brittle. The value of quenching and tempering in raising not only the tensile strength but the endurance ratio has already been emphasized. The higher combination of "endurance ratio" with ductility obtainable with alloy steels justifies their use in machinery in which a high endurance limit is of importance.

When a forging cannot be quenched and tempered or when the effect of heat treatment is greatly limited, as in the case of large forgings, chemical composition becomes of even greater importance. The effect of an alloying constituent such as nickel in raising the endurance ratio is worthy of consideration. The use of higher carbon percentages in large forgings in order to raise the tensile strength at the expense of the frequently excessive ductility would in many cases give material more suitable for the uses to which it is to be put.

When the stresses in service are to be repeated rather than alternating it is desirable, in order to secure the necessary high elastic ratio, that quenched-and-tempered steel be used. For the same reason, as well as on account of its high endurance ratio, the use of alloy steel is especially desirable. For large forgings or whenever an effective quenching treatment cannot be given, nickel steel, for the same reason, is especially useful.

There is no doubt that the information obtained within the last few years on the endurance properties of steel will lead to many changes in selection of material on the principles above outlined.

Acknowledgment.—The author acknowledges with thanks the encouragement given in this work by Captain John Halligan, U. S. N., Officer in Charge, Naval Engineering Experiment Station; also the assistance of Messrs. Snader, Wood and Basil and others.

[For a discussion of this paper, see page 122.—Ed.]

RESISTANCE OF MANGANESE BRONZE, DURALUMIN, AND ELECTRON METAL TO ALTERNATING STRESSES

By R. R. Moore[1]

In machines and structures embodying rapidly rotating or reciprocating members, or which are subject to considerable vibration, failures are found that do not appear to be directly due to stresses above the breaking strength of the material. These failures have been attributed to a phenomena commonly called "fatigue" of metals but which, in the light of recent research, is probably better designated as "progressive failure" of metals. The particular purpose of investigations on the fatigue of metals is to establish the endurance limit of the metals under certain fixed conditions, that is, the stress to which the metal may be subjected an indefinite number of times without failing.

While considerable work on the phenomena of fatigue of metals as pertaining to ferrous metals has been done in recent years, there is an evident lack of adequate information on this subject as related to non-ferrous metals, and especially on the matter of long-time tests. The Engineering Division of the U. S. Air Service, under whose supervision considerable experimental work on aircraft materials has been done, is conducting an investigation to determine the endurance limit of some of the non-ferrous metals, and the results of some of these tests are given in this paper. The tests described herein were made in the Physical Testing Branch of the Engineering Division at McCook Field, Dayton, Ohio.

MATERIALS

The materials tested were rolled duralumin in the annealed, tempered, and "as received" conditions; rolled electron metal; and cast manganese bronze. The chemical analyses of these materials are given in the following table:

	Duralumin	Electron Metal	Manganese Bronze
Copper, per cent	3.25	0.41	56.85
Iron, "	0.28	0.25	1.50
Silicon, "	0.28	0.24	....
Magnesium, "	0.70	Difference	....

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[1] Chief, Physical Testing Branch, Engineering Division, U. S. Air Service, McCook Field, Dayton, Ohio.

	DURALUMIN	ELECTRON METAL	MANGANESE BRONZE
Zinc, per cent	....	4.38	Difference
Manganese, "	Trace	....	0.20
Aluminum, "	Difference	Trace	0.23
Tin, "	....	....	0.32

Duralumin specimens for the alternating stress tests were taken from ½-in. diameter rolled bars. Specimens for other tests to determine physical properties were taken from ¾ and 1-in. diameter rolled bars. Duralumin was tested in three conditions, namely, "as received," tempered, and annealed. The "as received" bars are really tempered stock, and according to the Manufacturers' Handbook were treated as follows: Heated to 930 to 950° F. for from 7 to 30 minutes, and quenched in boiling water. The tempered specimens were made from "as received" bars, tempered as follows in the McCook Field Metallurgical Laboratory: Heated to 925° F. in an electric furnace, held there for one-half hour, and then quenched in boiling water for two hours. Tests were made about one week after heat treatment. The annealed specimens were made from "as received" bars treated as follows in the above mentioned laboratory: Heated to 700° F. in an electric furnace, held there 20 minutes, and then allowed to cool with the furnace.

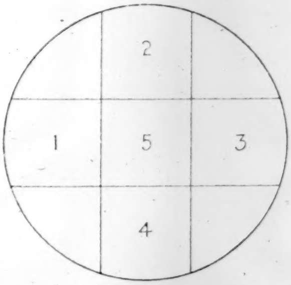


FIG. 1

Electron metal specimens were cut from 2-in. diameter rolled bars received by the Air Service from Frankfort-on-Main, Germany. The method of taking alternating stress specimens from these bars is shown in Fig. 1.

The manganese-bronze specimens were made from test bars cast in the McCook Field foundry under the supervision of Mr. E. H. Dix, Jr. The pouring temperature of this melt was 1760° F., which was also the maximum furnace temperature. The time in the furnace was 2 hours and 10 minutes. The method of casting the alternating stress test bars is shown in Fig. 2. The risers for the other test bars have the same cross-sectional dimensions as shown in Fig. 2, except for the 8-in. gage length tension specimens and the torsion specimens, in which cases the width of the top of the riser is 3 in. instead of $2\frac{3}{16}$ in. The sizes of the test bars from which specimens were taken are as follows:

Tension Specimens: Bars $\frac{5}{8}$ in. in diameter over the gage length with $\frac{7}{8}$-in. ends.
Compression and Shear Specimens: Bars $\frac{7}{8}$ in. in diameter by 13 in. long.

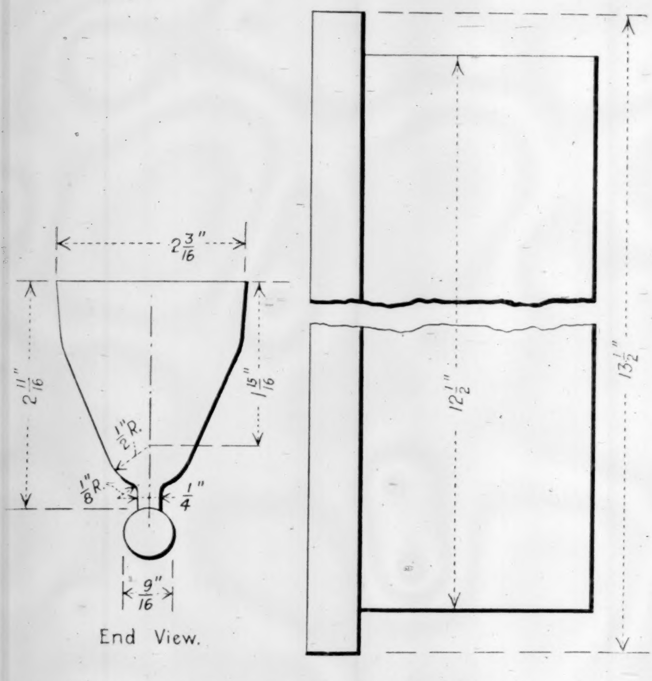


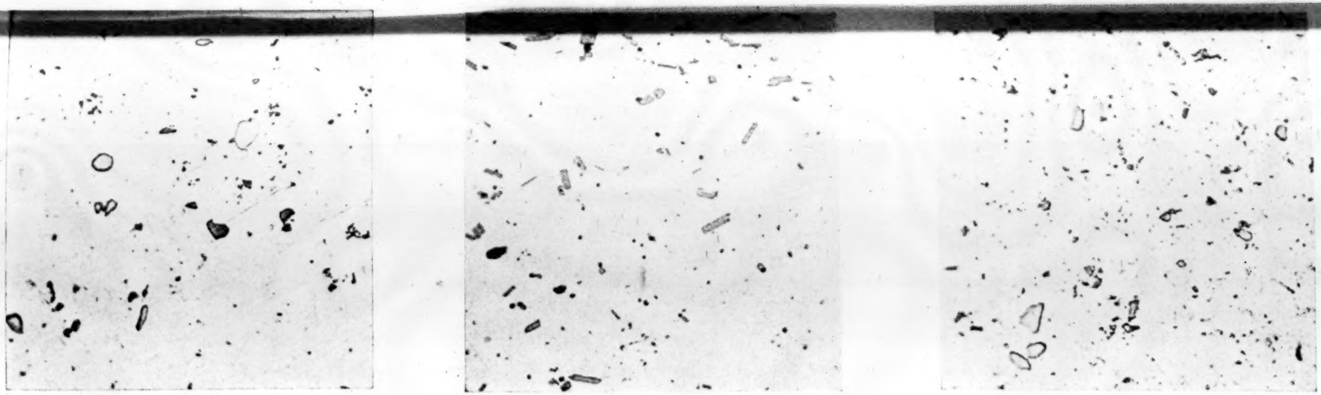
FIG. 2.—Method of Casting Test Bar for Alternating Stress Specimen

Torsion Specimens: Bars $\frac{7}{8}$ in. in diameter over the gage length with $1\frac{1}{8}$-in. ends.
Impact Specimens: Bars $\frac{3}{4}$ in. square by $9\frac{3}{4}$ in. long.

Micrographs of cross-sections of test specimens of each material are shown in Fig. 3.

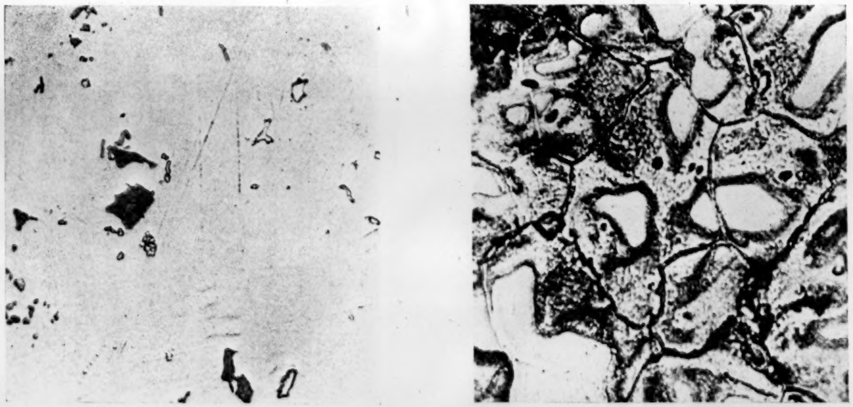
GENERAL TESTS FOR PHYSICAL PROPERTIES

In order to determine the general physical properties of the metals under investigation, the following tests were made: tension, compression, torsion, shear, impact (Izod and Charpy notched bar), hardness (scleroscope, Rockwell and Brinell) and specific gravity.



"As Received' Tempered Annealed

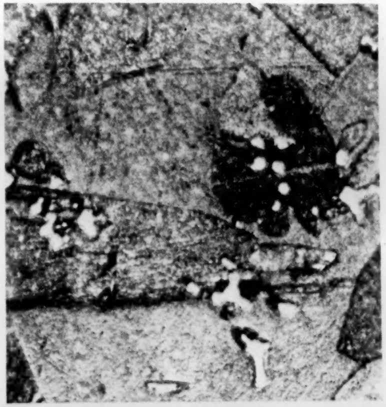
DURALUMIN. Unetched.



Etching: None. Dark particles have bluish tinge under visual examination; other particles are clear with black boundaries.

Etching: 10 per cent Aqueous HNO_3 Solution. Cored matrix and grain boundaries revealed by etching.

ELECTRON METAL



Etching: $NH_4OH + H_2O_2$ Alpha mottled and delta prime clear white.

MANGANESE BRONZE

FIG. 3.—Micrographs of Duralumin, Manganese Bronze and Electron Metal. × 500

Test Specimens.—Tension tests were made on two types of specimens: The standard A.S.T.M. $\frac{1}{2}$-in. diameter, 2-in. gage length specimen with threaded ends, and a $\frac{3}{4}$-in. diameter, 8-in. gage length specimen with threaded ends which was used for obtaining the modulus of elasticity, elastic limit, and proportional limit.

Compression tests were made on a specimen of $\frac{5}{8}$-in. gage diameter and 2-in. gage length with ends enlarged to $\frac{3}{4}$ in.

Torsion tests were made on a specimen $\frac{5}{8}$ in. in diameter with 10-in. gage length.

TABLE I.—GENERAL PHYSICAL PROPERTIES

Each value given is the average of two tests except as specified.

	Duralumin			Electron Metal	Manganese Bronze
	As Received	Tempered	Annealed		
TENSION					
Elastic Limit, lb. per sq. in.	22 500	16 880	6 230	5 150	10 240
Proportional Limit, lb. per sq. in.	25 030	18 550	6 800	6 260	13 000
Tensile Strength, lb. per sq. in.	50 960	51 170	25 250	36 500	70 000
Elongation in 2 in., per cent	16.0	29.3	25.0	17.5	32.8
Reduction of Area, per cent	50.4	47.5	60.8	20.5	40.8
Modulus of Elasticity, lb. per sq. in.	10 134 000	10 950 000	10 737 000	6 135 000	14 235 000
COMPRESSION					
Proportional Limit, lb. per sq. in.	20 860	17 270	6 860	8 150	15 500[a]
Compressive Strength, lb. per sq. in.	50 060	50 090	26 620	47 780	42 390[a]
SHEAR					
Ultimate Strength, lb. per sq. in.	29 120	26 580	16 240	16 970	43 190
TORSION					
Proportional Limit, lb. per sq. in.	18 970	14 090	7 820	3 930	11 140
Modulus of Rupture, lb. per sq. in.	43 260	47 200	27 080	24 630	61 890
Modulus of Rigidity, lb. per sq. in.	3 838 000	3 838 000	3 838 000	2 161 600	4 619 000
HARDNESS					
Scleroscope, Magnifying Hammer	19	22	12	19	17
Brinell, 500 kg.	100	100	50	64	93
Rockwell, $\frac{1}{8}$-in. ball	53	63	..	33	65
NOTCHED BAR IMPACT[1]					
Charpy Round Notch, ft-lb.	13.9	19.4	17.3	2.9	21.0
Charpy "V"-Notch, "	....	....	....	5.5	34.4
Izod Round Notch, "	....	....	....	3.0	18.4
Izod "V"-Notch, "	....	....	....	5.0	29.4
SPECIFIC GRAVITY	2.80	2.80	2.80	1.78	8.26

[a] Average of six tests.

[1] Each value is the average of four tests.

Shear tests were made on a specimen $\frac{1}{2}$ in. in diameter and 2 in. long. The test was made in double shear.

Izod and Charpy tests were each made with two different types of notches, the round and the "V" notch. The specimens were 0.394 in. (10 mm.) square in cross-section.

Methods of Test.—Stress-strain curves were used to obtain the proportional limit and modulus of elasticity in tension, the proportional limit in compression, and proportional limit and modulus of rigidity in torsion. The proportional limit throughout this work is designated as the unit stress at which the strain ceases to be proportional to the stress, and in every case was selected as the point of tangency of the straight line with the curve as near as was practical to do so. Special tests were made on the 8-in. gage length tension specimens to determine the true elastic limit of the material, that is, the highest unit stress which does not produce a permanent set. This point was obtained by applying and removing increasing loads until a load was reached which produced a permanent set in the test bar.

Results of Tests.—The results of these tests are given in Table I.

Electron metal is shown to have a very low elastic limit, modulus of elasticity, and tensile strength. On a weight basis, however, it compares favorably with duralumin in tensile strength and modulus of elasticity, but not in elastic limit. The micrographs shown in Fig. 3 indicate that the cast structure was not entirely broken up in rolling the bars, which may account to some extent for the low elastic limit.

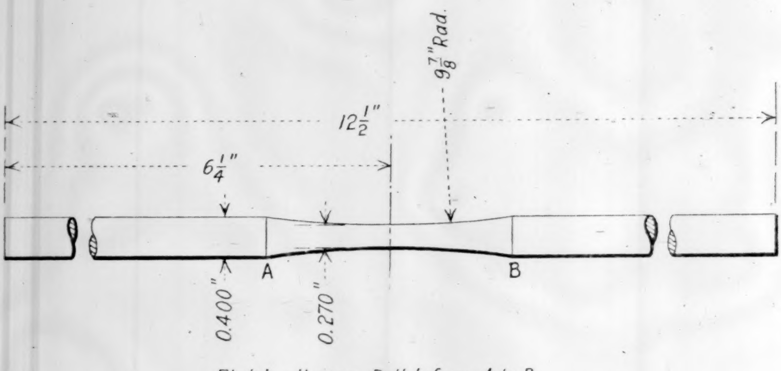
Manganese bronze has a very low elastic limit in proportion to its tensile strength. The compressive strength was also found to be very low, and as this was unexpected, check tests were run, and the value given is the average of six tests. The true compressive strength of a material is rather difficult to determine because the test result is influenced very greatly by the form of the test specimen. The results obtained, however, are at least comparable with the compressive strengths of the other materials of this investigation, for the same type of specimen was used throughout.

The effect of tempering the "as received" duralumin bars was to lower the elastic limit and to increase the hardness, ductility and notched bar impact values. It must be remembered here that these tempered specimens have really received a double treatment for they were already tempered by the manufacturer after rolling. The specimens tempered in this laboratory were quenched at 925° F., whereas it is possible that the manufacturer went as high as 950° F., for this is not outside the limits specified. This difference in quenching temperature, and the fact that the rods were probably given a straightening pass after heat treatment, may account for the difference in elastic limits of the "as received" and tempered materials.

ALTERNATING STRESS TESTS

Testing Machine.—The alternating stresses produced in the specimens under test were reversals of stress of equal magnitude from tension to compression. The machine used was the two-point symmetrically loaded rotating beam type. The particular model of this machine (see illustration, Fig. 8) is very similar to the one described by H. F.

Moore.[1] The machines were mounted on concrete bases in units of four. In adapting this machine to tests of non-ferrous metals, several changes were made. The stresses desired to be produced in the specimens ranged from 11,000 to 30,000 lb. per sq. in., which is much less than would be required in the case of steel specimens. Such a range of stresses requires loads varying from 5 to 15 lb. at each of the load points. In the original model of this machine, the steel hangers through which the loads are transmitted to the revolving specimen weigh about $4\frac{1}{2}$ lb. complete, including the ball bearing and its housing, the yoke, the hanger arm, and the necessary bolts and nuts. It is evident that in the case of the low stresses the weight of these hangers constitutes a very large part of the required load. This is clearly not a good condition when it is desired to eliminate the effect of vibration.



Finish all over; Polish from A to B.

Fig. 4.—Alternating Stress Specimen for Rotating Beam Machine

The ideal construction would be to have the entire required load applied through some shock-absorbing medium, such as a spring or rubber shock absorbing cord. In order to overcome this disadvantage, several changes were made: the ball race housing, yokes, and hanger arms were constructed of duralumin; the design of the hanger arms was modified to reduce the weight; and, in place of the standard bolts and nuts at the top and bottom of the yokes, short pins with projections were used and these were held in place by light spring slips. The weight of the new hanger complete is less than $2\frac{1}{8}$ lb. Bags of shot attached to the hanger arms by $\frac{1}{4}$-in. rubber shock absorber cords supplied the necessary additional weight. The use of the spring clips and pins is a decided advantage in assembling.

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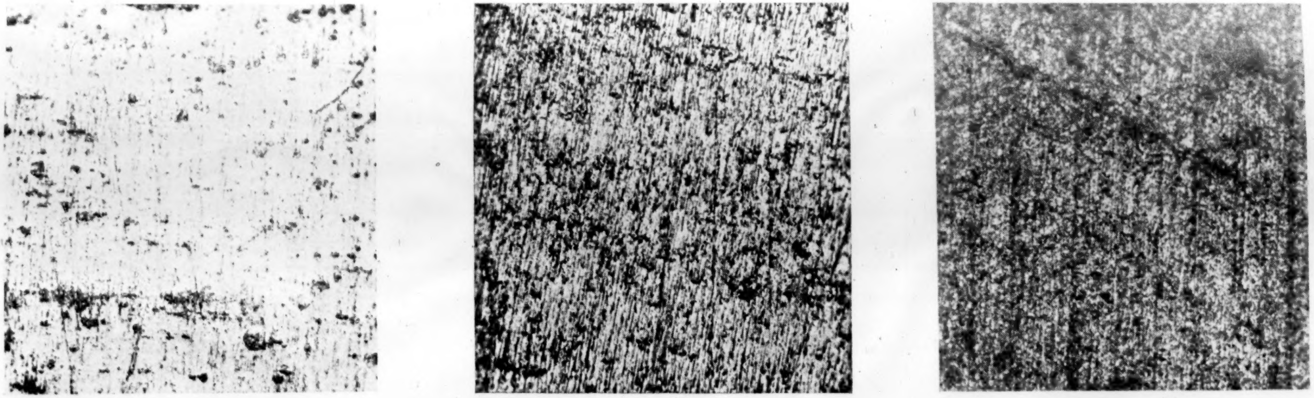
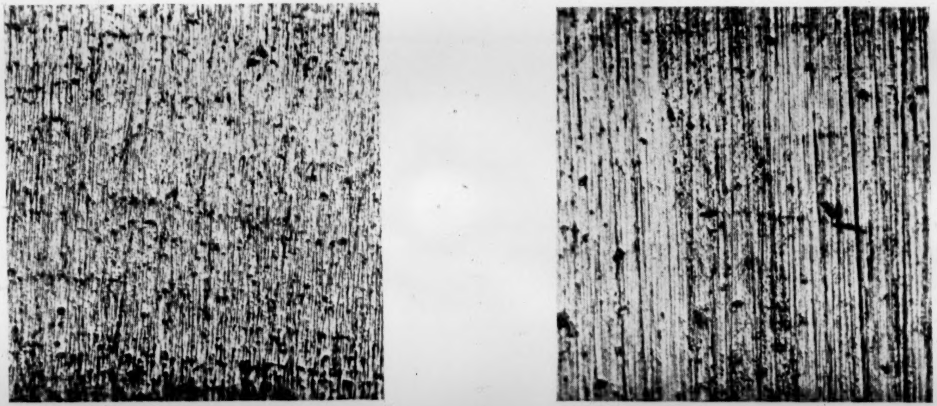
[1] *Bulletin No. 124*, University of Illinois Engineering Experiment Station.



"As Received" Duralumin

Tempered Duralumin

Annealed Duralumin



Electron Metal

Manganese Bronze

FIG. 5.—Micrographs of Surface Finish of Polished Section of Alternating Stress Test Specimens. × 100

In setting up the machine, it was found to be very difficult not to bend the soft annealed duralumin and the electron metal specimens when tightening the nuts on the collets. By using a special jig which supported the weight of the ball bearings and gripped one end of the collet so that it was only necessary to use one wrench, much better results were obtained and at a great saving of time.

Preparation of Specimen.—The specimen used is shown in Fig. 4. In order to eliminate as far as possible the effect of tool marks or scratches, all specimens were polished as fine as possible at the reduced section. Polishing was done by applying to the rotating specimen several grades of Manning paper (in order, Nos. 2, 1, 0, 00, 000), No. 65 alundum powder with water on kitten-ear polishing cloth and levigated alumina with water on the same cloth. The paper and polishing cloth were made into polishing pads by forming over the surface of a light stick shaped to the curvature of the reduced section of the specimen. As the specimen rotated, the pad was moved back and forth longitudinally along the specimen. The whole process of polishing a specimen took from 30 to 45 minutes. The manganese-bronze specimens seemed to be the most difficult to polish.

In order to record permanently the surface finish of the specimens, micrographs of the polished surfaces were taken at 100 magnifications and are shown in Fig. 5.

Specimens of electron metal and annealed duralumin are very soft and easily marred by the ball faces of a micrometer if not carefully applied. For this reason, no attempt was made to measure the diameter of the specimen accurately after the machining operation. The diameter of the specimen was measured after the final polishing by means of a Brinell meter of the traveling microscope type which read to 0.01 mm.

Speed of Tests.—All tests were run at a speed of 1500 r.p.m.

Results of Tests.—The results of the alternating stress tests are plotted graphically in Figs. 6 and 7.

It has been found that for steels the graph obtained by plotting the results of tests to either logarithmic or semi-lograithmic coordinates takes the form of a sloping straight line or possibly a shallow curve convex downward which breaks rather suddenly and then runs more or less parallel with the axis representing the number of repetitions of stress. This tendency to become parallel to the repetition-of-stress axis leads us to believe that a stress has been reached at which the material will stand up indefinitely. Whether the graph becomes truly parallel or assumes a very gradual slope has not as yet been determined. It is clear then that the location of the beginning of this apparently parallel section of the graph is an important point. If it were possible to determine at what number of repetitions of stress the endurance curve of any class of materials breaks and tends to become parallel to this axis, then the maximum number of repetitions of stress necessary to run a specimen in order to determine the endurance limit of any material in this class would be known. H. F. Moore[1] has shown in his work at the University of Illinois that the "break point" of the endurance curves for steels usually occurs below 10,000,000 reversals of stress. While tests at or just below the stress of the "break point" were not run indefinitely, some tests were run out to 100,000,000 reversals of stress with no signs of failure. In one case,[2] a test was run to 1,000,000,000 reversals of stress with no failure and in another to 948,019,300, at which point it failed. This is very good evidence that the graph becomes parallel to the repetition axis or very nearly so in the case of steels, and that a specimen withstanding 10,000,000 reversals of stress will also withstand a great many more. It would probably run indefinitely if a perfect test and specimen were obtained.

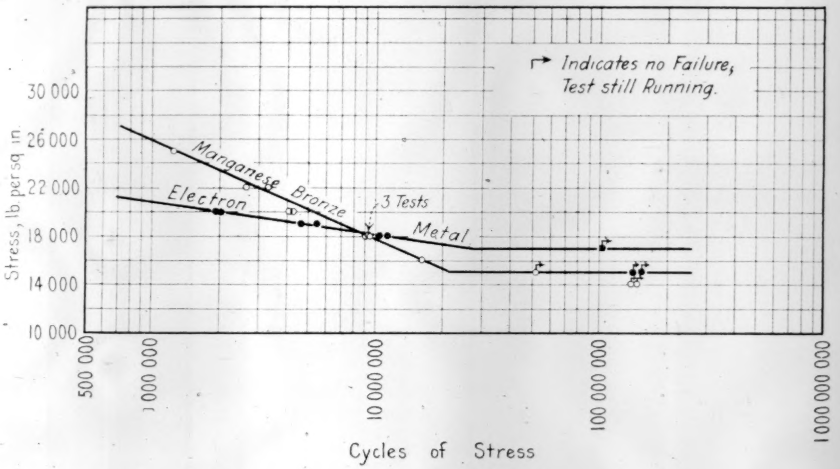


FIG. 6.—Alternating Stress Tests on Bronze and Electron Metal. Semi-Logarithmic Plot

The question naturally arises as to whether this condition holds for non-ferrous metals. Fig. 6, which shows the results of the tests on manganese bronze and electron metal plotted to semi-logarithmic coordinates, indicates that the "break point" for manganese bronze occurs at about 20,000,000 reversals of stress and for electron metal at about 28,000,000. The endurance limit for manganese bronze might be taken at about 15,000 lb. per sq. in. and for electron metal at about 17,000 lb. per sq. in. It is possible, however, that these values could be raised somewhat if long-time tests were run at stresses between the tests now running and the lowest stress at which failures occurred. On the other hand, if after running several hundred million reversals of stress the graph should slope off again, the endurance limit would have to be readjusted. The values given above may be said

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[1] *Bulletin No. 124*, University of Illinois Engineering Experiment Station.

[2] *Bulletin No. 136*, University of Illinois Engineering Experiment Station.

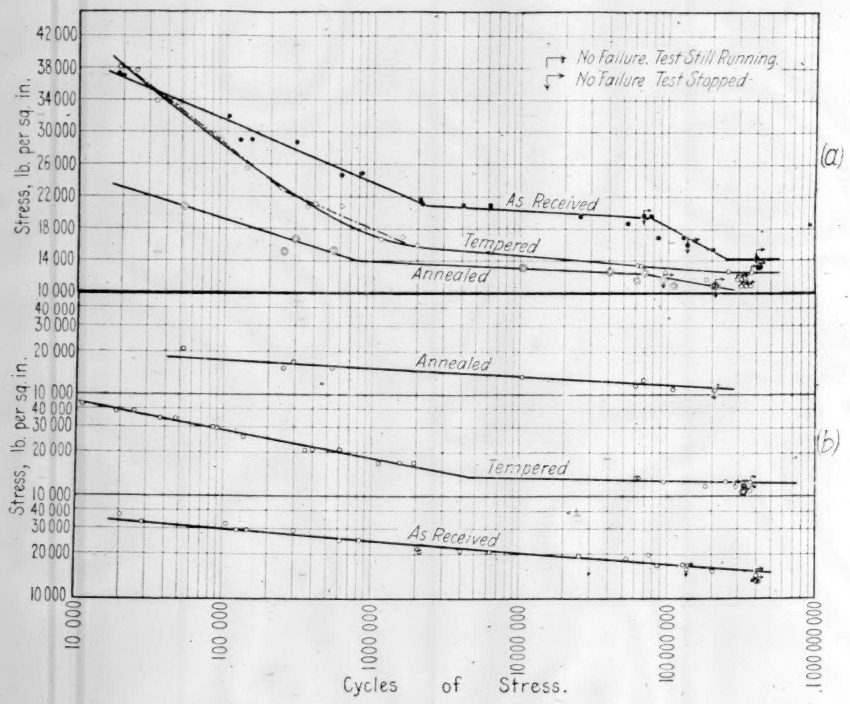

FIG. 7.—Alternating Stress Tests on Duralumin
(*a*) Semi-Logarithmic Coordinates. (*b*) Logarithmic Coordinates

to be temporary pending further tests. It is interesting to note, however, that the manganese bronze seems to resist failure better under the heavy stresses than the electron metal does, but under low stresses does not seem to be as good as electron metal.

Fig. 7 (*a*) shows the results of the tests on duralumin plotted to semi-logarithmic coordinates. The graph for the "as received" material has several "break points." The first occurs at about 2,300,000 reversals of stress. From here on the slope is very slight until about 76,000,000 reversals are reached, at which point another break occurs and the graph turns down very sharply. The last point on the graph at which actual failure occurred is at 200,000,000 reversals of stress. There are several other tests, but none of them failed. A test at a stress of 14,200 lb. per sq. in. stood 400,786,500 reversals and was then taken out. Two tests at 13,000 lb. per sq. in. are still running—one has made 420,000,000 reversals and the other 401,000,000 reversals. It is difficult to select a true endurance limit from this graph, but we can get a good working value. A stress of 14,000 lb. per sq. in. should stand 400,000,000 reversals of stress. As this is a great many more reversals of stress than the material will ordinarily be called upon to withstand, this value can be used as a temporary endurance limit. For less severe working conditions higher values would be amply safe; for example, a stress of 16,000 lb. per sq. in. should resist between 100,000,000 and 200,000,000 reversals of stress.

The first part of the graph for the tempered material may be plotted either as a broken straight line or as a curve. In either case, a break point occurs at about 2,000,000 reversals of stress. From this point on, the graph is straight. The last test giving actual failure is at a stress of 12,530 lb. per sq. in. and gave 253,295,000 reversals of stress. Another test at the same stress has already run 368,000,000 and is still running. There are three tests at 11,690 lb. per sq. in. which have run 292,000,000, 324,000,000, and 336,000,000 repetitions, respectively. Two tests at 10,750 lb. per sq. in. have run 326,000,000 and 330,000,000, respectively. The endurance limit selected from this curve would be about 12,000 lb. per sq. in.

The graph for the annealed material shows a break point at about 800,000 reversals. From here it continues straight to about 70,000,000 reversals, at which point there is another break. The last two points on the curve are at a stress of 10,860 lb. per sq. in.; one ran to 204,000,000 and failed, the other ran to 203,200,000 and was taken out without failing. The endurance limit is taken at 10,860 lb. per sq. in.

The duralumin graphs indicate quite clearly that their endurance limits cannot be obtained by tests at 10,000,000 reversals of stress. They also seem to show that there is a double break in the graph. These graphs were re-plotted to logarithmic scales on both axes as shown in Fig. 7 (*b*). The result of this method of plotting is to straighten out the graph to a single line in the case of the "as received" and annealed tests, but there is still a break in the tempered tests at about 4,500,000 repetitions. The double break or curve of the first part of the tempered graph is straightened by this method of plotting. The final endurance limits, however, remain the same. It will be noticed that the points lie very close to the straight line, due to the shortening of the vertical scale. This seems to be a disadvantage, for eccentricities in the tests are not easily recognized.

RELATION OF ENDURANCE LIMITS TO OTHER PHYSICAL PROPERTIES

The relation of the endurance limits found to the other physical properties is given in Table II. It is evident that the endurance limit does not bear a constant relation to any of the properties determined in this investigation for all the materials tested. This is especially interesting in the face of the fact that it has been pretty well established that the endurance limit for steels varies with the tensile strength and Brinell hardness. There is, however, a good relation between endurance limit and compressive strength for the manganese bronze, electron metal, and annealed duralumin. It will be remembered that the compressive strength of the bronze was lower than its tensile strength, and that of the electron higher. The tempered and "as received" duralumin show a constant relation with tensile strength, but the ratio of endurance limit to tensile strength is different for the other three metals. The relation between endurance limit and tensile strength for the three duralumins is the same as their relation between endurance limit and compressive strength, the tensile and compressive strengths being the same.

TABLE II.—RELATION OF ENDURANCE LIMITS TO OTHER PHYSICAL PROPERTIES

	Duralumin			Elektron Metal	Manganese Bronze
	As Received	Tempered	Annealed		
Endurance Limit to Elastic Limit in Tension	0.62	0.71	1.74	3.3	1.5
Endurance Limit to Proportional Limit in Tension	0.56	0.65	1.6	2.7	1.2
Endurance Limit to Tensile Strength	0.27	0.23	0.43	0.47	0.21
Endurance Limit to Compressive Strength	0.28	0.24	0.41	0.36	0.35
Endurance Limit to Shear Strength	0.48	0.74	0.41	1.00	0.35
Endurance Limit to Proportional Limit in Torsion	0.74	0.85	1.39	4.33	1.35
Endurance Limit to Modulus of Rupture in Torsion	0.32	0.25	0.40	0.69	0.24
Endurance Limit to Charpy Impact, Round Notch × 1000	1.01	0.62	0.63	5.86	0.71
Endurance Limit to Scleroscope × 1000	0.74	0.55	0.91	0.89	0.88
Endurance Limit to Brinell × 1000	0.14	0.12	0.21	0.27	0.16
Endurance Limit to Rockwell × 1000	0.26	0.19	....	0.51	0.23

STUDY OF VIBRATION OF SPECIMENS

The test results just discussed were made on specimens which showed no vibration or in which the vibration was extremely small. Some of the other specimens were bent in setting up, because of the small center section and the softness of the material. Such specimens did not run smoothly, but showed more or less vibration. As it was not known to what extent vibration affected test results, it was decided to record the relative vibration of the different specimens. It was found very difficult to distinguish between different degrees of vibration with the naked eye so that a mechanical method of recording vibration was adopted. The Brinell microscope which was previously mentioned as being used to measure the diameter of the polished specimen was set up so as to travel vertically, and then mounted on a shelf in front of the testing machine, as shown in Fig. 8. This whole set-up is very simple and can be moved from one machine to another in several minutes. With this arrangement, the amplitude of the vibration of the center of the specimen could be measured by focusing the telescope on a high light on the specimen produced by an electric light placed behind it. By rotating the specimen slowly at first, the eccentricity of the specimen could be checked up.

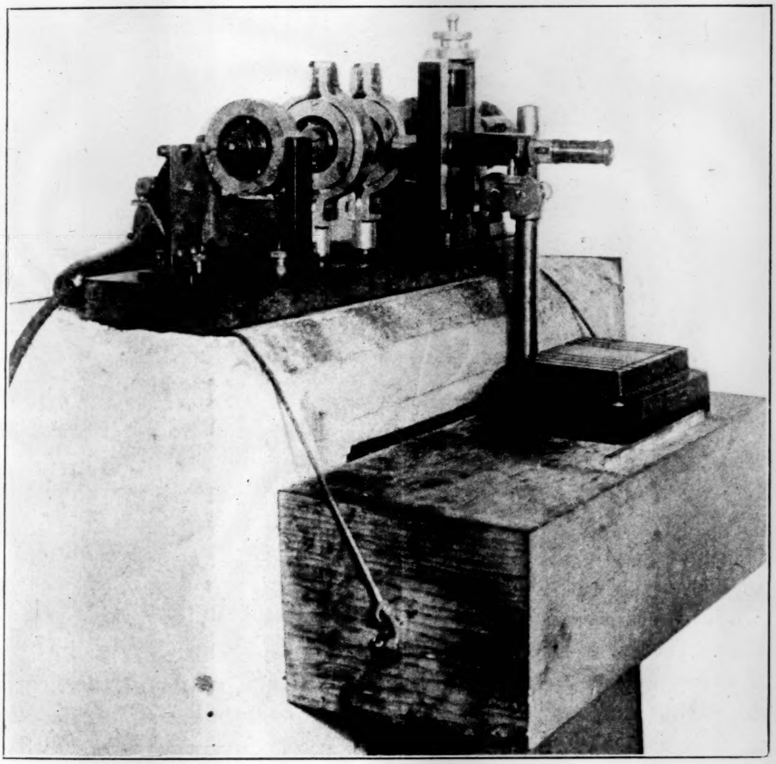

FIG. 8.—Apparatus for Measuring the Vibration of Test Specimens

When the specimen was speeded up, the high light formed a broad band if vibrating badly. The amplitude of vibration was then obtained by measuring the width of this band and subtracting from it the original width of the high light. As the microscope measured to 0.01 mm., it is evident that very small degrees of eccentricity or vibration could be detected. It was found that specimens which appeared to be running smoothly when observed by the naked eye at a distance of a couple of feet from the specimen were actually vibrating a measurable amount. It was also found that specimens did not vibrate regularly, but had periods of small and large amplitudes of vibration.

The results of the observations on the effect of vibration are difficult to express quantitatively as no definite line of procedure was followed. The idea at first was to pick out the badly vibrating tests, and use the measurements of amplitude of vibration as adding more or less weight to the consideration which these tests would receive in plotting the curve. It was found, incidentally, that all tests showing an amplitude of less than 0.30 mm. (0.012 in.) were not effected by vibration. There were cases, however, where specimens showing greater vibration than this gave good results. One of the most important points brought to light is that on tests at or just below the endurance limit, the specimens withstand considerable vibration. For example, one of the electron metal specimens at 15,000 lb. per sq. in. has an amplitude of 1.17 mm., but has withstood 140,000,000 reversals to date; the other is running very smoothly. Also one of the tempered duralumin specimens at 10,750 lb. per sq. in. has an amplitude of 1.04 mm., but has withstood 326,000,000 reversals to date.

Again, by observing the specimen through the microscope when starting a test, it was clearly shown that in speeding up to 1500 r.p.m. the specimen passes through the first critical speed and then settles down, so that it is safe to say that these tests were not run at a critical speed, but were run somewhere between the first and second critical speeds.

The type of alternating stress machine used in these tests, while satisfactory for tests on steel specimens, is not well adapted to light alloy specimens. A machine has now been constructed which proves very satisfactory for this class of work. The same type of specimen is used, the only changes being in minor details. It is also adaptable to testing steels. Its main advantages are:

1. The assembling of the machine does not require the use of a wrench, or even more than light finger pressure at any step in the process.

2. It runs very silently.
3. There is perfect allignment with minimum opportunity for wear.
4. The load bearings and hangers are very light.

Acknowledgment.—The writer wishes to acknowledge the valuable assistance of Mr. S. W. Thompson in running the tests and Mr. C. F. McMahon for the polishing of specimens and micrographic work.

SUMMARY

1. The endurance limits of the metals tested are as follows:

Manganese Bronze	15 000	lb. per sq. in.
Electron Metal	17 000	" "
Duralumin "As Received"	14 000	" "
Duralumin "As Received," tempered at 925° F.	12 000	" "
Duralumin "As Received," annealed at 700° F.	10 860	" "

2. The endurance limits of the non-ferrous metals tested cannot be obtained from tests at 10,000,000 reversals of stress or less, as is true for steels. In some cases it may be necessary to go as high as 200,000,000 reversals of stress, as was found for duralumin.

3. The endurance limits of the non-ferrous metals tested, in so far as they were investigated here, do not bear any definite relation to other physical properties.

4. A fatigue specimen of the rotating beam type will stand considerable vibration when stressed at or below the endurance limit.

5. The speed of 1500 r.p.m. is above the first critical speed for the metals tested and at the loads used.

6. The semi-logarithmic method of plotting seems to show up variations better than the logarithmic method.

DISCUSSION ON ENDURANCE TESTING OF METALS

Mr. Lessells

MR. J. M. LESSELLS[1] (*presented in written form*).—As a contribution toward the discussion of the papers by Messrs. McAdam and Moore, the writer wishes to advance a theory supported by experimental evidence that the value of the limit of proportionality in tension appears to have an effect on the value of the endurance limit. To show this effect clearly, recourse is had to one of the short methods of determining endurance values. There are two short methods known at present; the deflection method and the rise-in-temperature method.

TABLE I.—ENDURANCE VALUES OF METALS

Material	Condition	Elastic Limit, lb. per sq. in.	Endurance Limit by Deflection Method, lb. per sq. in.	Imposed Stress, lb. per sq. in.	Broken or Not Broken	Cycles of Stress
0.37-per-cent Carbon	Rolled	30 500	27 000	28 100	N B	20×10^6
" " "	Annealed	35 400	27 600	28 600	N B	20×10^6
" " "	Normalized	34 500	27 600	29 300	N B	20×10^6
" " "	Heat-treated	62 000	46 700	49 000	N B	20×10^6
0.79-per-cent Carbon		43 000	45 300	44 800	B	1.5×10^6
Stainless Steel	Rolled	44 000	41 400	41 400	N B	9×10^6
3½-per-cent Nickel Steel		29 000	46 800	46 750	B	89 600
				39 600	B	305 200
30-per-cent Nickel Steel		32 500	59 400	57 600	B	114 800
				55 800	B	195 000
Copper	Rolled	22 500	24 400	23 400	B	314 700
				19 800	B	1×10^6
Phosphor Bronze	Rolled	52 000	51 000	48 500	B	79 600
				36 000	B	608 900
Brass	Rolled	26 000	34 000	27 000	B	1.3×10^6
Brass	Annealed	9 500	18 000	18 000	N B	4×10^6

Since a large amount of work has been done in the research department of the Westinghouse Co. on the deflection method the discussion is confined to such results although similar results might be expected from the temperature method. In these tests a cantilever type of machine was used. The test specimen carried a mirror suitably mounted at the free end so that the filament of a lamp fixed to a sliding scale 11 ft. 3 in. from the mirror could be readily observed in a telescope similarly located. On applying load to the rotating specimen the lamp was moved down the scale until the image of the filament

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[1] Westinghouse Research Laboratory, East Pittsburgh, Pa.

Mr. Lessells

again became visible. In this manner by taking scale readings for successive loads a curve of stress and corresponding deflection was obtained, the point at which the deflection ceased to be proportional to load being the endurance limit for that particular test piece.

In Fig. 1 is shown a diagrammatic sketch of the arrangement; in Fig. 2 is shown the test specimen used, while in Fig. 3 a typical curve of deflection is given.

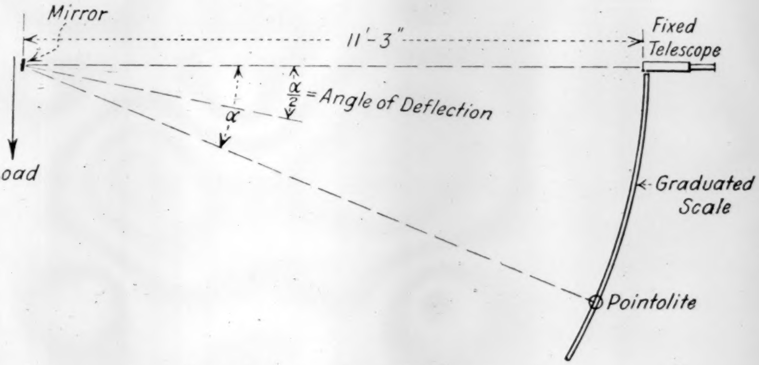
Mirror
11'-3"
Fixed Telescope
$\frac{\alpha}{2}$ = Angle of Deflection
α
Load
Graduated Scale
Pointolite

FIG. 1.—Arrangement of Apparatus

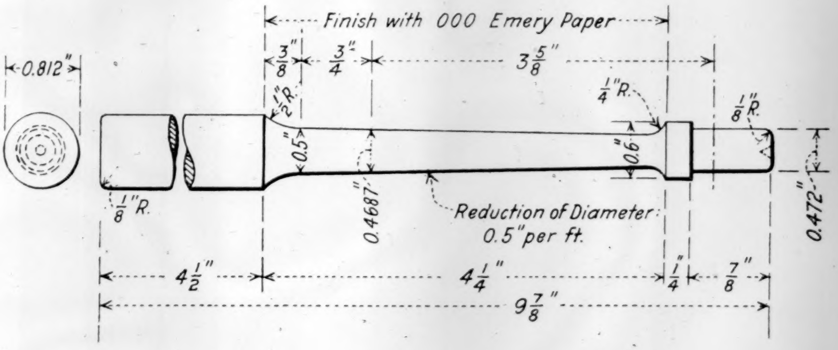
Finish with 000 Emery Paper
0.812"
$\frac{3}{8}$"
$\frac{3}{4}$"
$3\frac{5}{8}$"
$\frac{1}{2}$"R.
$\frac{1}{4}$"R.
$\frac{1}{8}$"R.
0.5"
0.4687"
0.6"
0.472"
$\frac{1}{8}$"R.
Reduction of Diameter 0.5" per ft.
$4\frac{1}{2}$"
$4\frac{1}{4}$"
$\frac{1}{4}$"
$\frac{7}{8}$"
$9\frac{7}{8}$"

FIG. 2.—Cantilever Test Specimen

There is reason to believe and accept that, given certain conditions, the deflection method forms a very convenient and accurate means of determining the endurance limit. In Fig. 4 is shown the endurance curve obtained by the longer methods for a 0.37-per-cent carbon steel as annealed together with the "deflection" endurance curve for the same material. It will be noted that the agreement is very close. In Table I is given other results for the same steel.

Mr. Lessells Other steels show similar results. Furthermore there is the evidence of other investigators such as H. F. Moore in this country and Lea and Gough in England.

It appears, however, that while this is true for certain steels that such cannot be accepted as general, and it also seems that the value of the limit of proportionality in tension has a decided influence on the degree of this exception. In Fig. 5 is shown a deflection curve for a $3\frac{1}{2}$-per-cent nickel steel. In this case it will be seen that the deflection value of endurance is 46,800 lb. per sq. in., whereas by running a test piece at 47,000 lb. per sq. in., fracture occurred at 89,600 cycles of stress, and, with one run at 39,000 lb. per sq. in., fracture occurred after 305,200 cycles, the elastic limit in tension being 29,000 lb. per sq. in.

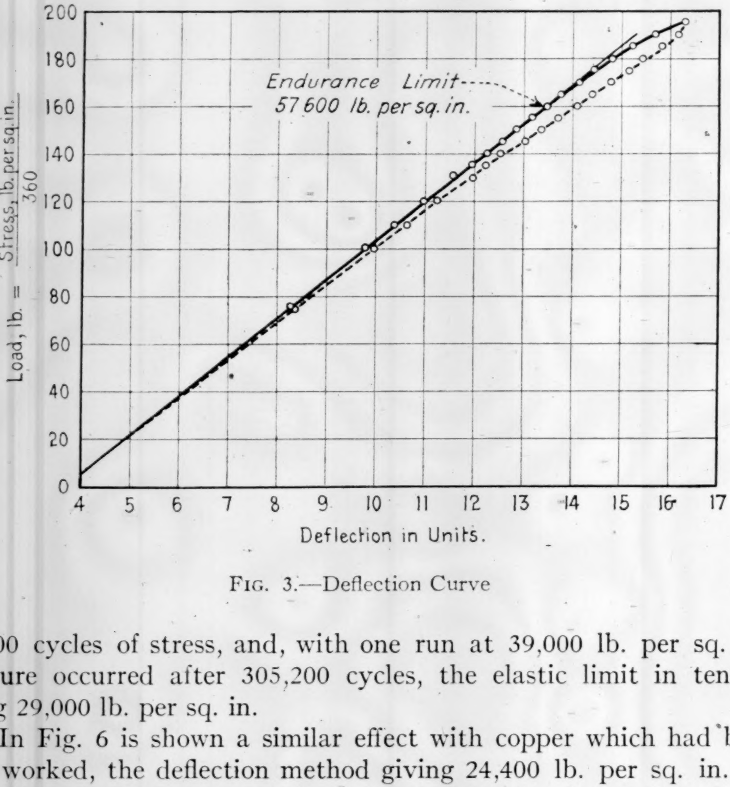

FIG. 3.—Deflection Curve

In Fig. 6 is shown a similar effect with copper which had been cold worked, the deflection method giving 24,400 lb. per sq. in. for the endurance limit, whereas fracture could still be obtained at a stress of 19,800 lb. per sq. in. after 1,010,400 cycles of stress.

A general summary of various materials is given in Table I. From this it will be seen that this method initiated at the National Physical Laboratory, England, gives very good results, provided the proportional or elastic limit in tension is naturally higher than the

Mr. Lessells

endurance limit so found. But if the limit of proportionality is lower than that obtained by the deflection method or has been artificially raised by cold working, then the deflection method, apparently due to the position of this natural elastic limit, gives misleading results. In such cases, if the time be sufficiently prolonged there is no evidence that fracture will not occur if stressed indefinitely beyond the natural elastic limit, while from what has been said if stressed at or within the natural elastic limit the endurance value is determined more rapidly and leads us to believe that fracture would not occur when stressed indefinitely at this value of endurance so found. These statements do not necessarily infer that the natural elastic limit can be determined from a single tensile test for such materials as Armco iron, austenitic steels and non-ferrous alloys, but the writer believes that such materials may form a class by themselves in which a stable elastic limit is possible by reversal of stress having a value clearly defined and higher than that obtained from a single tensile test. This may offer an explanation why the endurance value of such materials may exceed the value of the proportional limit as usually

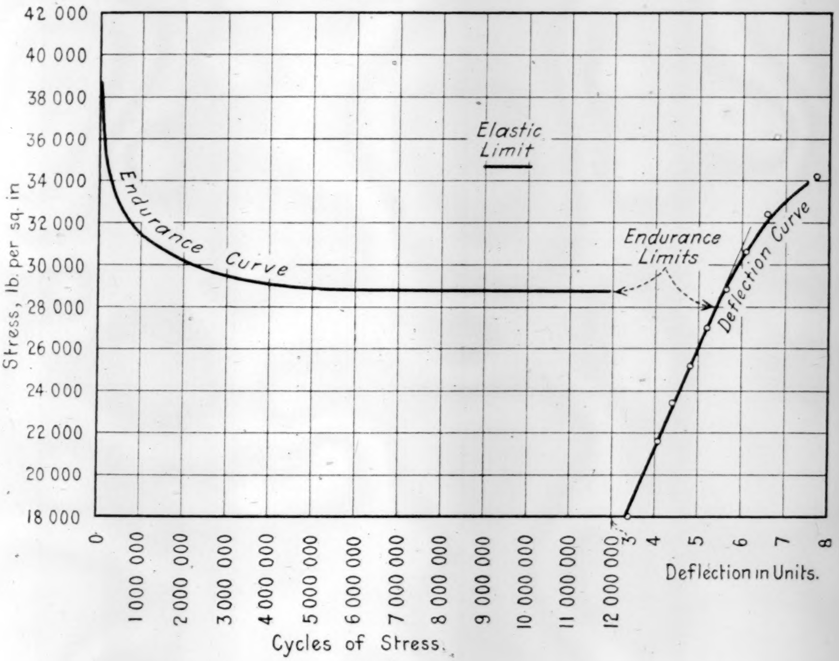

FIG. 4.—Endurance and Deflection Curve

Mr. Lessells determined. These results therefore lend weight to the writer's contention that the endurance limit is related in a simple manner not only to the tensile strength, but is also connected with the value of the proportional limit in tension. The author wishes also to defend the position of Mr. Stanton. The machine designed by him was never intended for carrying through tests at a single arbitrary drop and he has frequently criticized such results as being of little or no value. The writer wishes to thank Mr. Irwin for assistance in carrying through the tests herein described.

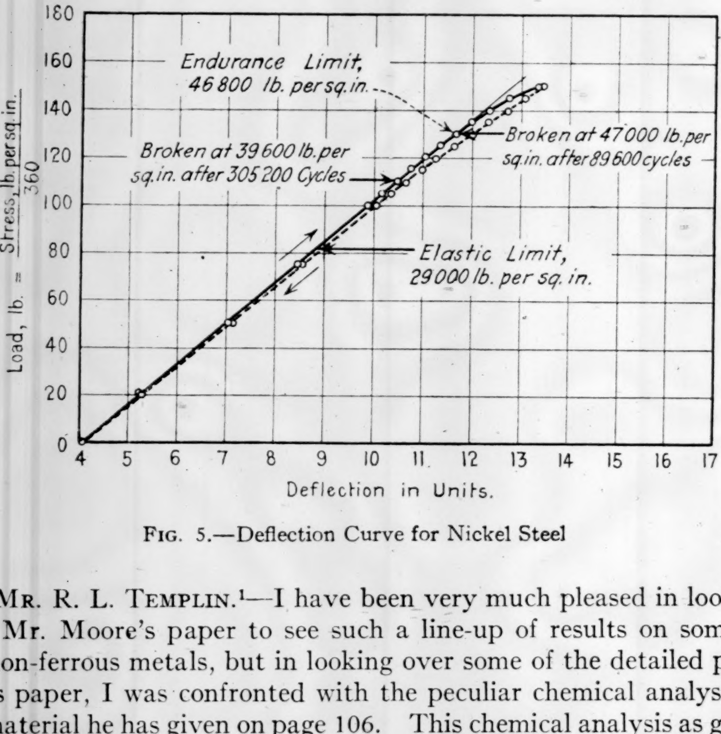

FIG. 5.—Deflection Curve for Nickel Steel

Mr. Templin MR. R. L. TEMPLIN.[1]—I have been very much pleased in looking over Mr. Moore's paper to see such a line-up of results on some of the non-ferrous metals, but in looking over some of the detailed parts of his paper, I was confronted with the peculiar chemical analysis of the material he has given on page 106. This chemical analysis as given is not in accordance with the Government specifications for the material which the tests are supposed to cover. The Government specifications indicate that the copper content should be from 3.5 to 4.5 per cent; manganese, 0.4 to 1 per cent; magnesium, 0.2 to 0.75 per cent; and aluminum, minimum, 92.0 per cent. In reality, the content runs very closely to 4 per cent copper, 0.5 per cent manganese and 0.5 per cent magnesium. I should like to venture the opinion

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[1] Chief Engineer of Tests, Aluminum Co. of America, New Kensington, Pa.

Mr. Templin

that if the material is of the composition given in the paper, the static properties which are indicated would not be obtained.

A considerable amount of the work relative to the determination of the properties of heat-treated duralumin which we have done in the laboratories of the Aluminum Company of America has indicated, in general, concurrence with the results obtained by Mr. Moore at McCook Field. When we are asked what is the fatigue limit for this metal, we usually have to counter with the question, "What criterion

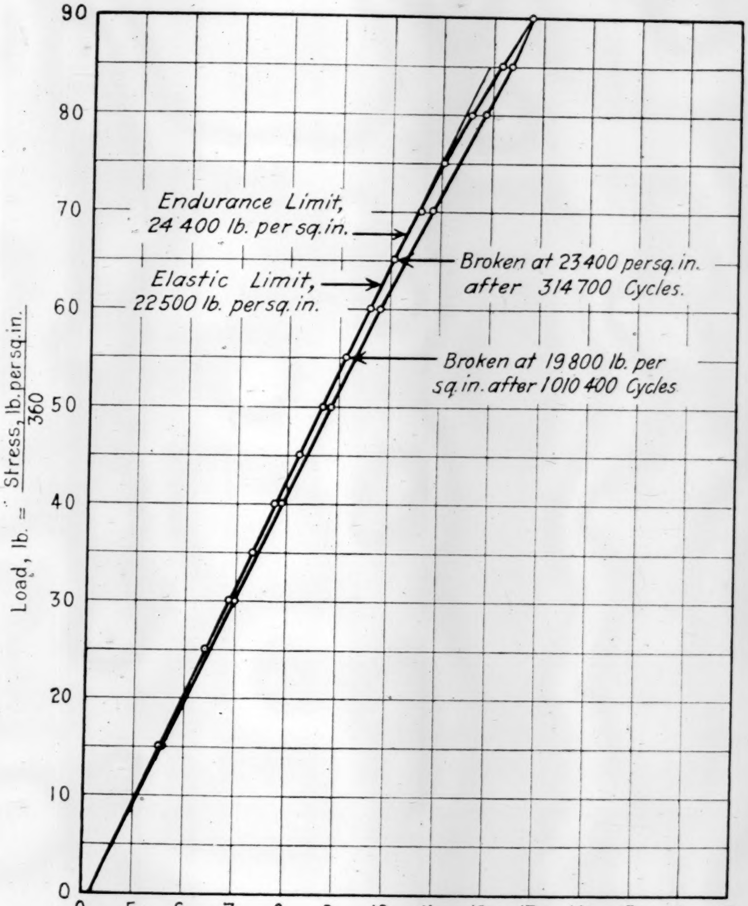

FIG. 6.—Deflection Curve for Copper

Mr. Templin do you use in determining the fatigue limit? That is, how many repetitions of stress do you consider necessary to define the fatigue limit of the metal?" In our work in testing this metal, we try to extend the number of repetitions as far as possible. We have three specimens now running in our laboratory, one of which, up to June 18, had run 545,500,000 repetitions at a stress of 4000 lb. per sq. in.; another, 702,168,000 at a stress of 6000 lb. per sq. in. and the other 555,244,000 at a stress of 8000 lb. per sq. in.; and they are still going. The static properties of the material which we are testing are as follows: tensile strength, 56,800 lb. per sq. in.; yield point, 34,000 lb. per sq. in.; proportional limit, 25,000 lb. per sq. in.; elongation in 2 in. for the standard 0.505-in. diameter specimen, 23 per cent; reduction of area, 34.4 per cent.

There is quite a marked difference between the character of the curves in the upper part of Fig. 7 for the "as received" material and the "tempered" material. You will notice the two breaks in the "as received" material and only a suggestion of one break in the other. We have found similar results for material where there was some difference in the heat treatment. We are not in a position to say at the present time just why such discrepancies occur, but evidently the heat treatment has something to do with the shape of the curves obtained. That is one of the reasons, perhaps, why our results for this material have not been published to date. You will note that there is also quite a difference in characteristics between the curves in the lower part of the diagram in the "tempered" and the "as received" material.

Metals which have essentially magnesium for their main constituent, such as electron, nearly always have an elastic limit appreciably if not considerably above the proportional limit. This may or may not have a bearing on some of the fatigue properties reported by Mr. Moore.

Mr. Moore MR. R. R. MOORE.—In reply to Mr. Templin's remarks on the chemical analysis of the duralumin given in my paper, I desire to state that this material was obtained several years ago and the chemical analysis given is representative of the material received at that time; further, Air Service Specifications for duralumin do not prescribe any chemical analysis, for such material is bought mostly on physical properties.

Mr. Tuckerman MR. L. B. TUCKERMAN.[1]—I wish to emphasize the present uncertain, anomalous state of the non-ferrous metals. We all felt that the problem of fatigue resistance was very nicely settled when the results

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[1] Engineer Physicist, U. S. Bureau of Standards, Washington, D. C.

Mr. Tuckerman

of H. F. Moore at Illinois and the work of Gough in England had shown agreement between short-time tests and long-time tests. We felt that probably here was a chance to make a routine laboratory method for determination of fatigue resistance. The results on non-ferrous alloys and especially the results on duralumin have completely shattered that hope so far as these are concerned. The long-time test which the author reported is merely an illustration of what we are facing in determining what such alloys as aluminum alloys and brasses can endure under long-time repeated stresses. To-day we have fatigue machines which can, for selected types of specimens, run in the neighborhood of 2000 r.p.m. (less than 3 million per day). In dealing with thin sheet material as we are at the U. S. Bureau of Standards, we have not as yet developed machines which for the thinnest materials run more than 400 or 500 r.p.m. (less than a million per day). When you find that the differences in fatigue endurance of different specimens of duralumin do not appear until 50 or 100 or even 200 million (and Mr. Templin of the Aluminum Company of America says not until 500 or 600 million) alternations of stress have been reached, you can see that it looks like a 25 or 30-year problem.

The program of tests we have started at the Bureau was figured on the assumption that only a relatively few tests would be carried beyond ten million alternations. If, as seems necessary, we carry them to 200 million alternations instead of ten, the tests already outlined will occupy our machines for the next fifteen years.

Let us hope that somebody will furnish us with a short-time test which can be relied upon for non-ferrous alloys.

Mr. Styri

MR. HAAKON STYRI.[1]—I would like to ask why so few tests are made at each load? The data usually given will not permit of a calculation of the average endurance at each load and a positive determination of the fatigue limit. It may be true that for soft steels you get fairly consistent results. We have run half a dozen specimens of soft steel under the same alternating load applied axially on full sections and found 100 per cent variation. On ball bearings we find many times greater variation in endurance when we apply the same load. We find that we have to run about twenty specimens to get one-half of the average results within a probable error of about 15 per cent. I would like to mention this because it is very important in dealing with hard material; it may not be so important for soft steel.

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[1] Chief of Research Laboratory, S. K. F. Industries, Inc., Philadelphia, Pa.

METHODS OF CASTING TEST SPECIMENS OF GUN METAL

By E. H. Dix, Jr.[1]

The experience of the McCook Field Foundry of the Engineering Division of the Air Service in connection with casting test bars of gun metal is presented in this paper at the suggestion of the Society's Committee B-2 on Non-Ferrous Metals and Alloys, through the chairman of its Sub-Committee III on Sand-Cast Metals and Alloys.

In selecting the method to be used for casting test bars, the first consideration is whether the bars are to be tested "as cast," that is, without removing the skin, or in the "machined" condition. Practice seems to vary with the different alloys; for instance, in the brasses and bronzes it is usual to test the bars after machining, whereas in the aluminum alloys it is very general practice to test them as cast. It is generally known that the metal in the "skin" or outside surface is considerably stronger than the remainder of the metal in the specimen, and for this reason the "as cast" specimen will show a greater strength than the "machined" specimen cast in the same manner. Certain of the methods of casting test bars experimented with, namely, those in which the test bar is gated along its entire length after the manner of Webbert, should be machined before testing, whereas some of the other methods, such as the one used by the Engineering Division, can be very successfully pulled without any machining.

Whether self-alligning adaptors are necessary depends largely upon how well the testing machine is lined up and upon the skill of the operator. Experience of the Engineering Division has shown that with a careful operator and the machine well lined up, the wedge grips can be made to give satisfactory results. In any event, it is believed that a shouldered specimen can be tested in self-alligning adaptors without receiving any machining. It is, therefore, felt that an effort should be made to obtain a satisfactory method of gating which will not disturb the gage length of the test specimen so that it may be tested in the "as cast" condition with a resulting saving in time and expense. It is well known that it costs more and requires a much longer time to machine bars than it does to perform the actual testing, and unless a large machine shop is available, tests are very likely to be seriously delayed owing to loss of time in the machine shop.

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[1] Chief, Metallurgical Branch, Engineering Division, Air Service, McCook Field, Dayton, Ohio.

For the past several years, a very simple, convenient method of casting bars to be tested "as cast" has been used at the McCook Field Foundry for all non-ferrous alloys with the exception of manganese bronze and other high shrinkage metals. This method is known as Pattern TB-1 and is indicated as No. 1 in Fig. 1. The pattern, including gates, is made up in the form of a match plate. Three test bars are cast in one mold, the metal being poured down a $1\frac{1}{4}$-in. diameter pouring sprue from which it is gated into the cope half of one end of the bars, the other end of the bars being joined through similar gates to a common riser $1\frac{1}{4}$ in. in diameter. A 3-in. cope is used for all metals. In general, all three bars from one mold are tested and the average results used as a basis for comparison. The results from this method of casting have been, in general, very satisfactory, but once in a while one bar of the mold will show a low strength and elongation in comparison to the other two bars. There has been no consistency in regard to whether the low bar was an outside or the center one. The following results on the third mold poured of melt No. 1984 illustrate the point in question:

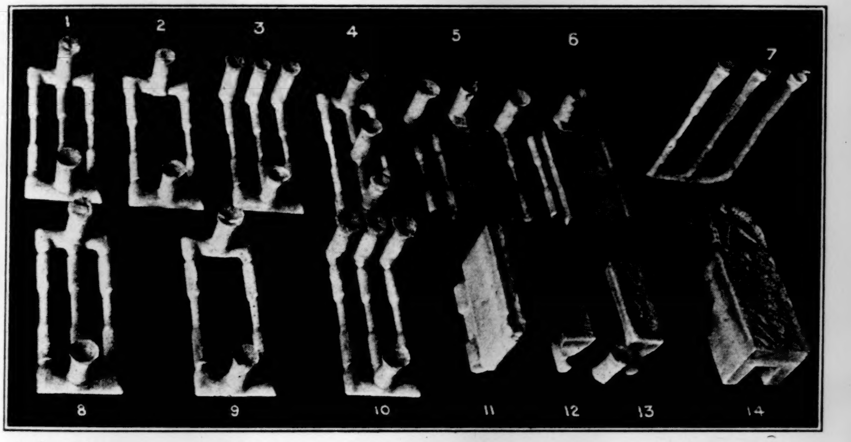

FIG. 1.—Methods of Casting Test Specimens

SPECIMEN NO.	3 A	3 B	3 C
Yield point, lb. per sq. in.	25 560	26 090	22 830
Tensile strength, lb. per sq. in.	46 410	46 760	36 170
Elongation in 2 in., per cent	18.5	18.0	6.5

In this case, specimen No. 3C (the specimens are numbered so that A and C are the outside bars and B the center) is obviously so much lower than A or B as to indicate a lack of consistency which is difficult to understand because there was no evidence of a flaw in the specimen.

Experiments with short run molds showed that in some cases the metal would run straight through one bar and begin to back up in the other two bars, causing the two streams of metal to meet in the center of the gage length of one or more of the bars. This is obviously an undesirable condition for test specimens, where uniformity should be the keynote. Variations in the path of the flow would be expected with different pouring temperatures, different rates of pouring, and different positions of the pot with respect to the mold in pouring, thus probably accounting for the low bar sometimes being located in the center and sometimes on one or the other side positions.

It was in order to eliminate this possibility of variation in results that an investigation on methods of casting test bars was undertaken. Fourteen different methods were experimented with, some of which were obviously unsuitable for this alloy, but were included in order to ascertain the variation in physical properties which may be expected from different methods of gating.

METHODS OF CASTING TEST BARS

The various methods experimented with can best be explained by referring to Fig. 1. In all of the methods except No. 11, a standard size and shape of test specimen has been adhered to. For testing "as cast" the specimen is $\frac{1}{2}$ in. in diameter by 2 in. gage length and is joined by large radii to the $\frac{3}{4}$ in. diameter shanks, the total length being 7 in. For testing after machining, the diameters are increased by $\frac{1}{8}$ in., and later machined to 0.505 in. in diameter over the gage length with $\frac{3}{4}$ in. diameter threaded ends.

Method No. 1 is the pattern TB-1 previously referred to.

Method No. 2 is the same as No. 1, except that the middle bar is omitted, thus reducing the chance of circulation of the metal through the bars.

Method No. 3 is also the same as No. 1, except that all possibility of the circulation of the metal has been prevented by using individual risers which are not connected.

Method No. 4 has been arranged to promote circulation of the metal by using the center bar as a runner and joining the bars to common risers at each end.

Method No. 5 was suggested by Mr. W. M. Corse.[1] The specimens are joined through the shanks by a web $\frac{3}{8}$ in. thick to a reservoir block 8 by 4 by 1 in. thick.

Method No. 6 is an adaptation of the Webbert[2] gate in which the specimen is joined to the reservoir block, 8 by 4 by 1 in. thick, by a $\frac{1}{8}$-in. web extending along its entire length.

Method No. 7 consists of two bars cast vertically by bottom feeding, with risers on top of each bar.

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[1] Private communication from Mr. Jesse L. Jones.

[2] L. P. Webert, "Study of the Strength of Non-Ferrous Castings: Comparison of Different Test Specimens," ***Proceedings***, Am. Soc. Testing Mats., Vol. XIV, Part II, p. 145 (1914).

Method No. 8 is the same as pattern TB-1 except that the bars are $\frac{1}{8}$ in. larger in diameter to allow for test after machining.

Method No. 9 is the same as No. 2 except that the bars are $\frac{1}{8}$ in. larger in diameter for machining.

Method No. 10 is the same as No. 3 except that the bars are $\frac{1}{8}$ in. larger in diameter for machining.

Method No. 11 is the standard used by several of the Government Bureaus[1] for this type of alloy in which the riser block, 8 in. long by 1 in. wide by 4 in. high, is joined the full length of the test bar by a web $\frac{1}{4}$ in. thick and $\frac{3}{4}$ in. high. The specimen was cast $\frac{5}{8}$ in. in diameter over a 2-in. gage length with $1\frac{1}{8}$-in. diameter ends and was machined to the standard 0.505-in. diameter specimen with threaded ends.

Method No. 12 is similar to the method used by the Air Service for manganese bronze except that the web joining the specimens is $\frac{1}{8}$ in. thick.

Method No. 13 is the method used by the Air Service for casting manganese bronze.[2] The riser is 3 in. across the top and tapers down to a $\frac{1}{4}$-in. web joining the specimens. The height is 3 in.

Method No. 14 is a common method for casting manganese bronze test bars.

Method No. 15 is not shown in Fig. 1, but is the same as No. 3, except that the risers are omitted.

Method No. 16 is not shown in Fig. 1, but is the same as No. 10, except that the risers are omitted.

All specimens were cast in green sand molds, except that in the case of Method No. 11 the upper half of the test bar, including the $\frac{1}{4}$-in. thick web, is formed by a dry sand core. The molding sand was a mixture of Sandusky and Albany, well adapted for bronze molding. The cores were made of a 1 : 1 mixture of Sandusky molding sand and lake sand bonded by boiled linseed oil.

Some of the metal from melt No. 2247 was poured into the form of a casting, sketched in Fig. 2. Fifteen specimens were cut from this casting at the locations indicated. The casting was poured by two horn gates as shown, three risers 2 in. in diameter and 5 in. high being used.

The weight of metal required for each method follows:

METHOD	NUMBER OF BARS	WEIGHT, LB.		METHOD	NUMBER OF BARS	WEIGHT, LB.	
		PER MOLD	PER BAR			PER MOLD	PER BAR
1	3	6.10	2.03	9	2	6.0	3.00
2	2	5.10	2.55	10	3	9.3	3.10
3	3	7.60	2.53	11	1	11.0	11.00
4	2	7.10	3.55	12	1	9.0	9.0
5	2	16.60	8.30	13	1	9.0	9.0
6	2	13.10	6.55	14	2	31.9	15.95
7	2	4.30	2.15	15	3	5.4	1.8
8	3	7.10	2.37	16	3	6.6	2.2

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[1] Naval Gun Factory Blue Print S. A. 16033.—Tentative Ordnance Specification.

[2] E. H. Dix, Jr., "Methods of Casting Manganese-Bronze Test Bars as a Check on Melts of Small Castings," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 912 (1921).

MELTING PROCEDURE

The results given in this paper, totaling over 300 tension tests, were made on test bars from three lots of metal, one of which was remelted several times. These lots can best be identified by the regular foundry melt number. A description of the melting practice for each melt is given below:

Melt No. 1984 is a 360-lb. melt of the standard gun metal composition: copper 88 per cent, tin 10 per cent, zinc 2 per cent, made up in a No. 400 Hausfeld gas-fired crucible tilting furnace. The charge consisted of electrolytic copper 99.70 per cent pure, Banka tin, and zinc ingot which contained 0.03 per cent of iron and 0.24 per cent of lead. A temperature of 2100° F. was maintained in the furnace. One-fourth of 1 per cent of 15-per-cent phosphor-copper was added just before pouring. The metal was poured from the furnace into a No. 40 plumbago crucible from which the test bar molds were poured. The molds were placed about 20 ft. from the furnace. About 70 lb. of metal were carried each time so that it required five trips to the furnace to pour all of the molds of test bars.

Melt No. 2127 is a remelt of No. 1984, the remelting being accomplished in the Hausfeld gas-fired furnace at the same temperature.

Melt No. 2247 is a remelt of the gates of No. 2127, the remelting being done in the Hausfeld furnace at the same temperature.

Melt No. 2335 is a 414-lb. melt of new metal of the following composition: 88 per cent copper, 8 per cent tin, and 4 per cent zinc, made up in a Monarch open-flame rocking furnace in which electrolytic copper of 99.94-per-cent purity, Banka tin, and horsehead zinc containing 0.011 per cent of iron and 0.05 per cent of lead were used. One-fourth of 1 per cent of 15-per-cent phosphor-copper was added just before the metal was poured. Owing to insufficient air pressure at the furnace, the maximum furnace temperature was 2075° F.

Melt No. 2348 is a remelt of No. 2335 in which 1.5 per cent of zinc was added to the charge. The melting was done in the Hausfeld furnace and a temperature of 2150° F. maintained.

RESULTS

The results of these tests, together with the chemical analyses, are summarized in Table I. In this table the method of casting the test bars is given with reference to Fig. 1. The order in which the molds were poured is given by the mold number. The uniformity obtained in the tests is shown by the mean deviation from the average, expressed as a percentage of the average.

Table II gives the detailed results of the tests on the specimens cut from the casting, melt No. 2247.

The Brinell, scleroscope and Rockwell hardness and the specific gravity for the various melts have been given in Table III as a matter of general information.

TABLE I.—SUMMARY OF RESULTS

Method of Casting (Numbers refer to Fig. 1)	Mold No.	Number of Tests	Average Yield Point, lb. per sq. in.	Tensile Strength		Elongation in 2 in.	
				Average Values, lb. per sq. in.	Mean Deviation,[1] per cent	Average Values, per cent	Mean Deviation,[1] per cent
MELT NO. 1984. CU, 86.80; SN, 10.95; ZN, 2.22; P, 0.026							
Tested "As Cast"							
1	1-2-3	9	24 920	43 530	7.94	14.5	28.3
2	4-5-6	6	26 520	44 500	3.56	14.8	13.6
3	7-8-9	9	26 360	44 640	2.18	15.7	9.6
4	10-11	4	26 360	42 460	6.06	12.6	22.8
5	28-29	4	25 880	49 240	7.9	25.4	26.1
6	30-31	4	24 170	48 660	6.75	30.3	19.3
7	32-33	4	25 770	37 540	4.07	9.0	11.1
Tested after Machining							
8	12-13-14	9	26 000	37 120	11.2	11.2	37.5
9	15-16-17	6	24 820	38 670	7.25	15.0	33.3
10	18-19-20	9	24 800	33 270	16.89	8.2	63.7
11	23-4-5-6-7	5	24 940	41 190	4.29	16.5	32.8
12	A21-A22	2	25 005	36 500	6.0	9.5	15.8
13	B21-B22	2	25 195	38 210	2.1	11.0	9.09
14	34	1	23 500	34 950	....	10.0	....
MELT NO. 2127: REMELT OF 1984							
Tested "As Cast"							
1	1-3-5-7	12	27 970	45 160	6.7	18.8	27.2
3	11-13-15-17	12	28 040	45 255	3.08	17.3	14.5
15	19-21-23-25	12	27 000	44 470	5.65	15.7	19.8
Tested after Machining							
8	2-4-6-8	12	22 600	39 840	7.81	15.6	25.7
10	12-14-16-18	12	22 230	40 600	2.62	16.9	4.7
16	20-22-24-26	12	23 660	39 620	6.76	12.4	25.6
11	9-10-27-28	4	22 320	43 300	2.20	26.9	4.17
MELT NO. 2247: REMELT OF 2127							
Tested "As Cast"							
1	3-4	6	26 800	46 040	1.19	19.1	7.1
Tested after Machining							
8	1-2	6	23 830	40 830	2.04	17.8	6.5
11	5-6-7-8	4	22 510	41 770	3.52	25.5	20.6
[a]	Casting	15	18 000	26 600	....	13.8	....
MELT NO. 2335. CU, 88.93; SN, 8.17; ZN, 2.90							
Tested "As Cast"							
1	1-2-3	9	23 480	48 390	4.11	39.5	14.1
2	4-5-6	6	23 590	48 810	1.51	37.0	8.54
3	10-11-12	9	23 420	47 140	3.86	31.5	15.00
15	7-8-9	9	23 390	42 250	5.38	23.4	2.1
Tested after Machining							
8	13-14-15	9	21 900	38 300	12.55	22.5	38.2
9	16-17-18	6	22 300	34 200	10.50	12.6	27.1
10	19-20-21	9	21 900	34 500	9.64	14.6	31.0
11	25-26-27-28	4	20 830	45 000	2.80	38.4	12.7
12	A22-A23	2	22 130	43 450	1.73	33.5	14.9
13	B22-B23	2	23 250	46 200	0.65	40.8	1.8

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[a] See Table II for detail of test results.

[1] The mean deviation was obtained by adding the difference between the average result and each individual result dividing by the number of individual results, and expressing the answer in per cent.

TABLE I.—SUMMARY OF RESULTS—*Continued.*

Method of Casting (Numbers refer to Fig. 1)	Mold No.	Number of Tests	Average Yield Point, lb. persq.in.	Tensile Strength		Elongation in 2 in.	
				Average Values lb. persq. in.	Mean Deviation,[1] per cent	Average Values, per cent	Mean Deviation,[1] per cent
MELT NO. 2348. CU, 88.15; SN, 8.26; ZN, 3.59							
Tested "As Cast"							
3	1-6-11-16-21	15	......	46 000	3.94	31.5	15.25
Tested after Machining							
8	5-10-15-20, 25	15	......	43 000	3.00	28.1	12.45
11	2-3-4-7-8-9-12-13-14-17-18-19-22-23-24	15	......	42 200	3.16	39.0	13.75

[1] The mean deviation was obtained by adding the difference between the average result and each individual result, dividing by the number of individual results, and expressing the answer in per cent.

TABLE II.—TESTS OF SPECIMENS FROM CASTING—MELT NO. 2247

Specimen No.[1]	Yield Point, lb. per sq. in.	Tensile Strength, lb. per sq. in.	Elongation,[1] per cent
A1	16 470	20 560	7.5
A2	17 380	20 840	....
A3	17 100	23 750	....
Average	16 980	21 720	7.5
B1	18 100	27 160	16.7
B2	17 450	28 040	18.7
B3	18 800	30 980	....
E1	17 200	23 640	8.0
E2	20 460	30 910	....
E3	17 820	24 130	10.0
Average	18 300	27 480	13.4
C1	17 740	27 760	15.3
C2	19 300	28 600	....
C3	18 540	29 550	....
D1	17 900	28 300	17.3
D2	18 350	28 300	16.7
D3	18 100	25 980	....
Average	18 330	28 080	16.1
Grand Average	18 000	26 600	13.8

[1] A1 and A3: 0.505 in. diameter by 2 in. gage length; all others, $\frac{3}{8}$ in. diameter by $1\frac{1}{2}$ in. gage length.

TABLE III.—HARDNESS AND SPECIFIC GRAVITY TESTS FOR VARIOUS MELTS

Melt No.	Specimen No.	Hardness			Specific Gravity
		Brinell, 500 kg.	Scleroscope, Mag. Hammer	Rockwell, $\frac{1}{8}$-in. ball	
1984	21B[1]	65	24-25	45	8.57
2127	1B	70	26-27	50	8.68
2247	1B	70	23-25	47	8.56
2335	7B	70	26-27	50	8.73
2348	16B	69	.....	..	8.72

[1] This specimen was cast with heavy riser.

DISCUSSION OF RESULTS

Comparison of Methods Nos. 1, 2, 3, 4, and 15.—These methods comprise the different variations of pattern TB-1. All of the specimens were tested "as cast." The averages for all the melts of the mean deviations for these different methods are as follows:

METHOD No.	NUMBER OF TESTS	MEAN DEVIATION, PER CENT.	
		TENSILE STRENGTH	ELONGATION
1	36	4.99	19.20
2	12	2.54	11.07
3	45	3.28	13.59
4	4	6.06	22.80
15	12	5.65	19.80

The superiority of Methods Nos. 2 and 3 is evident. There is a slight advantage shown in favor of No. 2, due possibly to the fewer number of tests made. While the results of method No. 3 have been found very satisfactory, it is suggested that a slightly greater uniformity might be obtained by using individual risers at each end of the specimens, and a single pouring head with gates leading into the risers at the pouring end. It is also thought that a more uniform feeding action may be obtained by cutting the gates the full diameter of the test specimen, although this is undesirable from the standpoint of fettling. Further experimentation would be necessary to ascertain if the gain in uniformity, if any, would warrant the extra cost in metal and labor.

Comparison of Methods Nos. 8, 9, and 10.—These methods comprise the different variations of pattern TB1-A, which is the same as TB1 except for the $\frac{1}{8}$-in. increase in the diameter of the test specimens. These were tested after machining in self-alligning adaptors. From Table I it is seen that the mean deviations in these three methods are relatively large, and there seems to be very little to recommend any of these methods.

Methods Nos. 5 and 6.—These methods gave the highest strength and elongation of any of the methods used for melt No. 1984, but were not experimented with further because the results were so high as to be misleading to designers if used for specification values.

Method No. 7.—This method did not show promising results in melt No. 1984, and so was discarded.

Method No. 11.—Specimens from this method were tested in self-alligning adaptors after machining. This method of casting has given the most uniform results of all of the specimens tested in the "machined" condition.

A comparison of the mean deviation from the average for all melts obtained with methods Nos. 3 and 11, together with the weight of metal per test specimen, follows:

METHOD No.	NUMBER OF TESTS	MEAN DEVIATION, PER CENT.		WEIGHT PER SPECIMEN, LB.
		TENSILE STRENGTH	ELONGATION	
3	45	3.28	13.59	2.5
11	32	3.19	16.80	11.0

Methods Nos. 12, 13, and 14.—These methods were included merely to show the variation that might be expected from some extreme methods of casting. In melt No. 1984 the results from all three methods were low but fairly uniform, whereas in melt No. 2335, methods Nos. 12 and 13 showed high and very uniform results.

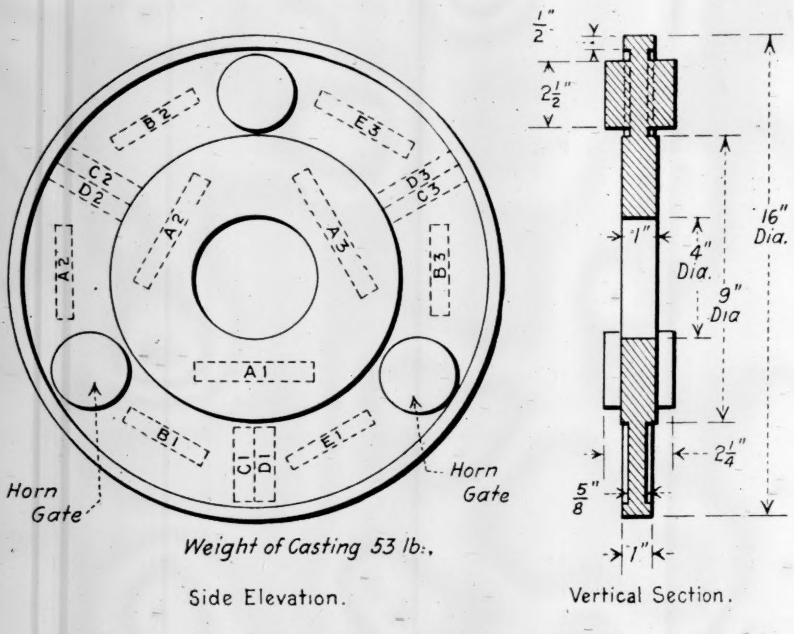

B2
E3
C2
D2
D3
C3
A2
A3
A2
B3
A1
B1
C1
D1
E1
Horn Gate
Horn Gate
Weight of Casting 53 lb.
Side Elevation.
1/2"
2 1/2"
1"
4" Dia.
9" Dia
16" Dia.
2 1/4"
5/8"
1"
Vertical Section.

FIG. 2.—Location of Specimens in Casting of Melt No. 2247

Specimens Cut from the Casting (Fig. 2.)—The details of these tests are given in Table II. The "A" specimens cut from the center, where the metal was 1 in. thick, showed a lower strength and elongation than those cut from the $\frac{5}{8}$-in. thick flange. Whether the specimens were taken radially or tangentially did not seem to make any difference, nor did the location with respect to the horn gates.

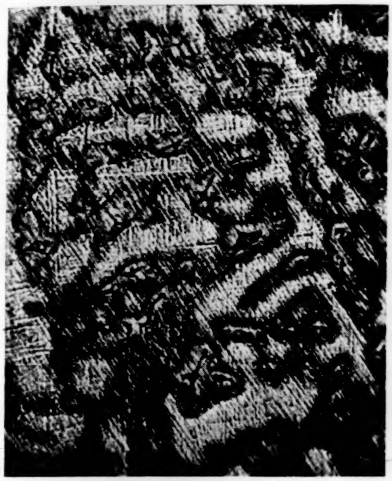


FIG. 3.—Structure of Specimen 1*B*, Melt No. 2335×100. Etched $NH_4OH + H_2O_2$

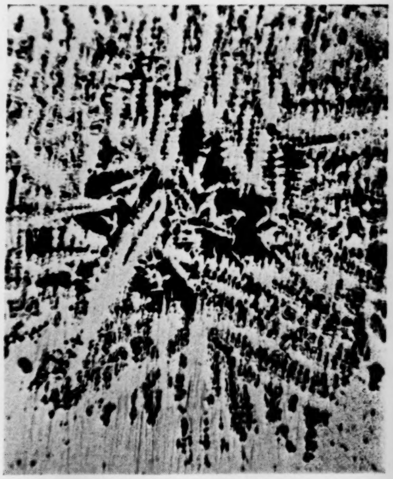
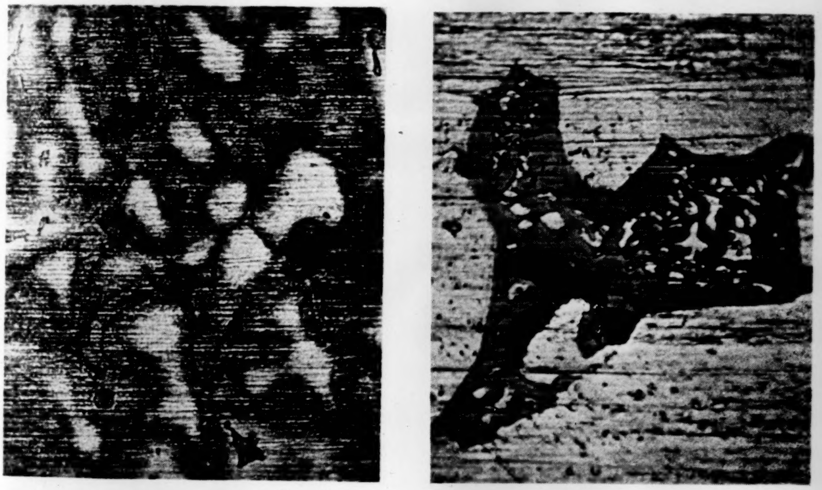


FIG. 4.—Porous Center in Middle of Gage Length of Specimen 1*B*, Melt No. 2348 × 30. Unetched



(*a*) ×100 (*b*) ×1000

FIG. 5.—Structure of Specimen *A*3 Cut from the Center Portion of the Casting of Melt No. 2247, showing Oxide Films and Eutectoid Area at High Magnification. Etched $NH_4OH + H_2O_2$

The average strength was not much over half that obtained on the separately cast test specimens and the elongation was also lower. This agrees with the experience of Webbert.[1]

Microstructure and Fractures.—A study of the fracture of the test bars of this investigation has shown that the strongest and most ductile test bars exhibit a gray radial fracture with a minimum of light yellow spots. In cases where the light yellow spots are sprinkled across the section, giving a mottled appearance, the tensile strength and elongation have been found to be low and it may almost be said that these properties are in nearly the inverse proportion to the amount of the yellow structure present in the fracture. Comparative examination of the microstructure of specimens which showed the gray fracture and those showing the mottled gray and yellow fail to reveal any difference in the microstructure. Fig. 3 illustrates the microstructure of this type of alloy. Some of the test bars, particularly those cut from the casting, showed reddish-orange spots in the fracture. Such specimens always showed a very low tensile strength and elongation, and the microstructure showed oxide films similar to those in Fig. 5 (*a*). The eutectoid areas are often associated with porosity, similar to that shown in Fig. 5 (*b*). Occasionally, even in the high strength specimens showing a gray fracture, there would be segregated at the center a small circle of yellow constituent. Microscopic examination of such a test bar revealed a small porous center as shown by Fig. 4, into which the dendrites extended.

Macrographic examination of the cross-section of specimens cast according to pattern TB1 and its variations showed radial grains of uniform size, whereas those cast according to method No. 11 showed fine grains radiating from a point below the center, toward the bottom of the specimen where the cooling was most rapid, and large grains radiating toward the top where the heavy riser caused slow cooling.

Acknowledgment.—Grateful acknowledgment is made to those of the Material Section staff at McCook Field, particularly Lieutenant A. J. Lyon, Messrs. R. R. Moore, M. R. Whitmore, and D. M. Warner, who aided in this investigation.

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[1] *Loc. cit.*

DISCUSSION

MR. JESSE L. JONES[1] (*presented in written form*).—While numerous **Mr. Jones** forms of sand-cast test pieces have been suggested or used for non-ferrous alloys, no single test piece has yet appeared that is entirely satisfactory. The list of patterns for casting given in the paper by Mr. E. H. Dix, Jr., covers practically all the more valuable patterns that have been used. Some of the objections to the various forms experimented with are, as follows:

Patterns Nos. 1, 2, 3, 4, 7, 8, 9 and 10 all have the common fault of not allowing free shrinkage of the parallel portion of the test bar. The pouring gate and riser constitute two fixed points which, during the process of solidification, prevent free shrinkage, especially where the molds have been rammed very hard. The shrinkage stresses are likely to be localized in the weakest portion of the casting, which is the 2-in. parallel section. This section is, of course, the most important portion of the casting, as it is the part upon which the tension test is made. In the modifications mentioned by Mr. Dix, in which the risers are omitted, this objection is possibly not so pertinent. An additional objection to pattern No. 7 is that the coldest metal is in the risers and this interferes with adequate feeding. The rather homely, but important, advice of a very successful foundry superintendent, to designers of important castings is, to "always keep the hottest metal in the garret and the coldest in the basement."

Patterns Nos. 6, 11, 12 and 14 are all equally at fault, owing to the fact that the grain structure of the test piece immediately under the riser portion of the casting, is likely to be considerably coarser than the rest of the test piece. This gives an area of weakness that prevents the development of the full strength of the metal when the test piece is tested; in other words, the coarser grain acts very much as a nick and may result in premature fracture. In this connection, Mr. Dix's advice of studying the fracture of specimens, rather than their microstructure, is most excellent. An etched cross-section of a test piece, showing its macrostructure, is even more satisfactory than the appearance of the fracture. The orange or yellow spots in the fractured test pieces, are invariably an evidence of inadequate feeding. Of all the patterns listed by Mr. Dix, No. 5 is the least objectionable. This form is the one now under consideration by Sub-Committee III,

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[1] In Charge of Chemical and Experimental Laboratory, Material and Process Division, Westinghouse Electric and Manufacturing Co., Pittsburgh, Pa.

Mr. Jones of Committee B-2 on Non-Ferrous Metals and Alloys, of the Society. It has the distinct advantage that the parallel portion is entirely surrounded by sand. The webs are connected with the grip ends of the test piece, and as these portions of the test piece are amply strong, as a rule, a slight weakening at these points is immaterial. The risers shown in pattern No. 5 have been omitted in the latest improvement of this test piece and the shrinkage of the test piece and the reservoir block can be assumed to be almost identical. In the present form of this pattern, which is being considered by Sub-Committee III, it has been found necessary to very much increase the size of the reservoir block in order to satisfactorily feed alloys of high shrinkage, like manganese bronze. This has resulted in the entire casting being very heavy indeed, and this constitutes the chief objection to this pattern at the present time. It is suggested that this objection can be minimized by reducing the width of the reservoir block in the center and giving it the general form of a dumb-bell.

It is suggested that any patterns for sand-cast test pieces that are under consideration be compared with an ingot test bar. A form that is very satisfactory has a length of 6 in., a width of $1\frac{1}{4}$ in. at the bottom, $1\frac{1}{2}$ in. at the top and a depth of $1\frac{3}{4}$ in. The mold for this test piece should be very much thicker at the bottom than at the top, so that the metal poured into it will set from the bottom up. The test piece should be taken from the bottom portion of the ingot cast, thus avoiding the heavy shrinkage area in the upper portion.

Mr. Webert MR. L. P. WEBERT[1] (*presented in written form*).—In this valuable paper the author shows that Method No. 3 furnished a test specimen giving remarkable uniformity when tested in the "as cast" condition. According to Mr. Dix, specimens "as cast" without machining can be successfully used for testing gun metal. However, the test bars molded and cast separately will not become as popular in the foundry as the type of test bar which is cast attached to the casting. The casting with its attached specimen passes through the cleaning room and after the sand blasting the casting and specimen are stamped before removing the specimen. With the test bars cast separately it is necessary to prepare an extra mold for each heat and to exercise more care with the stamping of the bars.

On page 137, Mr. Dix describes Methods Nos. 5 and 6 as giving the highest strength and elongation of any of the methods used. These methods were not experimented with further, because the results were so high as to be misleading to designers, if used for specification values. When a $\frac{1}{2}$-in. diameter test specimen is cast according to Methods Nos. 5 and 6 the results obtained are very high. The results obtained with the two types of specimens (Methods Nos. 5 and 6) can be manipulated by changing the diameter of the specimen. A 0.798-in. diameter specimen gives results much lower than results obtained with a 0.505-in. diameter specimen. **Mr. Webert**

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[1] The American Brass Co., Waterbury, Conn.

Method No. 5, a type of test specimen suggested by Mr. W. M. Corse provides a test specimen that will appeal to the foundryman more than any type cast separately. It can be attached to the casting and can be tested without machining. It remains to find a diameter to give results that are not too high and that will represent the average strength of gun metal castings.

Mr. Oller MR. JOHN OLLER.[1]—In regard to test bars for bronze alloys discussed in Mr. Dix's paper, I should like to suggest that Committee B-2 on Non-Ferrous Metals and Alloys give this matter further study along the lines of standardizing separate test bars for certain high-strength and high-shrinkage alloys. Investigation of the methods of casting test bars proposed by Mr. Dix, made in various foundries on the Pacific Coast, shows that the process of casting bars to size is not practical in a commercial foundry. The test results tabulated by Mr. Dix indicate no difficulties as to pin holes under the skin of bars, uneven diameters and other defects usually eliminated in machining of bars. This is probably due to the fact that all experiments were carried out in the same foundry under identical conditions. In commercial practice it will be found that very few molders are able to produce a satisfactory test bar cast to size without several trials, which is not always possible.

It is certain that test bars for high-strength bronze, such as gun metal, manganese bronze, Tobin bronze and phosphor bronze, give best results when cast as suggested by Mr. Dix (Fig. 11). This bar requires machining and may either be cast separately or attached to the casting. If cast separately it should necessarily be cast in sand under the same conditions as the main casting and not in an iron mold. For other alloys it is possible to use the test bars shown in Fig. 1. However, uniform results can only be obtained by machining the bar, even if it does eliminate material which is considered to give added strength.

The question of relation of strength of the casting and test bar will always be difficult to settle, since the relation naturally varies with the type of casting, the thickness of its different sections, etc. If the metal is tested there is obtained some indication of its strength, but the exact relation between bar and casting has not yet been determined.

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[1] Inspector, Pacific Gas and Electric Co., San Francisco, Calif.

Mr. Oller The notes in Mr. Dix's paper as to fracture of metal indicating its strength checks with facts observed by the speaker. If the fracture is uniform and shows a color typical for the individual alloy in question, a favorable test can be expected. If the bar shows a mottled fracture and a color not typical, it is safe to say that the test result will not be satisfactory. This is typical for the high-strength bronzes and can be relied upon as a preliminary test of material, but not as conclusive evidence. In making the fracture test it is common practice to cast a special bar just before pouring the heat, cooling it in water, breaking it and observing the fracture. After the casting has been poured and cooled it is also possible to break off a small piece, and even the appearance of fracture of the riser will indicate to some degree if the material has been properly mixed.

Mr. Patch MR. N. K. B. PATCH[1] (*Chairman, Sub-Committee III, Committee B-2*).—Mr. Dix has made some very valuable contributions to the information now available on the subject of test bars for the cast non-ferrous alloys. The accurate records kept, and the care with which the different steps have been made, make this information of immense value, and also point the way for further investigation which Sub-Committee III of Committee B-2 has in view.

The subject of the most satisfactory test bar is one open to much argument. Some contend that the bar should be representative of the metal in the casting; others contend that the bar should represent the best that the particular melt in question is capable of, so that one heat may be compared with another and a test bar of one foundry may be compared with that from another with some confidence in the accuracy of the comparison.

When comparing cast materials it is obvious that the normal variation between two melts is, of course, to be expected. The very process of casting must necessarily introduce a maximum of factors which have their influence on the product, and thus introduce variations. It seems to me, therefore, that a test bar should be designed so that as many variables as possible are eliminated. If this is the case, then of necessity a test bar which gives consistently the best figures for a given alloy is unquestionably that which has eliminated the most variables, and, therefore, is to be the most preferred.

Mr. Dix advises that methods Nos. 5 and 6 give figures too high to be considered and, therefore, has not carried this investigation any further on these methods. It seems only reasonable to point out that these bars were not machined, whereas the bars on some of the other methods were machined, and comparison is, therefore, not reasonable.

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[1] Works Manager, Lumen Bearing Co., Buffalo, N. Y.

Mr. Patch

Again, method No. 11 is pointed out as giving most uniform results. This method is very similar to Nos. 5 and 6. The bars tested from method No. 11 were machined. Is it not probable that methods Nos. 5 and 6 would have been found to compare very favorably with method No. 11 had those bars also been machined?

A machined bar, which has been well fed, obviously may yield comparable results with a bar not machined but poorly fed, since the skin of the unmachined bar may compensate for the lack of feed in the cast-to-size bar.

One of the values of test bars is that of indicating proper melting practice. A bar for a given alloy showing say 40,000 lb. per sq. in. tensile strength, cast by one foundry, compared with one showing 30,000 lb. per sq. in. cast in another foundry, but of like analysis, should be an indication that the melting practice in the first foundry is to be preferred to that in the second. However, such may not necessarily be the case. If the castings in the first foundry have been improperly fed and those in the second very carefully fed the castings from the second foundry are to be preferred, in spite of the low results yielded by the tests of the bar. It thus becomes necessary to have the very best of practice apply to the castings, and then the comparison of the bars is representative. Both foundries must use a like method of gating and casting the bars themselves. Mr. Dix has pointed out the errors that can arise from changes in method. He has proved that the fed test bar gives the most uniform results.

As to Mr. Dix's remarks in reference to the misleading of designers by high results on the test bars, is it not reasonable to have it understood in all specifications that the figures specified for test bars are to be considered as the ideal for that alloy and that test bars cut from castings of the same heat are not expected to yield on the average much more than one-half the figures for the ideal test bar?

In methods Nos. 1, 2, 3 and 4, as well as Nos. 8, 9 and 10, it is doubtful if the center of the bar is entirely well fed, which may account for the irregularities indicated.

Sub-Committee III of Committee B-2, which is studying the question of test bars, will welcome further suggestions and if any other investigator can bring any light on this subject it will be considered a favor if such information is made available to our subcommittee.

In the report of Committee B-2 there is shown a sketch of a test bar which has been suggested.[1] This bar has thus far indicated extremely uniform results in a wide number of alloys. It is capable

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[1] *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 178 (1923).

Mr. Patch of modification from the standpoint of economy, and will probably be so modified by the sub-committee.

Mr. Templin MR. R. L. TEMPLIN.[1]—We have had occasion in the last three or four years to make a number of green-sand cast-aluminum test specimens, and aluminum alloy test specimens, and in all this work we have used a form of test specimen which corresponds to method No. 2 as given by Mr. Dix, except for a slight variation in the gate. We have found this method to give very satisfactory results in the different alloys covered, which included pure aluminum, manganese-aluminum alloys, standard No. 12 alloy, other copper-aluminum alloys, copper-silicon-aluminum alloys, silicon-aluminum alloys, zinc-aluminum alloys, zinc-manganese-aluminum, and copper-magnesium-manganese-aluminum alloys. We found it was quite essential to have a generous radius from the gage section connecting the end of the test specimen; in fact we considered it necessary to have a radius of not less than $\frac{1}{2}$ in. I think that is larger than recommended by Committee B-2. We generally thread the ends of the specimen, but we do not machine the gage-length section which in our specimens is $\frac{1}{2}$ in. in diameter. We thread the ends simply to make sure that with the ball-and-socket attachment on our testing machines we can get a uniform and axial application of load, rather than depend on the wedge grips.

Mr. Jones MR. JONES.—I wish to say a few words in regard to machining specimens. I think it is essential to machine a test piece. When the metal is rising in the mold, there is a tendency for the dross and the impurities to cling to the sides of the mold, and the metal in that portion of the mold is not representative of the casting in a good many cases. As a rule, I would allow a certain amount of finish and machine castings for use. Why not give the test piece the same chance? I think it is very essential to machine test pieces.

Mr. Dix MR. E. H. DIX, JR.—I appreciate Mr. Jones' analysis of the methods of casting test bars, and I certainly agree with him very heartily, but I was just wondering how serious the strains were which were caused by the shrinkage of the metal between the pouring sprue at one end and the riser at the other, in a properly rammed mold. I think that the actual results which have been obtained in this investigation, which I think are as uniform as one may hope to obtain on sand-cast alloys, indicate that perhaps the shrinkage is not as serious as theoretical considerations would indicate.

Mr. Jones MR. JONES.—I performed an experiment once to ascertain this fact; it was just a single test with a very high shrinkage alloy, man-

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[1] Chief Engineer of Tests, Aluminum Co. of America, New Kensington, Pa.

Mr. Jones

ganese bronze. I made a test piece 36 in. long, having a very large riser at one end and a large pouring gate at the other. The sand was rammed fairly hard. That casting was well fed, but was a very poor test piece. I pulled the bar throughout its entire length, leaving only enough at the ends to take care of the grips. This experiment was in part the basis of my statement that shrinkage should always be taken care of.

Mr. Clamer

MR. G. H. CLAMER.[1]—There is a broad principle involved in connection with the discussion of this whole matter of the relative value of a test bar as showing the quality of the metal in the casting. The discussion has been going on for at least twenty years to my knowledge. It is generally agreed by everybody, I believe, that the test bar never does indicate the quality of the metal in the casting, and furthermore that test bars from various portions of the casting, depending upon its cross-section, may vary as much as 50 per cent in strength. Mr. Dix states in his paper that in general the metal in the casting may be taken as 50 per cent of the value of the metal in the test bar. Now that 50 per cent might have been 25 per cent or 75 per cent or only 10 per cent, depending on the kind of casting, its cross-section, the way it was fed and many other factors. I think it would be a very good step forward if this Society would make some definite recommendations in the premises. Can we not come to some agreement as to the value of the metal in the casting itself, compared to that in the test bar? Tomorrow there will be held a joint meeting of Committee A-3 on Cast Iron and a committee of the American Foundrymen's Association, at which our arbitration bar for cast iron will be discussed. There will probably be some research work carried on by the American Foundrymen's Association within the next year or two on that subject, and I think it would be highly desirable if this Society could once for all reach some agreement in this important matter.

Secretary-Treasurer

THE SECRETARY-TREASURER.—I heartily endorse Mr. Clamer's remarks and would urge this matter upon the Society as an important one for its consideration. Whatever the final outcome, I think our specifications should be made clear. Mr. Patch, for example, has asked if it is not reasonable to have it understood in our specifications that the specified strengths for test bars are to be considered as the ideal, and that bars cut from the castings themselves are expected to show not much more than one-half of these values. The point is that our specifications at present give no indication that this is the case. Take, for example, a specification that has been under dis-

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[1] First Vice-President, Secretary and General Manager, Ajax Metal Co., Philadelphia, Pa.

Secretary-Treasurer cussion this evening, our specification for gun metal (B 10 - 18). These specifications require that the alloy shall conform to certain minimum requirements as to tensile properties, namely, 30,000 lb. per sq. in. tensile strength and 14 per cent elongation. So far as I can see from reading the specifications the purchaser could not reject castings for which the test bar conformed to these requirements (all other requirements having been met), although the discussion this evening seems clearly to indicate that if the test bars are close to the minimum requirements of our specifications, the castings themselves may show considerably lower strength than called for by the specifications. In this way our specifications are apt to be misleading to the designer. Mr. Campbell, discussing the report of Committee B-2,[1] states that at the Navy Yard in New York they look with a good deal of suspicion upon gun-metal castings for which the ordinary test bar does not show a strength of 35,000 to 40,000 lb. per sq. in. and an elongation of 20 to 30 per cent. Should not such facts as these be more clearly reflected in the Society's specifications?

I may add that this matter is not at all confined to the non-ferrous metals. We are frequently asked to interpret such matters as these in our specifications for steel and iron castings as well as for the non-ferrous metal castings. There seems to be here a very definite problem for our committees to study.

Mr. Lynch MR. T. D. LYNCH.[2]—I should like to say a few words in addition to those of Mr. Clamer and the Secretary. I wish to emphasize the need for this Society to standardize methods of testing various alloys. We may find that more than one form of test specimen will be necessary to cover the great variety of non-ferrous alloys, but I do think that at least one test specimen representing a certain definite alloy should be agreed upon by Committee B-2 during the coming year in sufficient time to be recommended for adoption next year.

Mr. Patch MR. PATCH.—Some years ago at one of our conventions, the subject of the ratio of average strength of castings to strength of test bars was discussed, and it was shown that a more or less reasonable ratio to use was 50 per cent. That was along the line of Mr. Clamer's discussion, and I agree with him that it would be a very valuable thing if this Society could lay down such a rule; but it seems to me that Sub-Committee III will no doubt have some form of recommendation along that line as soon as they have the results from the investigations now in process. There are at present eleven different investigators making a series of tests on several alloys with the bar suggested by the sub-committee. **Mr. Patch**

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[1] *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 189 (1923).

[2] Manager, Materials and Process Department, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.

In answer to Mr. Lynch, the use of special test bars for each of the different alloys is a matter still to be determined. If a standard test bar for all alloys is found to be impossible, we have hopes that such a bar may apply at least to many of the non-ferrous alloys. If that proves to be impossible, then the sub-committee will offer special test bars for each of the different alloys.

THE INFLUENCE OF THE RATIO OF LENGTH TO DIAMETER IN THE COMPRESSION TESTING OF BABBITT METALS[1]

By John R. Freeman, Jr.,[2] and Paul F. Brandt[3]

Introduction

Babbitt metals are practically always used where they are subjected to compressive stresses and for this reason a knowledge of their compressive strength is important. Many data have been published on this property, but there is considerable variance in the methods used to determine it, particularly with regard to the size and shape of the test specimen. For example, in the appendix to the Tentative Specifications for White Metal Bearing Alloys of the American Society for Testing Materials[4] there is given a table showing the physical properties of the proposed standard compositions. The compressive strength in this case is expressed in terms of the deformation of a cylinder of the metal 1¼ in. in diameter and 2½ in. high, at three loads: namely, 1000, 5000 and 10,000 lb. The ratio of length to diameter for these test specimens is then 2 to 1. Commercial testing laboratories often use a 1-in. diameter specimen 1 in. high, which has a 1 to 1 ratio, and several investigators have published compressive strength values obtained on specimens which were 1-in. cubes.

The standard methods of the Society for the mechanical testing of metals[5] recommend for compression testing, a specimen 1 in. in diameter and a length between 2.5 and 4 diameters, but preferably 3 diameters. Freeman and Woodward,[6] in their tests of white metal bearing alloys at elevated temperatures, adopted the 3 to 1 ratio recommended but used a ½-in. diameter specimen 1½ in. long. Tests reported in a later paper by one of the authors[7] showed that a specimen with the same ratio of length to diameter, but 3 in. long and 1 in. in diameter, gives substantially the same results.

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[1] Published by permission of the Director of the U. S. Bureau of Standards, Washington, D. C.

[2] Physicist, U. S. Bureau of Standards.

[3] Laboratory Assistant, U. S. Bureau of Standards.

[4] Tentative Specifications for White Metal Bearing Alloys (B 23-18 T), *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part I, p. 538 (1920).

[5] 1921 Book of A.S.T.M. Standards, p. 847.

[6] John R. Freeman, Jr., and R. W. Woodward, "Some Properties of White Metal Bearing Alloys at Elevated Temperatures," U. S. Bureau of Standards *Technologic Paper No. 188.*

[7] John R. Freeman, Jr., "The Physical Properties of the A.S.T.M. Tentative Standard White Metal Bearing Alloys" Appendix to Report of Committee B-2, *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 207 (1922).

In view of these differences in testing practice and the lack of definite knowledge of the influence of the ratio of length to diameter of a compression test specimen on the values obtained in testing, the U. S. Bureau of Standards has made a series of tests to determine the influence of the ratio of length of compression specimens to their diameters on the results of tests of babbitt metals. The results of these tests are presented in this paper.

TABLE I.—COMPOSITION OF ALLOYS AND POURING TEMPERATURES

Alloy No.	Chemical Composition, per cent						Pouring Temperature, deg. Cent.
	Tin	Lead	Copper	Antimony	Arsenic	Iron	
1[a]	2	83	...	15	....	....	330
2	88.8	0.06	3.9	7.2	....	under 0.10	430
3	88.7	0.71	2.7	7.9	....	under 0.10	430

[a] Not analyzed.

TABLE II.—CONDITIONS OF TESTS

All specimens are 1 in. in diameter

Series	Length of Specimen, in.	Ratio of Length to Diameter	Gage Length, in.	Ratio of Gage Length to Diameter
A	3	3 to 1	3	3 to 1
	2	2 to 1	2	2 to 1
	1	1 to 1	1	1 to 1
B	3	3 to 1	$2\frac{1}{2}$	2.5 to 1
	2	2 to 1	$1\frac{1}{2}$	1.5 to 1
	1	1 to 1	$\frac{1}{2}$	0.5 to 1
C	3	3 to 1	$2\frac{1}{2}$	2.5 to 1
	3	3 to 1	$1\frac{1}{2}$	1.5 to 1
	3	3 to 1	$\frac{1}{2}$	0.5 to 1

SPECIMENS

Typical lead and tin-base white-metal bearing alloys were used for this investigation; the compositions are given in Table I.

The alloys were cast, at the pouring temperatures given in Table I, into chill molds 4 in. long by $1\frac{1}{2}$ in. in diameter, and the test specimens were machined from these castings.

TESTS

The compression tests were made in a 10,000-lb. Riehle testing machine with the specimens supported on a spherical seat. The compressometer used was a modification of one used in previous tests[1] of white-metal alloys at the Bureau. The modified form permitted the measurement of deformations of 0.0001 in. over gage lengths of ½, 1½, and 2½ in.

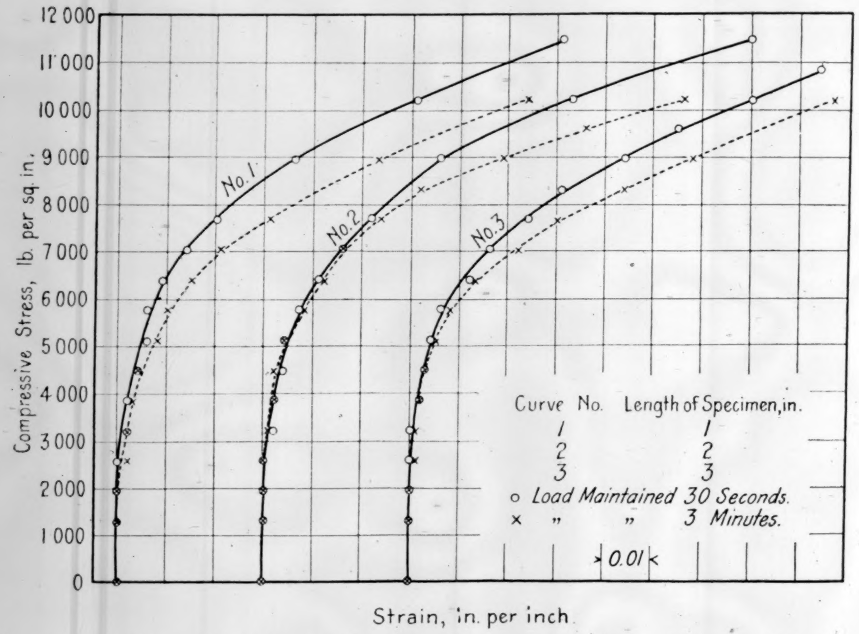

FIG. 1.—Compression Tests of Lead-Base Babbitt Metal (Alloy No. 1), Series A

Three separate series of tests were carried out, which will be designated as A, B, and C, as given in Table II.

Series A.—The compressometer was not used in this series, but the total length of specimen before and after loading was measured with a vernier caliper. The specimens were loaded in increments of 500 lb., and after each increment the specimen was removed from the testing machine and its length measured. For one group of the series, each increment of load was maintained for 30 seconds and for a second similar group, 3 minutes.

Series B.—In this series the deformation was measured with the compressometer. Load-deformation curves were obtained on specimens of each alloy, 3, 2 and 1 in. in length, over gage lengths of $2\frac{1}{2}$, $1\frac{1}{2}$, and $\frac{1}{2}$ in., respectively. The compressometer was always mounted symmetrically on the specimen with respect to the length, by placing the plane of the gage points of the upper and lower frames for each gage length $\frac{1}{4}$ in. from the end of the specimen.

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[1] John R. Freeman, Jr., and R. W. Woodward, "Some Properties of White Metal Bearing Alloys at Elevated Temperatures," U. S. Bureau of Standards *Technologic Paper No. 188.*

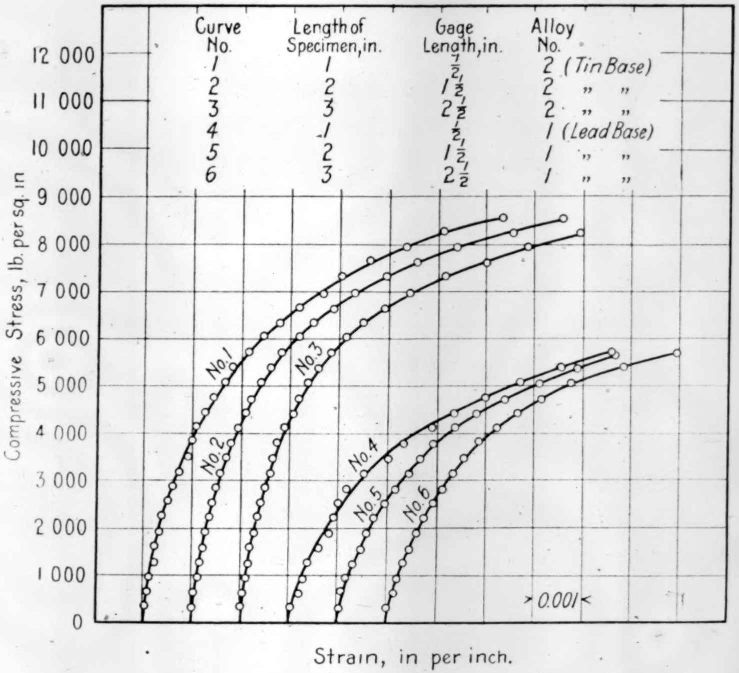

FIG. 2.—Compression Tests of Tin-Base (Alloy No. 2) and Lead-Base (Alloy No. 1) Babbitt Metals, Series B

Series C.—This series of tests was similar to series B, but in this case the length of specimen was constant at 3 in., and the deformations measured as in the previous series over $2\frac{1}{2}$, $1\frac{1}{2}$, and $\frac{1}{2}$ in. gage lengths.

RESULTS

The results of all tests are incorporated in the curves given in Figs. 1, 2 and 3. The ordinate in each case is the load in pounds per square inch and the abscissa the deformation in inches per inch. At least two independent tests were made for each of the individual conditions of test outlined in Table II, and the curves plotted in the figures are the averages of the two or more curves obtained for each condition of test.

The curves in Fig. 1 show very clearly the effect of time in the compression testing of babbitt metal, the deformation obtained for a given load applied for 3 minutes being greater than the deformation obtained if the load is applied for 30 seconds; but this is true, within the experimental error, only when the load applied is greater than the load which produces an appreciable yield.

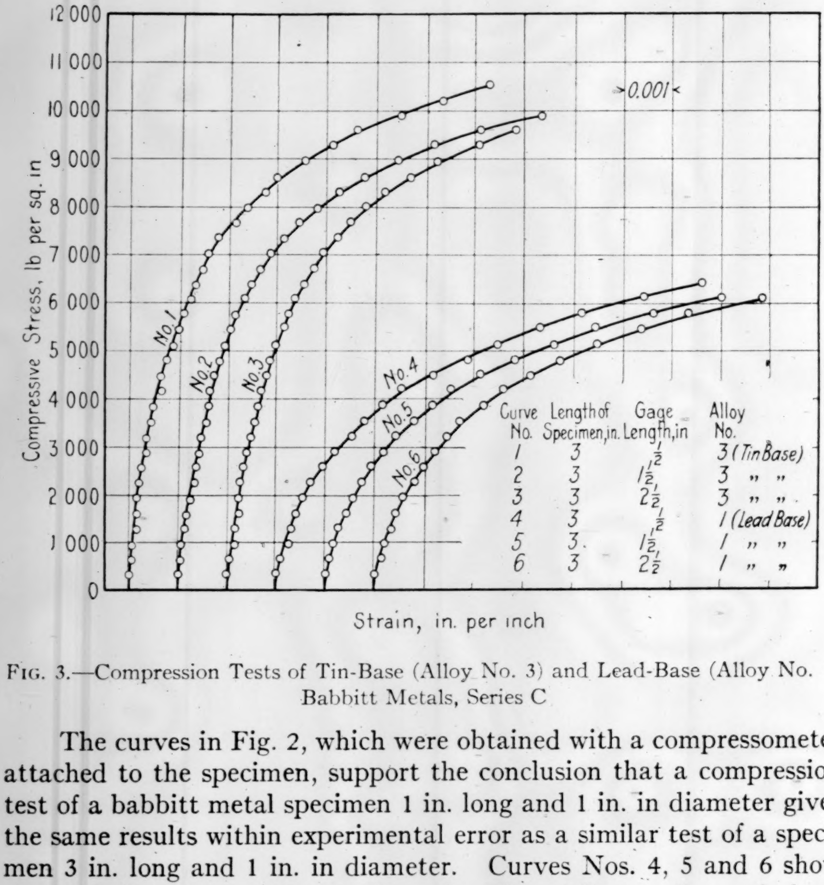

FIG. 3.—Compression Tests of Tin-Base (Alloy No. 3) and Lead-Base (Alloy No. 1) Babbitt Metals, Series C

The curves in Fig. 2, which were obtained with a compressometer attached to the specimen, support the conclusion that a compression test of a babbitt metal specimen 1 in. long and 1 in. in diameter gives the same results within experimental error as a similar test of a specimen 3 in. long and 1 in. in diameter. Curves Nos. 4, 5 and 6 show that this fact is equally true for a lead-base alloy as for a tin-base alloy. In fact, the compression test curves of any one type of alloy obtained on 3, 2 and 1-in. length specimens may be superimposed one on the other and all relative points which make up the several curves will coincide within experimental error.

The third series of tests (Series C), the results of which are plotted in Fig. 3, were made to determine the variation, if any, in the unit deformation of a 3-in. specimen over its length, and thus determine any appreciable end effect. The curves obtained show that the unit deformation in the central $\frac{1}{2}$ in. is the same within the limits of measurement as over a $2\frac{1}{2}$-in. length of the specimen and that this is true for both tin-base and lead-base alloys. It is also of interest to note that the curves for the lead-base alloy (Nos. 4, 5, 6), are identical with curves Nos. 4, 5 and 6 of Fig. 2, which were obtained on the same alloy and over the same respective gage lengths, but in which the length of specimen was varied. Curves No. 6 should of course be identical since the length of specimen and gage length in each case are the same.

CONCLUSIONS

It may be concluded from the results obtained that the total deformation in a compression test of babbitt metal is a function of the length of time the load is applied, but for loads below appreciable yield the differences in amount of deformation are negligible when the loads are applied for 30 seconds or for 3 minutes.

For loads up to and greater than those causing appreciable yield, the stress-deformation curves are identical, within experimental error, for ratios of length to diameter of test specimens between 3 to 1 and 1 to 1 for specimens 1 in. in diameter.

The unit deformation along the central $\frac{1}{2}$ in. of a 3-in. specimen is the same as along the central $2\frac{1}{2}$ in. length of the specimen.

The above facts are true for both tin-base and lead-base alloys

SYMPOSIUM: WHAT PROPERTIES OF AND METHODS OF MAKING CONCRETE REQUIRE FURTHER INVESTIGATION?

A Discussion of the Requirements of Concrete to Meet the Many Varieties of Service Conditions—What Can Be Learned from Behavior of Actual Structures?

FOREWORD

The behavior of out-door concrete is to-day a subject of especial interest and significance to the engineer, contractor and owner of concrete structures, and to the general public as well. Its importance has been recognized by the Society's Committee C-9 on Concrete and Concrete Aggregates in the study now being given to the conditions affecting the durability of concrete in structures. It has seemed desirable, therefore, to hold this Symposium and thus to provide opportunity, under the auspices of the Society, for enlightened discussion and constructive criticism of concrete in structures. The introduction will sound the keynote of the discussion. The constituents of concrete and methods of making will be considered in the light of service requirements, followed by a discussion of the behavior of concrete under various service conditions.

The underlying purpose of the entire Symposium is to arouse engineers to the need of further intensive study of concrete as a material.

INTRODUCTION

BY FRANK C. WIGHT[1]

This is a stock-taking discussion. It is an attempt to get away from the view of concrete as a combination of cement, sand, stone and water which possesses certain physical properties to be studied and to look at it in detached fashion as a material to perform a service.

For twenty-five years concrete has been rapidly increasing as a material of construction and with that increase there has naturally been an improvement in the technique of its manufacture. It is, however, a complex material, most susceptible to variation in the constituents and the process and subjected to a variety of service conditions. The tendency in investigative work is to concentrate on one or, at best, a few of the variations of constituents and to assume at least an approach to constancy of service conditions. As a result of the methods of testing, our standards of practice in making concrete are a combination of the various deductions resulting from the concentrated constituent assumptions. This, of course, is the standard method of applying tests to practice, and in the art of making concrete has yielded admirable results. On the other hand, the over-emphasis of strength as the main requirement to meet service conditions may have resulted in a one-sided view of the material. In this discussion it will be the aim, therefore, to see concrete as a material to meet many types of service, among which its ability to withstand load is but one.

The discussion is, in effect, a start on a *deductive* study of concrete as a material. No other structural material has had so great or so diversified an *inductive* study. Dozens of laboratories and hundreds of investigators have made millions of tests which have been duly reported and classified to form the basis for a gradually developing science of making concrete. There is no intention to decry these tests; they are the groundwork on which is based the undoubted improvement in structural concrete. But they are after all tests, which is to say, simulation in miniature of the conditions of material, manufacture, exposure and load to which the actual structure is subjected. Meanwhile there are in existence thousands of such actual structures, made of varying material under varying conditions of manufacture, exposure and load, whose behavior may be a matter of observation but in only a few cases is a matter of record. In our interest in the test we are in danger of losing sight of the end for which the test is planned. We are failing to record the behavior of the actual structure and to tie up such behavior both to the tests and to the facts that these tests indicate.

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[1] Managing Editor, *Engineering News-Record*, New York City.

It is more important that this record of actual structural behavior be studied in concrete than in any other material. Concrete is a chemical combination subject to all the variations in structure that that implies. To be sure, both steel and wood are chemical combinations, but the reactions in the former are so slow as to be negligible and in the latter obvious and well understood. Concrete, on the other hand, is an obscure compound, the chemical nature of which is debatable, and whose reactions are neither understood nor measurable. Nevertheless, these reactions take place and their occurrence continues over an unknown length of time. It is not beyond the possibility of belief, for instance, that there is some truth in the claim that there is a critical age in concrete—a period somewhere between ten and fifteen years, maybe more, maybe less—when there occurs a subtle change in its nature. At any rate, changes in placed concrete do occur. It is not like a Venetian glass, where perfection or lack of perfection is assured when it cools in the mold. Assuming a 100-per-cent perfection, those concretes which would fall in the lower 60 per cent, say, may be discovered when the forms are removed, but in the upper range only time and the attacks of time will serve to distinguish between the 60-per-cent and 90-per-cent perfect.

This behavior of actual concrete structures, therefore, has seemed to the Society's Committee on Papers to be of exceptional interest in the study of concrete as a material. The inductive tests and studies have led to a number of theories and principles of the manufacture and placing of concrete, the best of which are embodied in modern practice. It is firmly believed that adherence to these principles will result in a stronger and more lasting concrete, but unfortunately only time can tell whether this is a fact, always assuming that there is no readily discoverable dead line in quality, above which concrete will be everlasting and below which it will be of the most temporary nature. Five years from now possibly, better ten or fifteen years from now, we can observe concrete structures placed under recorded conditions and check up on the principles that governed their manufacture. One purpose of the present discussion is to move the clock back ten or fifteen years, to attempt to discover from the behavior of existing structures of that or greater age what were the properties and methods of making that affected their present known condition, be it good or bad.

It is recognized that this will not lead to a complete answer to the question "How can we make the perfect concrete?" It is recognized that conditions ten or fifteen years back are hard to discover, and that the elements entering into both manufacture and wear are many and diverse and that deductions made from the discoverable data are not incontrovertible. But it is hoped that by building up enough data there may appear some common facts which can be related to common results and which will be useful in the development of future practice.

Only those blinded by prejudice can fail to admit that a great deal of existing concrete falls far short of the expectations of its designers in appearance and in structure. Most of this concrete was satisfactory when the forms were first removed. It is probable that the builders when they left the job were convinced that they had turned over to the owner a structure that would continue in the acceptable condition they left it. For some reason or other there has been change with the passing of time—not change due to unexpected or undue load or wear, but change due to the natural exposure of the elements and the natural reactions in the concrete itself. To conceal such examples is to play the ostrich. They are usually where he who runs may read and as unexplained horrible examples they merely serve to deter the uninformed owner or prospective owner from accepting concrete as a suitable building material. Studied, however, and subjected to the light of criticism, they may reveal basic errors which can be avoided in future.

The scheme of the discussion is first to consider the constituents of concrete and its methods of making, in the light of the service requirements, to discover if possible wherein lie future possibilities in cement, aggregates and proportioning and mixing. This is to be followed by a study of service conditions in roads, in sea water, in alkali water, and under the normal attacks of weather, tying up the behavior in so far as possible to the kind of concrete actually placed. The discussion is not expected to close up the subject at once. It may, indeed, reveal only what we already know. But it is hoped that it will be the beginning of an addition to the study of concrete the material, a deductive study which, added to the inductive study, thoroughly covered by the many tests reported before this and other societies, will serve to standardize and elevate the practice of concrete making and placing.

WHAT PROPERTIES OF AND METHODS OF MAKING CEMENT REQUIRE FURTHER INVESTIGATION?[1]

By P. H. Bates[2]

As a text or a thought around which to write this partial contribution to the general topic of to-day, we might change the general title by substituting the word "cement" for "concrete" and have "What Properties of and Methods of Making Cement Require Further Investigation?" Our experience has been such that we are not well equipped to discuss the second part of this title, but it forms such an important part that at least some thoughts must be expressed in this connection. To discuss needed physical properties without taking into consideration how these would affect present methods of production or to omit a discussion of the investigations needed in production methods, would be possibly leaving the most important part of the subject untouched.

Portland cement must have certain properties to meet certain service conditions. These latter have been well condensed in the program of this meeting into six broad services or conditions of use, namely, roads, sea water, alkali, atmospheric, mass concrete and reinforced concrete. For the purpose of discussing the required properties of cement, we are going to further condense these to three conditions under which resulting cement products will be used: namely,

> *Class I.*—Those atmospheric conditions in which the product will not be subjected to actual contact with water after making, but be subjected to only the water vapor present in the air. In this class of products we have reinforced-concrete buildings and mortars used on the interior of structures, especially as the top coat of floors.
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> *Class II.*—Those conditions in which the product is subjected continuously or intermittently to contact with water of a fair purity or low in solids composed largely of sulfates. In this class we find the greater use of cement, for it comprises such uses as in roads, mass concrete, stuccos, sewers, drain tile, etc.
>
> *Class III.*—Those conditions in which the product is subjected to the action of a solution of salts, especially sulfates. In this class appear concrete used in sea water and concrete used in those arid regions where the soils contain considerable amounts of sulfate salts, as our "white alkali" regions of the West.

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[1] Published by permission of the Director of the U. S. Bureau of Standards.

[2] Chemist, U. S. Bureau of Standards, Washington, D. C.

There are of course certain cases where we might consider overlapping in these classes, as, for instance, the roads in our dry southwestern country where there is little or no precipitation of water and where the humidity is far less than in the northern and eastern part of the country; or the concrete in certain sewers where the salt concentration may be so high that we would have to consider it as being subject to salt action. But in general these three classes represent the major conditions in which cement products are used and for which the cement should have such properties as would enable it .to give adequate, if not permanent, service.

Before becoming too much involved in this discussion, it were better to elaborate upon what we should expect of cement in its various products—adequate or permanent service. Are we not fundamentally wrong in saying or hoping that cement products should be even approximately permanent? We are, of course, inclined to make those odious comparisons with structures of previous civilizations around the Mediterranean Sea and to refer to them as having withstood the ravages of the ages and to our structures as being on a rapid road to destruction. Then we have the frequent comparison of cement products with stone, the latter being considered the acme of permanence in our climate. But these comparisons are most unfortunate. Let us move away from that most salubrious climate existing around the Mediterranean or the continuation of that belt directly easterly or westerly around the world, and examine a few old structures and what do we find? We will just cite one or two instances from a note in *Science* recently:[1]

> "Every geologist from his own observations and reading can give many examples of rapid weathering, such as that on the western front of the Amiens Cathedral, probably not an original stone placed there by the builders in the fifteenth century can be found;[2] that the outside stones of Westminster Abbey have been renewed five times over; that the stone of which the British Houses of Parliament are built has crumbled so rapidly that already it has been necessary to replace many of the stone ornaments with cast iron. On the other hand, so many objects showing almost no evidence of weathering have been taken from tombs in Egypt where they were buried for many centuries, that little surprise was evinced when the Metropolitan Museum Expedition of 1919–20[3] announced the discovery at Thebes, in the chamber of the tomb of a man of great wealth, of a large number of remarkably preserved small wooden models . . . 'practically as perfect as the day they were made' . . . but so little have they been affected by the agents of the weather that even the finger and thumb prints of the men who carried the figures up to the tomb are preserved . . . In the

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[1] Hudman F. Cleland, "Weathering under Constant Conditions," *Science*, Vol. LVI; 1458, p. 660 (December 8, 1922).

[2] J. W. Gregory, "Geology of To-day," p. 91.

[3] *Bulletin*, Metropolitan Museum of Art, Vol. XV, December, 1920, pp. 12–40.

cavern of Tuc D'Audoubert . . . in the Pyrennes, is an even more remarkable example of the lack of disintegration. In this case are the clay models of bison which are in nearly as perfect condition as when made. . . . These models of bison are in fact slightly cracked but with this exception are unchanged. It is possible that the cracks were formed within the first few weeks after the figures were modeled and that none have developed in the 18,000 to 30,000 years that have followed. . . . The conditions under which the art of paleolithic man has been preserved almost unchanged for thousands of years are to be found in uniform temperature, lack of sunlight and absence of circulating ground water. . . . It is probable, however, that the moisture content of the air has remained constant and that because of this, the clay models have not disintegrated. The remarkable preservation of the Egyptian models of wood . . . is due to uniform temperature, lack of sunlight and absence of moisture."

We see immediately from these typical cases that stone is not permanent, although any geologist would have told us that our kaolins are disintegrated granites and would have referred us to water in certain places as being very "hard" because of the dissolved limestone which it may contain. We also see that uniformity in atmospheric conditions is the greatest aid to permanency of all products—even modeling clay and wood—and that belt extending around the earth's surface and of which the Mediterranean is a part is one of few atmospheric changes. Hence, let us acknowledge that in cement and its products we are only properly concerned with a relatively permanent material and "relatively" may mean anything from a few years, or even months, to a hundred or more years depending upon the service condition. But in all cases we are concerned in a material which will give such adequate service, be it months or years, as coupled with its cost of installation and superiority of resulting product will justify its use.

CLASS I CONDITION OF EXPOSURE

The uses indicated in our Class I may seem to be the least severe of those to which cement may be placed. But even if such is the case, it is quite evident that we are in need of much research before we have the ideal cement for such purposes. It is well known that if we exceed a certain water-cement ratio we have a distinct falling off in strength and a decided increase in the amount of shrinkage which takes place during the early aging of any cement product. But it is just as well known that the optimum ratio must be exceeded if we are to take advantage of that flexibility in the use of cement under those conditions which have been the main features in its rapid advance in the structural industry. Otherwise we cannot economically place the concrete in large structures or in the complicated forms or properly around the reinforcing. We must make a compromise therefore which entails the use of more water than we should use and consequently in certain ways—strength and shrinkage—obtain an inferior product, although in other ways it is superior. But what has been done, so far as the cement is concerned, to develop a cement which would possess a minimum strength reduction and a minimum increase in shrinkage with the use of increased amounts of water? We have not made any thorough or even preliminary investigation to see how the different brands of cement vary in these respects. It has been rather a marked fault with investigators, in concrete in particular, to use a blend of several cements and to obtain, consequently, average results. It is now time to investigate the quality of brands exhaustively, not only along those lines which would indicate their serviceability under Class I conditions but along any lines which would show the characteristic of different types of cement. This would form but a preliminary step in a research tending toward developing a cement affected in the least by the use of large quantities of water.

Very closely connected with the question of excess water used to produce readily flowing concrete is that of a more "workable" one obtained without the use of water in excess or admixtures of any kind. The need of a cement capable of producing "fat" mortars or readily "working" concrete has been the reason for the existence of a number of materials to be used as admixtures to concrete to produce this quality. The existence of these materials has furthermore been the reason for a number of series of researches tending to prove or disprove the conferring of these qualities. But how many researches have we in which it has been attempted to find if we have any cements more strikingly "plastic" than the others? Indeed, we have no method of determining this quality even relatively, yet when used as a bond in mortars, this quality is far more paramount than strength. With possibly one exception, there are no mortar bonding agents sold in competition with portland cement on a strength basis, but they are always sold and used on the basis of being more readily workable.

When cements are used in the top coats of floors, we come into a field where there has been more adverse criticism of it and possibly more "patent medicines" evolved to correct its defects in this respect, than in all other fields excepting possibly that where it is supposed to be impermeable to water. All floor finishers will say that with all cements there are occasions when a non-dusting, non-cracking floor is obtained and that with certain brands the likelihood of obtaining a good floor is far greater than with others. But they are seldom sure when the desired result will be obtained, and no one has developed a test or tests that will predict this quality. A research that would show just what constituent or combination of constituents in the cement, or what phenomena in the reaction of water on these, or what physical properties of the cement, as hardness of particles or fineness of grain, produce the expected and hoped-for result, would not only enable cement to regain a favor that it has lost in this use but would also reduce the cost of floors and materially increase the use of portland cement for such purposes.

It is believed also that this same research would solve the problem confronting road builders where or when a very low atmospheric humidity exists. A number of instances of trouble have been reported of roads cracking under these conditions before final set has been obtained—and the trouble has not been due to excess water or poor choice of aggregate or poor grading of aggregate or poor workmanship. *The difficulty has been with the cement.* Here again our tests are at fault. We store all test pieces of cement under close to saturated humidity conditions before and during the time of set. But much cement has to set in an atmosphere under considerably less than 50 per cent relative humidity and cannot be protected from evaporation during this period. If you are not familiar with what will frequently happen under such conditions, make duplicate pats from time to time and store them under low humidity as well as saturated humidity conditions. It will be surprising to note how often those specimens proving satisfactory under standard conditions of testing fail completely in the dry atmosphere. Also then remember that the cement must set in the dry atmosphere. Almost unquestionably the same set of phenomena that causes this cracking of roads before setting in dry atmospheres brings about the dusting and checking of floors.

Before leaving this class of uses we should like to call attention to the need of the study of volume changes in concrete when subjected to the atmospheric conditions defining this class. It has been our duty to examine the veneering of a large number of steel and concrete frame structures and we have been surprised at the relatively larger numbers of failures of the veneer on the concrete than on the steel structures. Architectural terra cotta, stone, and brick veneer show failures of a type that lead almost conclusively to the deduction that the concrete by contracting has induced strains in the veneer that have crushed it, especially when setting of the latter has been faulty due to the lack of fully embedding the individual units in the mortar. There has resulted from this combination of contracting and faulty setting a concentrated loading, especially on the edge of the units where pointing has been carefully done, which is manifested by a spalling at the edges. What causes this contraction—the aggregate, the cement or the water? The majority answers, the water. But excess water must be used for proper placement of the concrete. Hence, are we not back to the development of a cement or the adding of something to a cement after making, which will enable it to hydrate more fully in a given short time and retain this excess water? Furthermore, this water of hydration must be held so tenaciously that it will not be lost by desiccation under the usual atmospheric conditions. Our present "soundness" tests are supposed to determine "constancy of volume," but this is only very relative and not near to the degree of accuracy that we must have and will ultimately have for certain types of work.

CLASS II CONDITION OF EXPOSURE

In our Class II are embraced those uses of concrete into which the majority of cement ultimately finds its way. It is here also that cement has been eminently successful, but no one will question the value and need of research in still further improving it to meet the conditions of service existing here, for they are in many cases very exacting. A road saturated with water may freeze and in this condition be subjected to the action of a warm sun on one side while the other remains well below freezing. Or consider the condition of a sewer subjected to the action of water at practically all times and yet somewhat more than 50 per cent of the cement is soluble in water. A thoroughly dry stucco may suddenly be subjected to a heavy or long rain and while saturated be frozen. Such conditions as these are extremely severe, and if there were not universal success there should be no cause for wonder.

But note that the conditions here are somewhat different from those existing in the first class. In the latter the concrete is subjected to continued desiccation. Here it is subjected to either the continued influence of water or alternate periods of wetting and desiccation. It is assuming rather much to infer that a cement which will be successful under the first condition will necessarily be just as successful under the second. In fact no other structural product is available which is as adaptable and as flexible in use as portland cement, which will approach it in being so successful under these two widely different conditions. That we do need studies for making cement still more adaptable is realized when we examine roads or massive retaining walls or foundations or stuccos. In the first and last instances, we are confronted very often with a large number of shrinkage cracks which furnish a very unsightly appearance and would seem to mark a condition which may be conducive to ultimate failure. Careful use of the proper aggregates and careful placing unquestionably reduces this condition, but it occurs even under the most careful cases of application so frequently that it must be considered as produced by some inherent quality of the cement.

Again, there is no question but that if we made an intensive study of the materials we are using in these cases and the conditions under which we are using them and the conditions of service under which they are used, we would be on a fair way towards eliminating these faults. But suppose we have referred to us a bad case of cracked road or checked stucco and it is necessary to make a diagnosis of the trouble. Let us examine the data which we will have available to work upon. First, we are told that the cement passed the standards of the Government and of this Society. That is good so far as it goes, but actually it goes only so far as to lead us to say, in racing parlance, "cement was left at the post." It should not be inferred that we are decrying the standards. We are simply stating that for the purpose at hand they are inadequate and they are so because they are not designed to cover service under all conditions. Second, we may have furnished us a granulametric analysis of the aggregate. This again is good, but what does it represent? A sample taken during the preliminary survey of suitable material to enable a choice to be made of the best aggregate or the average of a few analyses made during the course of the work or even, in extreme cases, the result of daily analyses. In any case, what was done with the analyses? Was any regrading made, or, more vitally, were any tests made to find out how the aggregate, as available or regraded, acted in respect to volume changes during setting and hardening? Third, how much water was needed for a good consistency and how much was actually used; not how much was the foreman told to use but how much did he use *by actual measurement?* Other data of course are needed—temperature, humidity, precipitation, etc.—but when we come to use the data, invariably laboriously collected, it is found to be so decidedly lacking in one respect or another that deductions of uncertain value can only be made. Acknowledging the existence of many jobs which have given and are giving adequate service, we are confronted with those that have not; and for the elucidating of the reasons for the failures of these a series of observations should be made on a number of jobs in course of installation by trained testing (not constructing) engineers. These should be supplemented by a series of tests on materials as used, and by a number of corollary tests to determine what would happen if they were used in other manners and under other conditions. These tests must be more exhaustive than now used. Our "standard" tests for cement will not tell the whole story. The character of the grading of the aggregates and strength tests of the concrete are also insufficient. Our present methods only very roughly approximately enable us to predict the suitability of a concrete for any particular use.

Some time must be devoted to concrete in foundations, tunnel linings and other cases where it may be subjected to percolating waters. Briefly, concrete through which water *constantly* percolates is doomed, but only locally at that point where the percolation takes place. We can deduce this from the conditions in any large limestone section of the country, where in many cases streams of water, once on the surface, are hundreds of feet underground, and as a consequence of which, we have the many beautiful caverns sometimes characterizing such locations. Hydrated cement, the matrix of concrete, is just as soluble as limestone, in fact it is more soluble. It has been found by sad experience that water must be kept out of concrete and it is being done on all large jobs. With the prevention of the water percolating through the concrete, the latter is giving adequate service. But cement in itself can not be so changed as to make the concrete impermeable. Here is a condition which requires the joint proper use of all the ingredients of concrete, cement, aggregate and water, and when so used an impermeable concrete can be obtained.

CLASS III CONDITION OF EXPOSURE

Concrete when subjected to the action of the solution of certain salts, especially sulfates, has been found to give a service which has been adversely criticised. Yet we are compelled to believe that the same relative degree of service is obtained under these conditions as are obtained in certain other cases where the life of concretes has been the subject of attack, that is, it gives a far greater service and return on the investment of installation than any other material that might be substituted for it at less, equal or greater cost. Here is a case where concrete would not be so severely criticised were it not for those adherents who insist that it will be permanent, but who should only more rightly insist that it will be more permanent than any competitive product.

It is not within the scope of this paper to refer to any of the many papers or investigations which have been given to the public discussing the matter of the corrosion of concrete by sea or "alkali"[1] water. Many of these are but useless wastes of printer's ink, as they are solely expressions of opinion based upon few or no observations and very faulty hypotheses and unfortunately have created many false impressions. Others of these have been of some value in citing observations with due attention being paid to conditions and in discussing all of the history of the installation in so far as it can be obtained. A very few have been of extreme value in that they have attempted to make deductions from the observations of concretes very closely examined, not only as regards conditions but also as to history, and have supplemented their deductive studies with more or less inductive research. But even the best of these can be considered only as very preliminary studies and largely as suggestive of lines which should be followed in future work.

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[1] In this connection "alkali" is generally erroneously used. The "white alkali" of arid sections is usually either an impure sodium sulfate or an impure mixture of sodium and magnesium sulfates and is but infrequently alkaline. The "black alkali," which may be white when dry, is an impure sodium carbonate; it is, however, noted in but relatively limited areas.

Again we feel that the major part of such studies must be confined to the cement. If we have a matrix unaffected or but very slightly affected by these solutions, then we must of necessity assume that we would have reached the ideal condition or at least have it in sight. For we now know enough about the quality and the grading of aggregates needed for such work to be able to make the proper choice in this respect without further study. But we have such a meager amount of information in regard to the resisting qualities of cement to such salt action that this field of study may be considered a virgin one. However, we must start not with the cement as it reaches the market, but go to the plant and still further to the quarry. We must know its entire history just as much as we must know the entire history of the concrete—assuming as is generally but erroneously done that the history of concrete starts with the delivery of the cement and aggregates to the job. We must know not only that it is a certain brand made at a certain plant but we must also know from actual observation how and from what it was made and this detailed knowledge must be obtained by some keen observer more skilled in making such observations than in making cement. We must know not only the amounts of the elements present in the clinker but also how they are combined and the amounts of these combinations. In addition we must know the character of the development of these combinations, whether they have developed only incipient crystallization or whether they are distinctly and coarsely crystallin; whether they have fused into a hard, almost glassy clinker or into a porous coke-like one. We must know something "real" about the fineness of the cement made from the clinker, not solely the 200-mesh fineness, but the actual "grading" of the cement particles and an idea of the angularity or roundness of the cement grain. Then we must have tests of the cement which will give us values which are indicative of the different behaviors which cement reveals in service. However, when we have all these data we are only in the position to make concrete and feel assured that we have on file some observations which have in other cases been overlooked but which are essential. In the meantime investigations must be under way which will have for their purpose the determination of the course and the products of the reaction which takes place between the constituents of cement and water, how these are affected by the various solutions with which cement may come in contact and how the reaction may be affected by various proportions of the constituents or by the different physical characteristics under which they may exist.

There is no question but that a deductive study of concrete forces us eventually to conclude that we are grossly ignorant of cement; what it is as produced on a plant scale; what variations in ultimate quality we must expect in any plant's production from day to day; what is the nature of the reaction with water and how this reaction will deport itself in a variety of atmospheres and solutions. Some intensive work must be done that will solve these questions and enable us to produce the exact kind of cement we want at any time after having found out the kind of cement we must have for the case in question. There is no gainsaying the fact that cement as now produced, and known as imperfectly as it is, is giving us in by far the great majority of cases most adequate service. But while innumerable successes will cause no annoyances or concern, one failure may offset all the successes. And acknowledging that we are using cement as intelligently as we now know how, are we getting out of it all or even a majority of its potential qualities? The cement producers are assured we are not, the cement user feels that he is doing the best that general job conditions will permit, but what an assurance and relief it would be both to know exactly which is right and to what degree.

Our title also calls for a discussion of investigations of plant methods in addition to a discussion of the properties of cement as we now have it made according to the usual plant routine. This phase of needed researches could be developed to far greater length than the limits of this paper permit, and pending the conclusion of such investigations as would show conclusively that different cements are needed for different uses, and would furthermore indicate what the characteristics of the different types should be, it were well to go into this discussion but briefly.

Those but casually acquainted with the industry are surprised that a product made to meet our present standards can be produced under such a set of widely varying plant procedures and equipment as now used. Though the product may be "standardized" it is quite evident that its methods of production are not. While it is generally believed that this wide latitude in methods does produce the same character of product, we are not convinced that such is the case, especially in view of the incompleteness of the methods used in examining the marketed product. The majority of plants have carried on certain studies as to the effect on the finished product of one or more changes in plant procedure, but these have been isolated and not well correlated and the finished product has been examined only according to our very imperfect methods. There is needed a broad, well-developed, correlated investigation, conducted in a number of plants, of the question of the quality of cement as affected by the different types of equipment now generally used. (This could be readily made of much interest to the manufacturer by obtaining data as to the efficiency of these methods.) Indeed at the present time the only incentive for such a study is the matter of needed economies in production, and the limited studies already made have shown the economic value of certain changes in methods—as the waste heat boiler. Furthermore, in this case it seems reasonable to believe that the required care demanded of the kiln operator, in order to insure a uniform delivery of heat to the boiler, has produced a change in the cement, more marked than is generally realized. Studies of the efficiency of the grinding apparatus, not only as to the relative operating costs of the several types of machines available, but extended, as they should be, to the efficiency of the resulting cements, would be of extreme value to both consumer and producer. How interesting would be an investigation on the properties of the resulting cements in which the sole variable in the methods of production would be the use of wet or dry raw mixes! Fuel economy demands a thorough study of all available types of kilns and furnaces and of necessity such a study must include the question of the quality of the cement produced. May not such studies result in our specifying what methods of production are desired in addition to the physical properties, just as we now specify steel of certain physical properties made by open-hearth or bessemer process?

This paper has been given a title that is a slight alteration of the general topic of this colloquium. Permit us in conclusion to suggest to this group gathered here for a deductive study of concrete, another slight variation of a well-known slogan as a thought to have before it for the next few years—"Concrete for Study". We must confess that we hesitated for some time before choosing "cement" or "concrete" for the first word of our slogan, but the word used was adopted largely because we did not wish to deviate too much from the original. However, if we will remember that nature has produced, of the essentials entering into concrete giving adequate service, the aggregate, the water and especially the conditions under which it is used, while man has produced the cement alone, we will acknowledge that man must fit his product to nature's, for he can change the aggregate but little, the water still less and the conditions practically not at all. Hence, though the vital study of concrete must be a study of cement, we suggest "Concrete for Study".

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INFLUENCE OF AGGREGATES ON THE DURABILITY OF CONCRETE

BY DUFF A. ABRAMS[1]

Concrete consists of an intimate mixture of cement, aggregate and water, which is allowed to harden under proper conditions. It is the only important structural material which is manufactured on the work. As in any manufacturing process, it is necessary to inquire carefully into the effects of variations in the properties of the constituent materials and also into the influence of the proportions in which they are used. Aggregate consists of natural sand, gravel, crushed rock, slag or other inert materials. In usual concrete mixtures, the aggregate comprises about 75 to 85 per cent of the bulk of the materials used.

Recent investigations and observations have shown that the quality of concrete as measured by its strength and resistance to wear is dependent to a large extent on the relative quantities of cement and water used in the mix, and to a lesser degree on the quality of the aggregate, so long as it is properly graded, clean and structurally sound. This discussion deals only with the quality of the aggregates and not with the principles governing proportioning and quantities used in concrete.

The aggregate should be graded from the finest particles up to as large a size as the nature of the work permits. It should be clean, that is, free from injurious amounts of loam, clay, organic impurities, alkalies, etc. The aggregate should be structurally sound in order that it may resist the forces to which it is exposed due to load or to weathering action.

The American Society for Testing Materials has standard or tentative standard methods for many of the more important tests for concrete aggregates, such as: grading (sieve analysis), abrasion, organic impurities, silt, specific gravity, unit weight, and sampling. Strength tests on mortar or concrete at 7 and 28 days are frequently used as a criterion of the quality of the aggregates.

The above-mentioned tests do not serve in all cases to differentiate between materials which are suitable and those which are unsuitable for concrete; for example, the behavior of an aggregate upon exposure to weather would not be determined by any of the tests commonly employed. Some forms of rocks such as shales, feldspathic and micaceous sands are readily broken down upon exposure to weather, and hence should be avoided as concrete aggregates. Special tests have been suggested for revealing the presence of soft particles, such as freezing and thawing, test for quantity of shale in sands and gravels, tests for mica, impact tests for gravel, modified abrasion tests, etc.

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F. C. Lang, of the University of Minnesota, has suggested the use of a solution of zinc chloride as a test method for determining the quantity of shale in sands and gravel. Investigations are now under way in an effort to determine the effect of mica in sands. Messrs. Pearson and Laughlin in a paper before the American Concrete Institute, January, 1923, described an instance of disintegration of concrete building units due to the presence of altered lime-soda feldspar in the aggregates. The impact test for coarse aggregates, proposed by F. H. Jackson,[1] will serve to detect the presence of soft particles in gravel.

Recent investigations have shown that the strength and resistance to wear of concrete is dependent only to a slight degree upon the weight, hardness or toughness of the material of which the coarse aggregate is composed, so long as the compressive strength of the material is not lower than that of the concretes. The high strength secured from light-weight aggregates consisting of burnt shale, has shown the fallacy of the older views that the strength of the concrete is dependent upon the strength of the aggregate. More thorough investigations have shown that the concrete made from limestones, gravels, granites, traps, sandstones and blast-furnace slag as coarse aggregates, give essentially the same strength, so long as the grading is similar and the quantity of water absorbed by the aggregate is taken into account.

Experience has shown that rocks which give comparatively high loss in the abrasion tests are entirely satisfactory for the wearing surface of concrete roads. The softer limestones and in some cases sandstones have given excellent results. At certain points along the Gulf Coast, sea shells have been used as aggregates in concrete roads. The reason for steady lowering in the requirements of aggregates for concrete roads, which has been noticeable during the past few years, is partly due to the change in the nature of the traffic on our highways. Rubber-tired vehicles produce little or no surface wear on a properly constructed concrete road.

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[1] F. H. Jackson, "An Impact Test for Gravel," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 362 (1922).

Tests of concrete exposed to fire, and observations on the condition of buildings which have been damaged by fire, have demonstrated that the fire resistance of concrete is dependent to a large degree on the mineralogical composition of the aggregate. Siliceous aggregates, particularly those composed largely of quartz, give poor results; limestones, trap rocks, and blast-furnace slag are more suitable for concrete likely to be exposed to fire or to extremely high temperatures.

Little definite information is available on the relative merits of different types of aggregates on the resistance of concrete to alkali or sea water; such tests as have been made indicate that the quality of the aggregates exerts comparatively little influence. Field and laboratory tests now under way are expected to throw further light on the behavior of aggregates of different types on the resistance of concrete exposed to highly concentrated sulfate solutions. A great many studies of concrete aggregates are being carried out by the various governmental bureaus, state highway laboratories and other research organizations.

In conclusion, it may be stated that the strength and durability of concrete depend to a large degree upon the *quality of the concrete*, and only to a minor degree upon the characteristics of the aggregates used. In other words, if proper methods of proportioning, mixing, placing and curing are employed, concrete of acceptable quality can be produced from aggregates which are generally considered to be inferior; on the other hand, poor concrete may result from the use of constituent materials of the highest quality, if proper methods are neglected. Care to avoid an excess of mixing water and proper provisions for curing are the most important factors in producing durable concrete.

THE INFLUENCE OF METHODS OF PROPORTIONING AND MIXING

By R. B. Young[1]

In the past few years a great deal of study has been given to the proportioning and mixing of concrete. We have had proposed a number of methods for determining proportions, some of which have been tried out in practice with good results. Attention has also been paid to the more accurate measurement of concrete materials on the job and methods and apparatus for this purpose have been developed and successfully used. There still remains a great deal to be investigated and some of these problems are intimately related to the quality and economy of concrete structures.

There is the question of the limitations of the new methods of determining proportions. Under certain conditions, concrete mixtures can be designed by these methods to give the strengths sought. Under other conditions equally likely to be met with on the job, the methods will not give the results required. It is important that the engineer attempting to proportion a concrete scientifically shall know when the method he employs can be used with safety, when the method is not applicable, or when its use requires special precautions. Lack of this knowledge may cause failure to get the expected results with consequent detriment to the structure or needless waste of materials. Much of the data required for this investigation already exist and only require to be assembled and analyzed in order to obtain this information.

More exact methods for measuring the raw materials of concrete on the job can be studied to advantage. Notable progress along this line has recently been made, but the importance of the problem both from the standpoint of quality and permanence and from that of economy makes it worthy of a great deal of further study. It is not generally appreciated that under average conditions, our present methods of measuring concrete materials, particularly in the case of fine aggregate, result in lack of uniformity in the concrete mixtures and that their inherent errors tend to produce undersanded and permeable concrete more susceptible to the action of destructive agencies such as frost, alkali or sea water. Present methods are also responsible for a great many cases where contractors have overrun their estimates for cement. A 10 to 15-per-cent increase in cement requirements brought about by improper measurement of fine aggregate is not unknown and this is not the only loss occurring in such a case, as the output of the construction plant is reduced.

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Nor do we know all there is to be known about the laws governing mixing. The relationship between the time during which a concrete is mixed and its strength at a given period is fairly well established, but little is known as to the relative effectiveness of different mechanical mixers. We now consider all mixers, both big and little, as on a par. Our tests to date have largely been carried out in the laboratory on small mixers and then on only one type. What we need are large-scale experiments on mixers of different makes and different capacities.

What do we know about the influence of plant layout and construction on proportioning and mixing concrete? For example, the arrangement of bin gates in stationary plants influences the absolute volume of aggregate in a measured volume, and this in turn influences the cement content and water requirements and thus the quality of the concrete. Again, with the proper consistency there seems to be a remixing action during the transfer of concrete through chutes. How much do we know about this? Are our methods of charging a mixer altogether satisfactory? They are, probably, when we consider only our present equipment; but equipment could be modified if experiments demonstrated that other methods gave better concrete or were more economical in materials and power.

This is not an attempt to outline every phase of proportioning and mixing which could be studied with advantage, but is only to suggest a few which might help us to produce cheaper and more lasting concrete. We hope in the discussion which is to follow that many more of these problems will be brought up.

BEHAVIOR OF MASS CONCRETE UNDER SERVICE CONDITIONS

By Arthur P. Davis[1]

The writer has had opportunity to examine many large masses of concrete such as dams and bridge abutments, either in a supervisory or consulting capacity, or as an observer during and after construction. In the preparation of this material valuable assistance has been rendered the author by Mr. C. A. Bissell.

Alkaline Action

Percolation of alkaline water into or through thin sections of concrete produces disintegration that has not been noticed in large masses. Disintegration due to freezing or to the action of combustion gases or acids in liquid form is superficial in character and not characteristic of large masses of concrete.

The largest masses of concrete in which failure has been observed were built upon shales containing a large percentage of alkali from which ground water was absorbed by capillary attraction into the concrete. On the edges of the concrete the water would evaporate, leaving the contained salts under the surface, where its crystallization gradually produced superficial disintegration and as the disintegrated material was sloughed off or washed away new surfaces were exposed and the disintegration became progressive, leading finally to failure of the base of the column or buttress.

The best remedy discovered for these conditions was increased density, which will resist the absorption of alkaline waters.

Temperature Changes in Mass Concrete

In building any large structure of mass concrete, recognition must be given to the effect of temperature changes on the volume of the structure. In the construction of concrete dams, it is usual to provide frequent contraction joints to localize these effects. Such joints, however, if open, break the dam into more or less isolated segments, effectively preventing the flat-arch or short-beam effect which might be realized if the whole dam could be so built that it would at all times be under compression from end to end. Such beam effect, if achieved, would constitute an additional factor of safety against the failure of the dam through the weakening of one section.

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[1] Director, U. S. Reclamation Service, Washington, D. C.

In order to secure this compression, it has been the practice of some engineers to specify that certain designated sections of the dam shall be poured only in the winter months, bringing the adjacent sections up in advance in the hope that thus the entire mass will be thrown into compression with any rise of temperature.

Another method of securing an equivalent result is, where the topography is favorable, to construct the dam on a curved plan, so that the pressure of the retained water will tend to wedge the segments together. These are to be carefully distinguished from those sharply curved, thin arch dams in narrow canyons which depend upon arch action for their stability; the dams here spoken of secure their stability through their mass, the somewhat flat curve adding additional security at small additional expenditure.

In order to add to our knowledge of what actual temperatures exist in the body of a concrete dam while it is under construction and afterwards, a study was made by Reclamation Service engineers during the construction of the Arrowrock dam near Boise, Idaho. This is a gravity concrete dam, maximum height 349 ft., volume 585,000 cu. yd. Ten electrical resistance thermometers were embedded in the concrete of the dam at suitable points during construction and records kept of the range of temperature indicated by each.

From these studies[1] the following conclusions were drawn:

1. Large bodies of concrete deposited rapidly during a summer season develop a temperature of 90 to 95° F. within a period of about 30 days, and maintain nearly that temperature for several months.

2. In the case of concrete one foot from an exposed face there is a *daily* variation in temperature of about 2° when the daily variation in the temperature of the air is about 50° F.

3. In the case of concrete 2 ft. from an exposed face there is a daily variation of less than 1° when the daily variation in the temperature of the air is about 50° F.

4. In the case of concrete 3.5 ft. from an exposed face, no daily variation in temperature is apparent when the daily variation in the temperature of the air is about 50° F.

5. The *seasonal* variation in the temperature of concrete 3.5 ft. from an exposed face is about 32° when the seasonal variation in the *mean daily* temperature of the air is about 72° F.

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[1] Charles H. Paul and A. B. Mayhew, "Temperature Changes in Mass Concrete," *Transactions*, Am. Soc. Civ. Engrs., Vol. LXXIX, p. 1225.

6. The seasonal variation in the temperature of concrete 10 ft. from an exposed face is about 12° when the seasonal variations of the mean daily temperature of the air is about 72° F.

7. The seasonal variation in the temperature of concrete 20 ft. from an exposed face is very little, and after the effect of "setting heat" has once been overcome, the change in temperature of concrete at that distance from an exposed face is so slight as to be negligible, under ordinary conditions.

8 The effect of the "setting heat" in concrete 20 ft. or more from an exposed face is felt for several years after the concrete is placed. Concrete near the center of mass of a large dam probably retains some of this "setting heat" for five years or more after placing.

9. Contraction joints are desirable in all large concrete structures, whether or not they are to be exposed to wide variations of outside temperature, except possibly when all construction may be carried on during cold weather, and when the variation of outside temperatures, after completion, will be slight.

10. Changes in volume due to setting, hardening, and seasoning of concrete are more important, ordinarily—excepting very close to exposed faces—than changes in volume due to the influence of daily or seasonal variations in outside temperatures.

DISTORTION OF MASS CONCRETE UNDER PRESSURE

The service outlets of the Elephant Butte dam on the Rio Grande consist of two pairs of 60-in. balanced valves embedded in the concrete of the dam. The lower pair is at elevation 4234, the upper pair at elevation 4290, the top of the dam being 4414 ft. above sea level. The design of valve used consists of a conical-tipped cylindrical plug 6 ft. 2 in. in diameter at the base, moving in guides supported in a cast-iron bowl about 12 ft. in diameter and 5 ft. deep. In installing these valves the bowl-shaped castings were set to line and grade within the body of the dam and surrounded with concrete. When these castings were firmly embedded in the concrete the section of the dam in which they are located was carried on up, in successive lifts, never more than 2 ft. of concrete being poured at one time. After six months of operation of these valves, the plugs refused to work, it constantly becoming more difficult to open them. The service gates were closed and overhauling of the valves started. In the first valve opened, the plug was filed and replaced without measuring the distortion; in the other three, calipered measurements were made showing that the cylinders had been compressed in the vertical diameter and elongated in the horizontal.

Since the valves worked perfectly when first installed, the distortion must have been gradual, and is probably explained by assuming that the head of concrete over the casting continued to compress for a long time after final set would have been supposed to have taken place.

Since the plugs do not revolve, the portion of the bull ring showing binding was hand-dressed with files until a good fit was secured. The maximum distortion was noted in the lower west valve, 0.027 in. in a diameter of 6 ft. 2 in. (The lower east one was not measured.) The upper valves located 56 ft. higher with a consequent lower head of concrete, showed a maximum distortion of 0.015 in.

WEAR OF CONCRETE UNDER HIGH VELOCITIES

Frequent inquiries are made regarding safe velocities of water upon concrete and the following notes, some of which were published some years ago in *Engineering News*,[1] may be of interest in this connection.

The dam on Tuolumne River, near LaGrange, Calif., is an ogee overflow type of dam, giving a fall to the water of approximately 100 ft. The basin above it, being a narrow cañon, was filled with sand during the first few years, so that the sands of the river are carried over its top by the water thus discharged and the dam has withstood an overflow on various occasions from 15 to 16 ft. in depth over the crest.

This water, when it reaches the toe of the dam, probably has a velocity of more than 70 ft. per sec. The flood of Tuolumne River flows over this dam at varying depths for six or eight months every year.

The dam is built of rubble masonry, the rock used being of a very tough greenstone found at the site, which also comprises the bed of the river in the foundation and below the dam. The curved surface which the water follows on its course over the dam is built of concrete, consisting of a mixture of cement and sand with broken rock of the same tough greenstone. This concrete shows a slight wear at most points, the wear being greatest upon the sand and cement, leaving the pieces of greenstone projecting slightly from the surface, and they are highly polished.

At various points it is still possible to see the marks of the forms against which the dam was originally built, but over most of the surface some wear is noticeable, possibly reaching as much as ½ in.

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[1] *Engineering News*, January 4, 1912, Vol. 67, p. 20.

The wear is, of course, due to large quantities of sand which flow over the dam, and is probably greatest at low water, when the velocities are not as high as they are in the greatest flows. It is fair to say that absolutely no actual damage has been done on this dam under the high velocity to which it is subjected, and that even the slight wear shown would probably be absent were it not for the sand which the water carries.

The tunnel constructed through the south abutment of the Roosevelt Dam, designed for the diversion of water during the construction of the foundation, was built in hard sandstone occurring in strata inclined about 30 deg. to the horizontal. The rock, while hard, was rather seamy, and it was intended before putting it to regular use to line it with steel plates; however, before this was done the contractor desired to use it for diverting the river while constructing the foundation, and it was put to such use without lining.

By reason of a flood which overtopped the unfinished dam, the water was for some time discharged through this tunnel, under a head of about 60 ft., which did considerable damage to the natural rock in place in the perimeter of the tunnel. Though this is not an example of action on concrete, it is a very vivid illustration of what a turbulent broken-up stream of water under this high velocity will do. It was necessary to close the gates and make careful repairs to the tunnel before subjecting it to further use.

During the year 1899, the North Platte River experienced the greatest flood in its history and the Pathfinder Reservoir was nearly filled in the course of the summer, although a tunnel 10 by 13 ft. in section was left open, as was also a culvert 6 ft. in diameter at the upper end and 4 ft. at the lower through the Pathfinder dam. There were also two 36-in. pipes, discharging under full head at the same time. Notwithstanding all these openings, the water rose to a height of 181 ft. above the river bed, and water was discharging under a head of more than 150 ft. through all the openings mentioned from May to October, nearly six months. The Pathfinder tunnel was built in a good quality of granite and was lined with ½-in. steel plates, backed with concrete with all bolts countersunk and extra precautions taken to prevent the water getting a hold upon the lining.

The experience of those six months gives the best and most conclusive illustration known to the writer of the action of water under very high velocity. It is, of course, impossible to say just what was the velocity in any one of these conduits, but a close average was obtained by comparing the total cross-section of the conduits with the discharge of the river below the dam, and through most of the six months mentioned the average velocity of the water in these various conduits was in the neighborhood of 90 ft. per sec. The culvert through the dam, which was 4 by 6 ft. at its upper end, gradually tapering to 4 by 4 ft. at the lower end, was built of concrete, and after the flow of water through it for six months at velocities from 75 to 90 ft. per sec., it was absolutely uninjured, the marks of the forms still plainly showing upon the concrete. This showed clearly that clean water gliding over concrete without impact has little erosive effect regardless of velocity.

On the other hand, the stream flowing through the tunnel, which was broken into four parts by the gates and entered a conduit of nearly double the cross-section of the gate openings where the stream was turbulent and impact was permitted, was very destructive. Nearly all of the steel lining was torn out of the tunnel, all of the concrete thus exposed was carried away and considerable erosive effect upon the granite was noted. One block of granite containing about 4 cu. yd. was turned from the side of the tunnel into the tunnel like a door, and various other evidences of the enormous force of the water were noticed.

At Arrowrock dam the water is discharged through 17 regulating conduits 52 in. in diameter, passing horizontally directly through the body of the dam with valves on the upstream face of the dam. The conduits are arranged in two tiers, 197 and 110 ft., respectively, below the top of the parapet wall. Each of the lower set is 129 ft. long and each of the upper set 70 ft. These conduits are lined at the reservoir end for 28 ft. with semi-steel pipe $1\frac{1}{2}$-in. thick; the unlined portions are surrounded by a special mortar mixed in proportion 1 part portland cement to 1.85 parts sand. Special care was taken to secure a dense smooth interior surface for the conduit, four successive washes of portland cement being applied and after drying scraped off, followed by two coats each of water-gas tar and coal tar.

During the spring of 1916 the upper ten regulating outlets were used to pass flood waters. Over a considerable length of time all ten were running wide open under heads as great as 103 ft. Under this condition the vibration was very slight, and knocks or jars were barely perceptible. Each outlet was discharging approximately 1000 sec-ft., the water falling 145 ft. to the river. The jets present a very beautiful and spectacular sight. They are green and perfectly clear when the exit from the dam is made, but become opalescent, pearly, and pure white as they gradually ravel off in their descent. The bulk of the jet remains fairly solid and strikes the pool with a roar, sending the spray over the top of the dam, 250 ft. above, when the wind is upstream. As the jets descend they converge into a solid sheet of water at the pool, due to the outlets being built on radial lines. Under a 28-ft. head, the discharge from the upper outlets clears the toe of the dam, and under a head of 100 ft. falls into the pool about 100 ft. below the dam. In 1915, after a part of the upper outlets had discharged under heads up to 78.3 ft., soundings were taken in the pool, and it was found to be 23 ft. deep.

An inspection of the lower three positive-control outlets on May 6, 1916, after they had discharged intermittently under usual working conditions for eight months, one of them working continuously over six months under heads up to 118 ft., with a corresponding velocity of about 70 ft. per sec., showed them to be in almost perfect condition. During the previous season these same outlets were operated under heads up to 106 ft. Altogether one outlet (No. 6) worked for 8½ months before the inspection was made. The valve was tight. The tar coating on the semi-steel lining was in perfect condition; that on the concrete was about half gone, the heaviest wear being at the lower quarter points. Where the tar was gone the cement was in perfect condition. The whole surface was slick and to the touch was slimy, indicating that there is very little or no silt carried in the water and that the coefficient of friction must be very small. The patches in places were exposed. In no case was there any erosion at the edges of the patch, nor was any patch dug out. At one place at the quarter point there was a small hole 6 in. long, 1½ in. wide, and ½ in. deep eroded parallel to the axis of the outlet, probably along a horizontal joint between the layers of mortar.

Inspection at the close of the irrigation season of 1916 disclosed no wear in the plain-control outlets, except slight loss of paint and a few flakes of cement wash. Positive-control outlet No. 6 showed a honey-combed area in the concrete conduit about ½ in. deep and 3 ft. wide by 3 ft. 5 in. long on the north side of the outlet, 1 ft. from the bottom and 4 ft. from the end of the lining. Positive-control outlet No. 17 showed a hole in the concrete about 10 in. in diameter and about 1½ in. maximum depth on the north side of the conduit.

Annual inspections of the outlets have been made since the valves were first installed, and the condition of the valves and conduits in the fall of 1920 was reported to be practically as good as when first installed. In the fall of 1921, however, the throat liners and the concrete conduits below the semi-steel linings were found to be considerably worn. No. 16 positive-control valve in the upper tier was in the worst condition. The stationary bronze seat of the valve was slightly pitted, and the throat liner was pitted about ⅛ in. deep.

There was slight pitting of the semi-steel lining at one point on the bottom, but the paint is still on this metal over almost the entire area. The concrete at the bottom of the conduit below the metal lining eroded in waves about 1 to 5 in. deep, 1 ft. wide, and for a distance of 12 ft. along the pipe. On the right-hand side of the pipe, facing downstream, there were two pits about 2½ in. deep, 1 ft. wide, and 3 ft. long. On the left-hand side there were pits 3 in. deep and 1 ft. wide, extending 6 ft. along the pipe. The tar paint was about half gone from the rest of the outlet.

TABLE I.—VELOCITIES IN OUTLET CONDUITS IN DAMS

Reservoir	Outlet	Number and Type of Conduits	Purpose of Discharge	Normal Designed Capacity		Estimated Maximum Velocity, ft. per sec.
				Discharge, second-feet	Velocity, ft. per sec.	
Roosevelt	South tunnel	Two 48-in. steel pipes	Irrigation	325 each	26	35
"	North tunnel, conduit below 58-in. balanced valves	Three 60-in. cast-iron pipes.	Irrigation	900 each	32	70
"	North tunnel, conduit above 54-in. needle valves	Three 54-in. steel pipes	Irrigation	800 each	50	60
Shoshone	Lower outlet tunnel through gate passages	Three rectangular cast-iron lined	Irrigation	1,000 each	42	45
"	Power	One $8\frac{1}{2}$-ft. horseshoe lined tunnel	Power	500	8	10
"	By-pass, above 36-in. needle valve	One 42-in. steel pipe	Irrigation	400	42	50
Pathfinder	North tunnel, above 58-in. needle valves	Two 6-ft. diameter concrete conduits	Irrigation	1,000 each	35	45
"	South tunnel, below 58-in. balanced valves	Six 60-in. cast-iron pipes	Irrigation	1,050 each	53	70
"	South tunnel, below 58-in. balanced valves	Six 52-in. steel pipes	Irrigation	1,000 each	63	70
Belle Fourche	North conduit	One concrete	Irrigation	650	14	35
Strawberry	Outlet tunnel	One 98-in. diameter concrete	Irrigation	636	12	25
Arrowrock	Balanced-valve outlets	Seventeen 52-in. diameter concrete	Irrigation	1,000 each	68	70
"	Sluice gates	Five 60-in. cast-iron and concrete	Sluicing	....	..	70
Elephant Butte	Service outlets	Four 60-in. cast-iron and concrete	Irrigation	1,200 each	61	70
"	Power outlets	Six 60-in. cast-iron pipes	Power	250 each	9	11
Lahontan	Cylinder gates	Two 9-ft. horseshoe concrete	Irrigation	1,400 each	21	30
Minatare	Outlet tunnel	Two 48-in. steel pipes	Irrigation	150 each	12	20
Jackson Lake	....	Twenty concrete	Irrigation	....	44	45
Sherburne Lake	Outlet tunnel	Two concrete	Irrigation	500 each	12	20
McDonald Lake	Outlet tunnel	One 6-ft. diameter concrete	Irrigation	300	21	25

No. 18 plain-control valve in the upper tier was in the worst condition of any plain-control outlet. The stationary bronze seat was pitted at top and bottom about $\frac{1}{16}$ in. deep. The throat piece was pitted at the bottom about 1 in. deep and over an area of about 18 in. in diameter. Almost the entire top half of the liner and the conduit for a few in. below the liner was pitted to a maximum depth of about 1½ in. The semi-steel linings were not damaged and the concrete was intact. The paint was about half gone from the concrete, but that on the metal linings was nearly intact. The damaged concrete was patched and the cavities in the throat liners were filled with Smooth-on.

These observations are very convincing upon two points:

1. That where clear water can be made to glide over concrete without disturbing its velocity or abruptly changing its direction, there is no practical limit to the velocities that can be permitted without harm.

2. That concrete which is subjected to the impact of water under high velocity is rapidly eroded and that under such conditions the velocities must be very carefully limited.

Water carrying sand or grit will gradually erode good concrete if the velocity is high.

The accompanying Table I shows the velocities reached in outlet conduits of the Reclamation Service dams.

BEHAVIOR OF CONCRETE EXPOSED TO ATMOSPHERIC CONDITIONS

By P. J. Freeman[1]

A careful study of many concrete structures which have deteriorated under atmospheric conditions has led the writer to believe that most of the trouble could have been avoided if the engineers and contractors had drawn their specifications and handled the construction so that the concrete produced had the qualities necessary to meet the demands of that particular service. Instead of doing this, the required strength was used as the principal guide and little or no thought was given to other requirements, particularly impermeability, but it was assumed that any concrete having sufficient strength for the purpose would also be durable, regardless of the conditions to which the concrete was exposed.

Numerous examples could be cited, but one or two will serve to illustrate the point which the writer wishes to make.

A concrete dry-dock on one of our northern lakes was constructed at a cost of about a million dollars, using a mixture of one part of portland cement, three parts of sand, and five parts of limestone. At the end of five years the inside or exposed surface began to disintegrate so badly that at the end of about six years at least thirty per cent of the entire inside surface had crumbled and spalled away to depths varying from one to six or eight inches. Horizontal cores were drilled from the walls, which were thirteen feet thick. An examination of these cores showed that, extending in for several inches from the exposed surface, the concrete was mostly sand and cement. In placing the concrete, care had not been exercised to make an impermeable surface, although the compressive strength of the concrete was entirely adequate for the load requirements. The alternate filling and emptying of the dock caused water to percolate into the porous or permeable concrete and the action of the weather, particularly freezing and thawing, produced the disintegration which continued to eat away the walls until extensive repairs were made. The outside of the dock had been back-filled with loose material, principally cinders, which permitted water to come in contact with the wall within a few feet of the top. Excavations to remove this back-fill were made and in no instance was any disintegration found, as the wall had been protected from freezing by the fill.

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[1] Engineer of Tests, Pittsburgh Testing Laboratory.

A similar example shows quite clearly the results of neglecting to consider impermeability as an important factor in the design of a large hydro-electric plant. The project was started about 1912 and after building part of the dam the project was abandoned until recently. The mixtures used were 1 : 2 : 4 and 1 : 3 : 6 of portland cement, river sand, and river gravel. The 1 : 2 : 4 mix does not show signs of serious disintegration, but the concrete of the leaner mixture is in very bad condition. A large number of cores were drilled from the concrete at various locations, and the results of crushing tests showed an average strength of 2166 lb. per sq. in. for the 1: 3: 6 concrete and 3400 lb. per sq. in. for the 1: 2: 4 concrete. The investigation has not been completed, but it seems to be another case of not having the concrete made to suit the particular class of service but designed only to furnish the required strength on the assumption that such concrete would be durable.

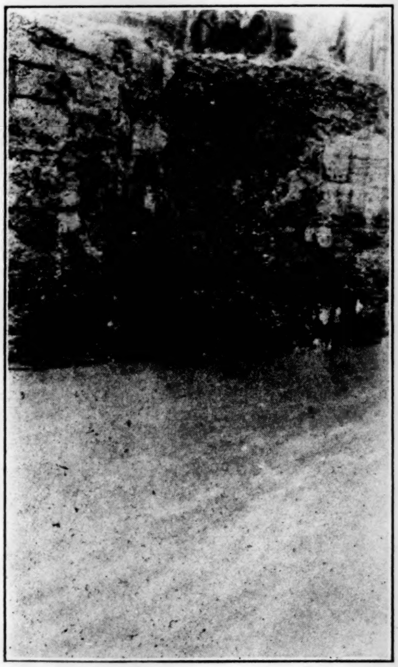

FIG. 1.—Common Type of Disintegration Close to Construction Joint

FIG. 2.—Close-up View of Part of Fig. 1

Figs. 1 and 2 show a common type of disintegration which occurs close to construction joints. Fig. 2 is a close-up view of part of

Fig. 1. Fig. 3 is another illustration of this type of disintegration. Fig. 4 shows the peculiar form of disintegration which occurs near the edges of exposed surfaces.

It is not necessary to discuss the effects of placing concrete in cold weather, except to observe that many jobs are spoiled because those in charge neglect to consider that concrete must be warm to set and that they have not satisfied this requirement by merely keeping the materials from freezing while being mixed and placed and for a short time afterward. A $300,000 reinforced-concrete building was built last winter under these conditions and when the first warm day came the concrete began to go to pieces, and it is doubtful if satisfactory repairs can be made to save the structure.

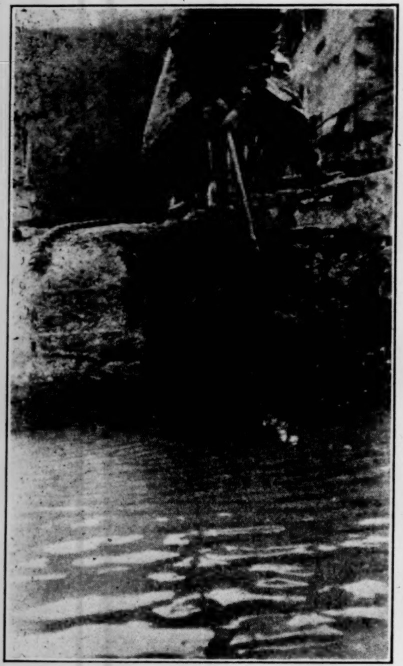

FIG. 3.—Common Type of Disintegration Close to Construction Joint

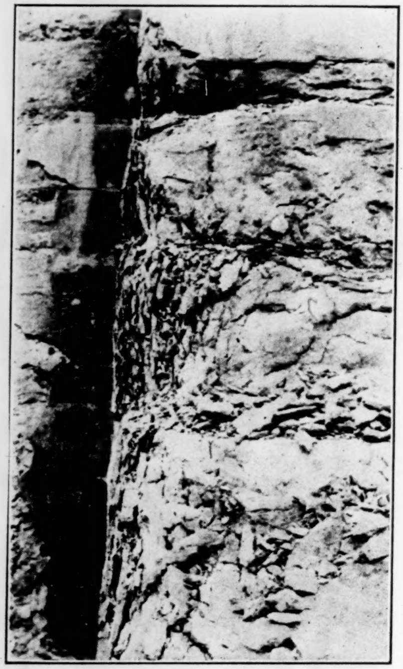

FIG. 4.—Disintegration Near Edge of Exposed Surface

An entirely different kind of trouble was experienced in connection with the base of a concrete highway. The mixture used was one part of portland cement, three parts of river sand, and six parts of river gravel, which were tested and passed by the State Highway Department. The concrete was placed during the summer months and allowed to harden before the asphalt top was applied. After about two years it was necessary to make some repairs and it was found that the concrete was not firm and hard but could be easily broken up, and that the gravel was covered with a white deposit of calcium sulfate. After exposure to the air the concrete had a tendency to become hard. Inasmuch as all of the materials had been found to comply with the best modern specifications and chemical analyses of the concrete showed that the mixture was 1 : 3 : 6 as specified, it was not possible to draw definite conclusions and a more thorough study should be made of similar conditions with particular reference to the specifications covering the richness of the mix.

In a very carefully prepared paper covering the results of an extended investigation of conditions in the Cascade Tunnel,[1] Mr. Charles A. Newhall mentions a somewhat similar condition which he calls "Sulfate Disintegration," which he states is due to fumes from locomotive fuels by which calcium sulfate is formed. There may be some possible relation found to the conditions encountered in the highway base mentioned above.

Although the writer had no personal connection with the investigation it seems worth while to include the summary of Mr. Newhall's investigations in this paper:

1. In the Cascade Tunnel two distinct types of disintegration were noted. In one type the concrete softened and became mushy without swelling or the formation of white mud. This disintegration is due to the actual solution and removal of the cement by water flowing over and through an originally porous concrete.

2. The second type, called the "Sulfate Disintegration" to distinguish it from the first, is prevalent in the Cascade Tunnel. It is always accompanied by swelling of the concrete and the formation of a white mud abnormally high in sulfur content.

3. This "Sulfate Disintegration" is the result of a reaction between the sulfurous fumes from the locomotive fuels and the cement of the concrete. In this reaction gypsum (hydrated calcium sulfate) is formed. Also the cement is replaced by or changed into a soft white substance, this change being accompanied by a volume increase of 716 per cent.

4. Moisture is essential to the reaction. It does not start in dry concrete. It stops when the concrete dries out.

5. This disintegration starts in porous spots in the concrete or in cracks and fissures that have formed as a result of stresses due to expansion and contraction, or possibly to earth movements.

6. The "Sulfate Disintegration" once started, progresses very slowly, requiring approximately ten years before it advances far enough to become apparent to the eye.

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[1] Charles A. Newhall, "The Disintegration of Concrete in Railway Tunnels," Pacific Northwest Society of Engineers, Seattle, Washington, October-November, 1916.

In every case of concrete disintegration mentioned, the cement was all tested and complied with the specifications of the American Society for Testing Materials. The German cement which was used for all of the work in the Cascade Tunnel was also tested in this country and found to comply with the same specifications.

Designers of concrete should realize that they must consider the atmospheric conditions to which the concrete will be subjected and eliminate from their design any features which would tend to cause or permit water to percolate into the concrete.

At the present time it is almost impossible to obtain the facts necessary to make a satisfactory investigation of any concrete structure that was built a few years ago. If concrete producers will make more careful records and tests and exchange ideas concerning their troubles, the writer feels that it will take only a few years to eliminate most of the unsatisfactory conditions which now exist.

BEHAVIOR OF CONCRETE IN ROADS

BY A. T. GOLDBECK[1]

It is difficult to conceive of any type of concrete structure which is subjected to a more complex set of influences than is a concrete road surfacing. Investigations of the past few years are just beginning to show us something of the stresses to which concrete roads are subjected by the many kinds of forces acting upon them.

Let us consider the concrete road beginning from the day the concrete is laid, and let us look into the many factors which are at work to influence the condition of the surfacing throughout its period of life. The concrete is deposited on the subgrade and slowly hardens. At the very beginning agencies might and in some cases do start to attack the integrity of the hardening mass for it is at this stage that hair cracks develop.

The concrete is "cured" by the use of some means for keeping the surface wet, such as frequent sprinkling and with the use of a wet earth covering, by ponding, by straw covering, and in the last few years by a sprinkling of calcium chloride which acts as a hygroscopic material. After a short period of curing, the moisture is allowed to dry out from the concrete, and, as has been well established, shrinkage now takes place and the concrete slab by contraction slowly moves over the subgrade. The forces of friction thus developed between the subgrade and the slab produce tensile stress in the concrete, and when this stress exceeds the tensile strength, transverse cracks form at frequent intervals. Changes in temperature of the slab also expand and contract it and cause it to slide over the subgrade, again producing compressive or tensile stress. During the day, the top surface is heated by the sun to a higher temperature than the bottom surface and naturally the slab is warped downward at the edges and corners. During the night the reverse is true; the corners warp upward. In many cases they actually leave the subgrade. During periods of dry weather, the top surface becomes quite dry, while the bottom surface is in contact with the more or less moist subgrade. Under such conditions the concrete might be warped and bent upward at the sides and ends. In the meantime the varying moisture conditions in the subgrade are swelling and shrinking the soil in a most non-uniform manner and the road is offered non-uniform support.

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[1] Chief, Division of Tests, U. S. Bureau of Public Roads.

When cold weather comes, the ground freezes. Very naturally the entire road is raised due to the expanding action of the frost, and if it happens that snow is cleared from the center of the road and is heaped up along the sides, the insulating blanket of snow prevents as deep a penetration of frost at the sides as at the center. The center of the road is then heaved more than the sides which overhang their center support like two cantilever beams, and through over-stress thus created, the slab is cracked down the center. Frost action within the concrete itself also must be resisted, and this requires a dense, strong concrete of low absorption. A number of natural influences then have been at work even before traffic is turned onto the road.

The heavy fast-moving loads of the present day produce high stress in concrete slabs under certain conditions. This is particularly so with heavy fast-moving vehicles having well-worn solid rubber tires. Impact stresses are apt to be high and this is especially true in the vicinity of joints and on roughly finished spots in the road. Not only does this heavy impact produce high bending stress, but the local pressure on the immediate surface is considerable. There are concrete roads in which exceptionally poor coarse aggregates have been used largely as a matter of expediency, and these are now showing signs of distress from the action of traffic.

When we add to all of these influences the troubles which are apt to occur due to the use of unsuitable materials, too much water, or greatly inferior workmanship, coupled with a design ill-adapted to the conditions of subgrade and traffic, the wonder is that concrete roads are capable of giving the most excellent service which, in general, they are giving under our present-day rubber-tired traffic.

The concrete road, however, like all other types of roads, develops certain defects, some of which are serious and some of which are of little consequence in their effect on the service value of the road.

In collecting information on defects in concrete roads I have been fortunate in having recourse to data supplied by the District Engineers of the U. S. Bureau of Public Roads, having headquarters in various parts of the country. The Bureau representatives in the various states likewise have presented their ideas through the District Engineers, so that the principal defects now occurring in concrete roads should in this way have been pretty well observed and reported.

DEFECTIVE DESIGN

The concrete road has had its share of failures due to the use of a design which has been ill-fitted for the traffic and subgrade conditions. There have been reasons for poor design in the past, for many principles are only now beginning to be recognized and understood, and moreover, our oldest concrete roads have witnessed an increase in variety and intensity of traffic such as has never been recorded in a similar period in history. Again, the phenomena in connection with the movement and accumulation of moisture in the subgrade and the action of moisture on various kinds of subgrade materials have never been given thorough study.

Truck wheel loads have been increasing in weight with great rapidity; speeds have been ever increasing, and in the absence of sufficient legal restraint on such loads, very naturally cases have arisen where the high stresses produced by loads coupled with inadequate and non-uniform support offered by the subgrade have resulted in the structural failure of the surfacing. The means for overcoming defective soils, the knowledge necessary for adequate design, and legal restrictions necessary for the control of maximum wheel loads and speed are now almost at hand, and will serve to minimize structural failures from these causes in the future.

DEFECTS IN CONCRETE ROADS OTHER THAN THOSE DUE TO FAULTY DESIGN

There are other defects occurring in concrete roads, some of which are of no particular importance, and others of which that are quite serious. These defects as observed by the District Engineers of the U. S. Bureau of Public Roads, by their assistants and by the author are listed as follows:

1. Cracking:
    - (*a*) Transverse;
    - (*b*) Longitudinal;
    - (*c*) Diagonal cracking;
    - (*d*) Corner cracking.
2. Hair Checking of the Concrete Surface.
3. Surface Irregularities in the Pavement as Constructed:
    - (*a*) Due to lack of uniform consistency of the mix;
    - (*b*) High joints;
    - (*c*) Slumping of concrete on grades;
    - (*d*) Unevenness of side forms.
4. Settlement of the Pavement.
5. "Blow-ups" of the Pavement.
6. Slipping of Joints.
7. Spalling at Concealed Joints.

8. Surface Scaling:
    (*a*) Due to fine aggregate;
    (*b*) Too wet a mixture or excessive finishing;
    (*c*) Calcium chloride curing;
    (*d*) Freezing.
9. Surface Raveling.
10. Surface Wear.
11. Concrete Disintegration:
    (*a*) Due to alkali;
    (*b*) Defective materials.
12. Surface Pockets:
    (*a*) Due to dirty aggregate, balls of clay, coal, wood chips, soft material.

CRACKING OF CONCRETE PAVEMENTS

Perhaps the most common defect noticeable in the concrete pavement is that of cracking, the most frequent cracks being transverse cracks across the width of the pavement. Longitudinal cracks parallel with the edges of the pavement, diagonal cracks and corner cracking also occur.

(*a*) *Transverse Cracks.*—It seems inevitable that transverse cracks must form in concrete pavements, because concrete will not stretch to the amount made necessary by shrinkage produced through drying out of moisture and by decrease in temperature. This becomes evident when we realize that concrete shrinks approximately 0.0004 to 0.0005 in. per inch of length due to drying out of moisture and approximately 0.0005 in. per inch of length due to 100° F. fall in temperature, a possibility of 0.001 in. shrinkage per inch of length due to these combined causes. This is equivalent to 0.1 ft. per 100 ft. with a combination of these extreme conditions.

The probable deformation of road concrete when cracking takes place does not exceed 0.02 ft. per 100 ft. With a deformation of this amount, transverse cracks are bound to form in any considerable length of concrete road. It is a question, however, whether they should be considered as a very serious defect, as they may generally be maintained with comparatively little expense and in such a manner that no harm results from their presence. No doubt, transverse cracks likewise form through subgrade settlement and through the bending action of heavy loads which produce a too high fiber stress.

(*b*) *Longitudinal Cracking.*—Longitudinal cracks occur, in general, due to non-uniform support offered by the subgrade. In those parts of the country where frost penetrates to a considerable depth, longitudinal cracking is quite prevalent. This is due, perhaps, to the unequal depth of frost penetration at the sides of the road as compared with the center. It has been observed, especially where snow is piled at the sides of the road, the center being exposed, that frost will penetrate deeper at the center than at the sides, thereby producing greater vertical motion at the center through the expanding action of a deeper layer of ice. The concrete surface having insufficient resistance to bending then develops longitudinal cracking. The practice of allowing wood side forms to remain in place seems to have led to cracking. The forms are raised by frost, a water reservoir is thus formed under them, and this aids in saturating the subgrade.

Another condition which produces longitudinal cracking has been observed in accentuated form particularly in the adobe soils of California. These soils swell and shrink to a considerable degree when their moisture content changes. Very naturally the moisture changes are apt to be higher at the sides of the road than at the center. Cases are on record of the subgrade having shrunk $1\frac{1}{2}$ in. away from under the side of the concrete slab. Under such conditions, longitudinal cracks might form within several feet of the side of the road. The remedy for such conditions, in part at least, lies in a proper treatment of the subgrade. It is stated that subgrade preparation by plowing, harrowing, and re-rolling to a considerable depth, making for homogeneity and waterproofness, greatly inhibits cracking from subgrade causes. Another treatment under the same conditions involves the use of a layer of sand between the subgrade and the slab. Experiments in which admixtures such as portland cement, hydrated lime, sand and bituminous materials have been harrowed and plowed into the underlying subgrade to decrease its shrinkage factor, have shown promising results.

Longitudinal cracks of themselves are not particularly harmful. There is a tendency, however, to develop forked cracking which produces weak spots in the road surfaces, and for this reason the more recent pavement designs have called for a center longitudinal joint in the road so that the position of the longitudinal crack is predetermined and confined to a straight line. Certain types of longitudinal cracks seem to develop, beginning at the transverse expansion joint and extending perhaps ten or more feet into the slab. Investigation of such cases seems to point to defective installation of the bituminous expansion joint, which has allowed the concrete no room for expansion at the sides owing to the use of too short a length of joint-filling material. On the other hand, the lateral resisting moment of the slab is least near the joint, and for this reason it is natural that longitudinal cracks should begin at the joint.

(*c*) *Diagonal Cracking.*—Diagonal cracking is another type of cracking which is particularly objectionable since it produces acute-angled projections in the slab which are readily broken off. Proper design and careful subgrade preparation will practically eliminate this trouble.

(*d*) *Corner Cracking.*—Corner cracking is noticeable particularly under soft subgrade conditions, heavy loads and narrow surfacing. It is indicated that the frequency of such cracking is increased by a sudden lowering of the surface temperature such as occurs at night, the corners of the slab then being warped away from the subgrade. Such cracks occur on the average from, perhaps, 2 to $3\frac{1}{2}$ ft. from the corner. The small slab thus produced is readily pounded into the subgrade and high maintenance expense results. Under unfavorable conditions, this becomes a serious defect in concrete roads which is now being remedied by a design which calls for a surface thicker at the sides than at the center, just the reverse of concrete road practice of several years ago.

HAIR CHECKING

In a number of concrete road projects, minute cracks appear on the surface very shortly after the concrete is placed, and these constitute a minor defect which might possibly interfere with the future integrity of the pavement. Several causes for this defect seem to have been demonstrated. It seems to be pretty definitely established that hair checking in some cases is due to the unusual capacity of some soils to draw moisture rapidly from the concrete. Then, too, the non-uniform swelling of a non-homogeneous subgrade upon the absorption of moisture is a possible cause for hair checking and cracking. Wetting down of the subgrade for several days previous to the placing of the concrete lessens this defect. It has been observed likewise that hair checking is accentuated on very warm and windy days. The careful laying of strips of wet burlap over the surface as soon as practicable after finishing seems to be an aid in reducing hair checking. Observation seems to point to dirty aggregate as another cause for this trouble. In a number of pavements, hair checking does not seem to be extending with the continued use of the pavement, nor does any particular harm seem to result. Nevertheless, this is a construction defect which offers possibility for harm.

SURFACE IRREGULARITIES IN PAVEMENT AS CONSTRUCTED

(*a*) *Due to Lack of Uniform Consistency of the Mix.*—Smoothness of the concrete pavement surface is sometimes rendered impossible, notwithstanding good finishing, due to lack of uniformity in the consistency of various batches of concrete. Because of unequal settlement of the alternate wet and dry mixtures, bad riding qualities and hence more rapid depreciation of the pavement result. This defect can be avoided by proper care to maintain a uniform consistency.

(*b*) *High Joints.*—This is a defect produced through poor surface finishing at the joints and can be eliminated if use is made of a templet for insuring the evenness of the surface finishing.

(*c*) *Slumping of Concrete on Grades.*—It has often been observed on grades of over 4 or 5 per cent that the pavement is noticeably rougher than on level stretches. This is due to the fact that the wet concrete has slumped or moved down grade after it has been finished to an even surface. This trouble is aggravated in cool weather when the concrete sets very slowly. It is difficult to overcome when a combination of heavy grades and cool weather is encountered during construction. In such cases the final finishing must be delayed until the concrete is stiff enough to retain its position after finishing.

(*d*) *Unevenness of Side Forms.*—This is an inexcusable construction defect and results in an uneven surface.

SETTLEMENT OF THE PAVEMENT

(*a*) *Adjacent to Bridges and on New Fills.*—Many pavements become defective in time due to settlement of new fills on which they are constructed, particularly adjacent to bridge abutments. The surest remedy seems to be to defer the placing of the pavement until the vertical settlement of the earth is complete. Heavy reinforcement is also of value under these conditions.

PAVEMENT "BLOW-UPS"

Blow-ups of the pavement are almost always occasioned by the expansion of the concrete and they occur at the apex of grades and at other places which can be traced to faulty construction. In a number of cases examined, it has been found that these blow-ups occurred on pavements which were laid during cold weather without expansion joints, and resulted largely from what might be called "tapered noon-day joints." In cases where no attempt has been made to construct a vertical joint at the noon-day interval and the length of time is such as to permit the initial setting of the concrete, a "tapered joint" results. These joints in all cases constitute a plane of weakness which ruptures usually from expansion stresses.

SLIPPING OF JOINTS

Where bituminous-filled joints are used, in a number of instances there seems to have been a decided tendency for adjacent slabs to slip. Extraordinary care should be taken to insure the placing of these joints at right angles to the pavement surface. A few degrees variation from a right angle can cause trouble. Such slipping constitutes a very serious defect and generally results in very rapid disintegration of the slab and the necessity for its renewal.

SPALLING AT CONCEALED JOINTS

Oftentimes the mistake is made of placing prepared joint fillers so as to form "concealed expansion joints," a thin layer, say one inch, of concrete covering the joint material. This defeats the purpose of the joint filler. When the concrete expands, the excessive compression produced in the thin web of concrete at the surface spalls off a layer of the surface and produces an inequality which requires expensive maintenance and seriously mars the appearance of the pavement. Such defects can be entirely avoided by proper design of the joint.

SURFACE SCALING

(*a*) *Too Fine an Aggregate.*—It seems to be evident that sand containing too high a percentage of particles passing a 100-mesh sieve will produce surface scaling, probably because of the accumulation of the finest material at the surface. For this reason it is well to limit the amount of material by elutriation to less than three per cent, and there should be not more than five per cent of particles passing the 100-mesh sieve.

(*b*) *Too Wet a Mixture and Excessive Finishing* are probable causes for the accumulation of a weak surface layer which will begin to scale at the end of several years. Particular care must be taken not to use a consistency wetter than is necessary for the proper working of the concrete.

(*c*) *On Account of Improper Curing.*—On a number of sections of concrete work which have been cured by surface application of calcium chloride, scaling of the surface has been noted after the pavement has been opened to traffic. Experiments have shown that this scaling is due to the softening of the pavement surface by the addition of too large a quantity of calcium chloride. Even as low a quantity as 3 lb. per sq. yd. of calcium chloride distributed over the surface has produced scaling. Surface scaling is a serious defect since it may lead to the rapid disintegration of the pavement. The present tendency in those localities which permit this form of curing is to limit the amount of calcium chloride to $2\frac{1}{2}$ lb. per sq. yd. Apparently this method of curing should be used with considerable caution.

(*d*) *Freezing.*—Concrete road construction is often carried on beyond the safe working season, and notwithstanding precautions against freezing, many concrete pavements have been nipped by frost. In most cases, the surface only is attacked, and as a result the surface layer becomes detached from the main body of the concrete, exposing the coarse aggregate and leaving a rough surface which is subjected to the possibility of rapid deterioration. The safest plan is to stop concrete pavement construction when the temperature is apt to approach a freezing temperature, but if economy demands that the road be finished before the approach of winter then the usual cold weather precautions for concreting should be applied. It would seem also that an accelerator might well be used under such conditions.

SURFACE RAVELING

In a number of instances it has been noted that the surface of the concrete, especially in the vicinity of cracks and expansion joints, is showing excessive deterioration due to raveling. In other cases the entire surface is showing signs of disintegration. In some of these instances, undoubtedly the extremely smooth surfaces presented by the coarse aggregate are responsible. In others, the extremely poor quality of the coarse aggregate and of the fine aggregate, or of the concrete in general is responsible for rapid deterioration.

SURFACE WEAR

The general surface wear which takes place on concrete pavements is quite insignificant, although there are several cases where very noticeable wear has taken place. For illustration, pavements finished late in the fall and thrown open to winter traffic have been badly grooved in localities where snow has led to the tracking of motor vehicles equipped with tire chains. In certain localities the local aggregates are exceedingly soft and friable. Pavements constructed of excessively soft materials such as these are showing signs of wear which may be a determining factor in their life. In general, however, well-constructed concrete pavements in which aggregates of a satisfactory quality are used do not wear excessively under the action of our present-day rubber-tired traffic and it is hardly likely that general surface wear through abrasion will ever be a factor in determining the life of a concrete pavement.

DISINTEGRATION OF CONCRETE

(*a*) *Alkali.*—The disintegrating effect of certain types of alkali, principally the sulfates of sodium and magnesium, is well established, and cases are on record in our Western districts in which alkali seems to have played a prominent part in causing rapid disintegration of certain concrete pavements. As the alkali must reach the pavement through capillary action, it would seem that where concrete pavements are to be laid on a soil highly charged with alkali salts there should be interposed between the concrete and the underlying soil a layer of material having low capillary action. Several inches of sand should be quite effective for this purpose.

(*b*) *Defective Materials.*—Concrete roads, like other concrete structures, have disintegrated because defective materials have been used. Projects have been constructed in spite of laboratory reports condemning the fine aggregate, and these projects have shown signs of disintegration within a comparatively short period after opening them to traffic. Dirty aggregates, especially those containing clay-covered particles, organic matter in the sand, fine aggregate which has too high a percentage of fine particles and clay, all have been used in concrete road construction, and all have resulted in poor concrete. Screenings for use as a fine aggregate are to be looked on with considerable suspicion and certainly when they are permitted their grading must be governed so as to insure a workable concrete which will not be highly absorbent and which will not contain a high percentage of dust. Undoubtedly, too, defective cement has found its way into concrete road construction.

There is no excuse for poor concrete roads from any of the above causes. Unfortunately, however, careless or ignorant inspection coupled with neglect of laboratory warning has resulted in poor materials being used in a number of concrete roads in the past.

SURFACE POCKETS

Surface pockets are to be found in a number of concrete roads. These are caused by soft pieces of material in the aggregate which have been deposited or have gravitated toward the top surface during the finishing process. Such pockets are to be found in roads built with all types of aggregates but particularly in those having gravel aggregate. They are due to clay balls, soft pieces of gravel, chips, coal or other foreign substances which have come to the surface of the concrete.

Light slag aggregate when used in concrete road surfaces produces pitting of the surfacing. In general, however, when these pits are of comparatively small size, say not over one inch in diameter, they do no particular harm and are not to be considered as a particularly serious defect.

With the possible exception of cracking, there is hardly a defect listed and described above which cannot be almost entirely eliminated from concrete roads through the use of proper design and construction methods, satisfactory materials and their intelligent combination. The facts which have been gained by observation of existing roads and by special investigations are giving the progressive engineer information which will place him in a position to practically eliminate all of the serious defects of concrete roads. Lack of knowledge of the mechanical principles of design, inappreciation of the necessity for careful attention to details of construction, the use of poor materials, failure to apply the known principles of good concrete manufacture, failure to obtain homogeneity of both subgrade and concrete have led to all of the defects we have been discussing. Probably, defective materials which our ordinary tests have not detected are responsible for some concrete road failures. Certainly, however, these defects are but a small fraction of a per cent of the total concrete mileage and the main defects can be avoided through the application of present knowledge.

Concrete for pavements must have high flexural and tensile strength in order to minimize the frequency of cracking, it must have high density to better enable it to exclude moisture; the aggregates must be well bonded together to resist the disintegrating action of traffic, and the mixture must be homogeneous in proportions and consistency.

Perfect concrete road building is an intricate operation which too often is entirely under the control of inspectors who are not well versed in the technique of materials or construction. They do not understand the great importance of careful attention to details. By all means, concrete road inspectors and, in fact, inspectors on other types of concrete construction should be schooled in the causes and effects in connection with the various operations under their control. It is absurd that millions of dollars worth of materials and expensive construction should be left at the mercy of any but the most competent of supervision.

I think I can safely say, notwithstanding the fact that the above defects in one form or another are to be found distributed over the thousands of miles of concrete roads in this country, the concrete pavement has paid for itself in traffic returns many-fold and has rendered most excellent service.

BEHAVIOR OF CONCRETE EXPOSED TO SEA WATER

By S. C. Hollister[1]

The behavior of concrete in sea water has aroused considerable speculation in recent years as to its suitability for marine use. Structures along both coasts have shown that, under certain conditions, concrete is seriously affected by sea water. The attacks in some instances have been so severe that partial or total replacement has been necessary. Of the many structures of plain or reinforced concrete exposed directly to sea water for the past ten years, few are free from some degree of deterioration.

The nature of the deterioration of plain and reinforced concrete by exposure to sea water is distinctly divided into two classes: (1) the deterioration resulting from the corrosion of the reinforcement, and (2) the disintegration under certain conditions of the concrete itself. Both forms are found only above the level of complete immersion. Attack is found regardless of whether sea or fresh water was used for gaging; whether the structure was cast in place or formed of precast units; or whether construction proceeded in the open air or under water. The deterioration seems to bear little relation to the variety of mixtures usually employed in construction work. Faulty placing or excessively wet mixture, however, hastens the attack.

The attack on reinforced concrete begins with the corrosion of the reinforcement. No corrosion is found where there is complete immersion. It begins just above the mean water level, in from two to five years after construction. A two-inch covering of $1:2\frac{1}{2}:5$ mixture or richer will protect the reinforcement from corrosion by fresh water; but no concrete mixture in this thickness will protect the steel from sea water corrosion.

Due to the expansion accompanying corrosion, the concrete covering is split in the direction of the reinforcement. These cracks permit more ready ingress of the sea water and the attack proceeds with increasing rapidity. Splitting does not extend below low tide to any great extent, because corrosion does not take place there; instead, it progresses upward, not only within the wave or spray belt, but as far as salt-laden air is found. A satisfactory permanent protective coating for the steel which does not seriously reduce the bond has not yet been found.

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[1] Light, Hollister and Ferguson, Engineers, Philadelphia.

Once a portion of the surface of the concrete has been spalled off, the sea water attacks the concrete mass in the same manner as in plain concrete structures.

The attack on plain concrete begins at the mean water line and extends in diminishing degree upward and downward from that level. It originates wherever the sea water can gain access to that portion of the concrete mass lying within the surface skin originally formed over the exterior of the structure. Sometimes this access is provided through a honey-comb, or through a layer or pocket of laitance; at other times it is provided through the removal of the surface skin by mechanical abrasion or by frost action.

Once the sea water has reached the inner concrete, chemical disintegration of the cement develops. The matrix is softened in time to such an extent that the concrete can be scooped out with the hand. Magnesium chloride, and especially magnesium sulfate, in the sea water appear to be the principal agents of attack. Calcium sulfate in considerable quantity is formed; and this compound having no setting properties under the conditions, the mass may be washed away by stream or wave action. Mechanical abrasion and frost action hasten the chemical process of disintegration.

Chemical disintegration begins at once after the inner concrete is reached by the sea water. Marked disintegration has been noted in two years where honey-comb or laitance forms the path of communication. Carefully made structures two years old, so situated as to be subject to abrasion by floating ice, have shown well-started attack. In some cases, the concrete has been wasted away for a depth of from eighteen inches to two feet on structures eight to ten years old.

To date, little has been learned of a constructive nature concerning the arrest or prevention of this wide-spread attack on concrete marine structures, other than to armour them with treated timber, stone or paving brick. Little is accurately known of the chemistry of the concrete disintegration. Cements other than portland are advanced for marine use; but although each may have some instances of successful use, there is no well-defined conclusion of preference over portland cement. The lack of thorough understanding of all cements is a handicap in the solution of the problem.

There is needed a well-organized investigation into the means of prevention of the attack of sea water. Scattered observations have been made during the past fifteen years, but they are so unorganized that the conclusions gained separately are not convincing. The magnitude of the problem, especially as relating to geographic variations, time required in testing, and fundamental research, necessitates a separately constituted body to carry on such an investigation. The great value of existing structures now seriously affected by sea water, and the rapidly growing use of concrete for marine construction because of its convenience and economy, make an early solution imperative.

This paper is not intended to contribute data to the solution of concrete disintegration in sea water. It will have served its purpose if the discussion it may provoke shall, first, contribute impetus to the much-needed research in this field, and secondly, contribute useful information.

BEHAVIOR OF CONCRETE EXPOSED TO ALKALINE CONDITIONS

By G. M. Williams[1]

The deterioration of concrete in the so-called alkaline soils has received prominence during the last fifteen or twenty years, following the development of irrigation in the Western states. In numerous instances, irrigation structures, foundations and other concrete works have shown signs of failure after short exposure to the salts in the soil. Various theories as to the cause of deterioration were advanced and it was first believed to be due to the crystallization of the soluble salts in the pore space of the concrete, which action was greatly accelerated by the use of poor aggregate and poor methods of preparation and curing. The problem was further complicated by the fact that not all concretes exposed to *apparently* similar conditions were attacked, tending to substantiate the theory that aggregates and workmanship were to blame. The possibility that the deterioration was primarily due to reaction between the salts in solution and constituents of the cement was also advanced but was not generally accepted owing to lack of experimental verification and the knowledge that materials and workmanship were of inferior quality in some cases.

Deterioration due to alkali has generally occurred in the arid or semi-arid sections of the Western states. The weathering and breaking down of rocks by long exposure to the elements has resulted in the formation of soil, and the salts of sodium, calcium and magnesium which were once among the constituents of the rocks are now found distributed throughout the soil, usually in greatest amount in the clays and shales. While most ground waters carry in solution appreciable quantities of one or more of these salts, it is in the arid regions that concentrations are sufficiently high to result in such waters being classed as alkali waters. In the humid region, heavy rainfall for many years has washed most of these salts from the soil, but in the arid and semi-arid districts, a smaller rainfall together with soils less easily drained have resulted in the conditions as now found. The excessive application of irrigation water, together with poor drainage facilities, has resulted in a gradual raising of the soil-water level which has brought about a high concentration of salts at the ground surface and a few feet below. It is this condition which is responsible for most cases of disintegration that have been noted, although there are localities outside of the irrigated sections where surface concentrations of salt are small and deterioration occurs some distance below the surface. Disintegration at or near the surface has no doubt been accelerated by frost action, but under the latter conditions frost action is generally absent.

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Investigations to study the cause of deterioration were started in those localities where the trouble was first encountered. The discovery, at about the same time, of disintegration of cement drain tile in Colorado[1] and of concrete sewers and culverts at Great Falls, Montana,[2] led to laboratory studies by State Agricultural Colleges of Colorado and Montana. Soon after, the Structural Materials Laboratories of the U. S. Geological Survey, then located at St. Louis, investigated conditions on irrigation projects in South Dakota, Wyoming and Montana and found cases of disintegration of concrete as well as stones and brick.

The conclusions drawn from these early laboratory investigations are well summarized by the following quotation from *Bulletin No. 81* of Montana State Agricultural College:

> "The disintegration of cement by alkali salts is principally due to reaction between these salts and the calcium hydroxide necessarily present in set cement. As a result of these reactions relatively insoluble new compounds are formed in the body of the cement structure. These new compounds have greater weight and require greater space than the Ca(OH) replaced. New compounds force apart the particles of cement, thus weakening or breaking the binding material."

A combined field and laboratory investigation[3] of the effect of both alkali soils and sea water upon portland cement was undertaken by the Structural Materials Laboratories of the Geological Survey, and later transferred to the U. S. Bureau of Standards. The conclusions drawn may be summarized briefly as follows:

> "Portland cement mortar or concrete can be disintegrated by mechanical means exerted by the crystallization of almost any salt in its pores if a sufficient amount of it is permitted to accumulate and a rapid formation of crystals is brought about by drying. Porous stone, brick and other materials are disintegrated in the same manner. In the presence of sea water and similar sulfate chloride solutions the most soluble element in the cement is the lime. If the lime of the cement is carbonized it is practically insoluble."

In both of the foregoing investigations, which were completed prior to 1910, it is concluded that deterioration is not due alone to mechanical and physical disintegration, but to the dissolving effect of the soluble salts on constituents of the cement. That portland cement could be disintegrated in the laboratory was well established, but it was also recognized that laboratory conditions were not in every way comparable with those encountered in actual practice.

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[1] *Bulletin No. 132*, Colorado State Agricultural College.

[2] *Bulletin No. 81*, Montana State Agricultural College.

[3] U. S. Bureau of Standards, *Technologic Paper No. 12*.

The first extensive field investigation[1] was that undertaken by the Bureau of Standards in 1913. This involved the manufacture in a commercial factory of sixteen varieties or types of 8-in. cement drain tile and shipment of a carload to each of eleven localities, in eight of which severe alkali conditions would be encountered. These tests of drain tile were later supplemented by the installation of concrete blocks in eight districts in the west. The ground waters in these districts varied considerably in concentrations and constituents, but in most cases the sulfates predominated. In one or two instances the quantities of chlorides and sulfates were practically equal. Sodium or magnesium was the base usually present in greatest quantity, with much calcium in a few cases.

This investigation, comprising as it does concretes and mortars of the highest quality and identical specimens exposed in widely separated districts, has resulted in confirming and extending the conclusions drawn in the previous studies. These may be summarized as follows:

1. The best quality of mortar or concrete may be disintegrated in sulfate soils and waters. The deterioration in chloride or carbonate waters appears to be very slow or almost entirely lacking.
2. Rapidity of disintegration is proportional to the sulfate content of the water.
3. Frost action appears to accelerate disintegration in sulfate waters.
4. Carbonizing of the lime at and near the outer surface of a structure will not prevent disintegration. While this carbonized coat may be immune or very slowly affected, it is permeable and the uncarbonized portions within may be attacked and result in the shelling off of the carbonized surface.
5. Tar protective coatings are not permanent.
6. Concentrations of alkali waters may vary as much as several hundred per cent in distances only a few feet apart.

The alkali-concrete problem of the Western states is also encountered in the Prairie Provinces of Canada. In 1918, the Calgary Branch of the Engineering Institute of Canada started a field investigation[2] by exposing blocks of different qualities of concrete, some of which were coated with various surface preparations, to sulfate waters. Observation of changes taking place has led to practically the same conclusions as were drawn from the Bureau of Standards' study.

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[1] U. S. Bureau of Standards, *Technologic Papers Nos. 95 and 214.*

[2] *Journal*, Engineering Institute of Canada, September, 1921.

In 1919, test specimens were prepared at the University of Saskatchewan and exposed to sulfate waters at Saskatoon. Here the water was encountered at a depth of 6 to 8 ft. below the ground surface, thereby eliminating possibility of frost action in the tests. Good quality of concrete was affected and in one instance an inferior quality a small distance away was apparently unaffected. Analyses of the soil waters showed a much higher concentration surrounding the higher quality concrete.

In 1921, the Structural Materials Research Laboratory of Lewis Institute commenced an extensive field investigation by installing concrete specimens of widely different qualities in two alkali districts in the United States and supplemented by the exposure of smaller groups in three districts in Western Canada. While sufficient time has not elapsed to draw definite or final conclusions, the results obtained after one year's exposure are very similar to those found in the same length of time by the previous investigations.

All of the foregoing investigations may be considered as establishing the first phase of the investigation—definitely fixing the problem—and demonstrating the fact that disintegration is primarily a chemical action, rather than a physical disruption, and indicating the variable factors and conditions which must be met in practical work. The second phase of the work involves the discovery of means, materials and methods for eliminating the possibility of disintegration in future work. Two distinct methods of approach are available. It is possible that a continuation of the field studies by the preparation of many different concretes containing various materials may lead to the production of a concrete which will be immune to attack. The second method is to study in the laboratory the constituents of cement and the reactions which take place when they come in contact with the different salts found in the soil. This, of course, requires a study of the reactions involved in the hardening and setting of cement, which in itself is a subject concerning which much is yet unknown. This second method is the scientific method of attack and will no doubt be more effective than the former.

A Committee on Deterioration of Concrete in Alkali Soils, appointed by the Engineering Institute of Canada in 1921, after considering the conclusions reached by previous investigators, decided that little further progress could be made by field studies and that its energies should be mainly directed towards a study of the chemical reactions involved. As a result, only sufficient field work is being done to try out in a practical way a number of waterproofing and alkaliproofing compounds which are now on the market, while the greater portion of a $40,000 fund contributed by the Research Council of Canada, the Prairie Provinces, cities and interested corporations is being devoted to chemical research. While it will be some time before definite results can be expected from the chemical research, it is interesting to note that conclusions drawn from previous investigations as to the action being primarily chemical have again been verified. No new conclusions are as yet available from the field tests except that the results obtained with admixtures and integral compounds have without exception been discouraging, which further emphasizes the committee's decision that all efforts at present should be directed toward the chemical research.

Additional investigative work which should add much to our knowledge of this problem is that now being conducted in connection with the work of the Society's Committee C-6 on Drain Tile. At Iowa State College the effects of various salt solutions on mortars are being studied, while the Drainage Division of the U. S. Bureau of Public Roads, in cooperation with the state of Minnesota, is doing much to improve the quality of drain tile in that state where deterioration of cement tile has occurred.

The status of the use of concrete in alkaline soils may be summed up briefly as follows:

1. In many districts of the Western states and Canada there are areas where the salt content of the soil is high and concrete cannot be used with assurance of success.

2. Study of field conditions indicates that deterioration of concrete structures is almost entirely confined to regions where the sulfate type predominates. Good quality concrete is apparently being successfully used in chloride and carbonate soils.

3. Rapidity of disintegration varies directly as the sulfate concentration.

4. Where concentrations are permanently low, good quality concrete appears to have a life which fully justifies its use.

5. Concentration of salt in ground water may be extremely variable at points short distances apart. There also appears to be seasonal or yearly variations in some districts.

6. Concrete of high quality is most resistant to action. High quality for exposure to alkaline conditions is measured by impermeability or resistance to passage of water through the mass under pressure. The one big factor necessary to secure low permeability is high cement content, so that high compressive strength can generally be used as a criterion of low permeability. Consistency, within practical working limits, and gradation of aggregate are secondary factors in obtaining a concerete of low permeability.

7. Where alkali conditions are bad, the factor of safety against failure can be greatly increased by employing proper drainage precautions.

8. The employment of a membrane waterproofing may prevent action for a time at least, but such a method is not practicable under all conditions.

9. Portland cement, as now constituted, is inherently subject to attack by sulfates in the soil and ground water and the practical and final settlement of the alkali-concrete problem is dependent upon modification of, or the discovery of some material which may be added to, portland cement to make it immune to such action.

DISCUSSION

Mr. Spackman

MR. HENRY S. SPACKMAN[1] (*presented in written form*).—Two important questions have been raised by Mr. Bates' paper: First, is the practically exclusive use of portland cement by American engineers and constructors for all purposes justified? Second, was the adoption of a uniform specification for portland cement, regardless of the use for which it was intended, a wise action?

So long as the cement user thinks only in terms of portland cement and the specifications insist on a uniformity of type and quality, there is small encouragement for the cement manufacturer to strike out on original lines and produce cements adapted to special uses. As pointed out in Mr. Bates' paper, cement is used under three conditions of exposure each differing radically from the other. Is it reasonable to suppose that one uniform cement will fulfill all the requirements of these widely differing conditions of exposure as well as they could be met by cements especially developed to meet the requirements of the particular use to which it was to be put? We do not use one grade of steel or one grade of brick. Why, then, insist on only one grade of cement?

As indicated by Mr. Bates, portland cement as it is made to-day, though in the main a satisfactory material, is far from perfect. The durability of concrete is dependent not on the durability of the cement but on the protective action of the aggregate. This is evidenced by the fact that neat cement is never used in construction work. As used in construction, portland cement is purely and simply a binder or glue. As a binder it fails to give complete satisfaction because of the relatively slow reaction with water of some of its components. As a consequence, the strength of concrete is a question of weeks or months instead of hours or days and the final strength developed is always subject to variability, because of the impossibility of maintaining on the work, uniform conditions suitable to secure the best results over the period required for the cement to become completely hydrated. As a result, especially where the concrete is subject to rapid drying out, we may have a considerable percentage of partially hydrated or inert cement which not only gives no strength but may be a source of weakness at some future time through expansion caused by its delayed hydration should water come into contact with it.

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[1] Consulting Engineer, Philadelphia, Pa.

Mr. Lesley MR. ROBERT W. LESLEY.[1]—I have listened with great interest to the papers presented this evening and to the remarks of the various speakers in connection therewith. It seems to me that in order to get a thorough grasp on the question under discussion some reference to early history might be of use.

We all know that the manufacture of portland cement, in this country especially, has only attained its great extent since the beginning of this century; and we all know that concrete building construction, which in a commercial way had its inception in France and with French engineers, began its growth at about the same time. In the early 90's I visited Paris and was in touch with Coignet, Bordenave and Hennebique, the then leading concrete constructors of France, and through their assistance undertook to develop work of the same nature in the United States, visiting while in France some of the factory buildings and other work of moderate height then constructed or under construction. At my suggestion, for the purpose of carrying out plans then made, Ira A. Shailer, a skilled and distinguished engineer and one of the contractors on the Croton aqueduct system, visited Paris to investigate from a technical standpoint the work done and the methods of construction. On his return to this country I consulted with him and advised him of the funds raised for the enterprise, but was surprised to have him say that he would have nothing to do in connection with the building of such work in this country, as he was convinced from his investigations that with American labor, American methods and American supervision he could not risk his reputation as an engineer in work of this kind. This was the conclusion of an important engineer, himself familiar with concrete dams, aqueducts and other similar construction.

Now, from the papers and addresses that I have listened to this evening, I have come to two conclusions: First, in practically everything that has been written or said there is little or no serious criticism of portland cement as a building material when properly used, with proper supervision, proper planning and careful labor; and, second, that for the most part the criticisms that have been leveled at concrete construction this evening may in a large way be classified to the use of concrete under conditions where it was never intended it should be used or on occasions where imperfect planning, imperfect supervision or imperfect labor conditions existed.

From these facts I draw the further conclusion that as a material for construction and for engineering work generally, portland cement, which from 1900 on has been constantly increasing in demand, has

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[1] President, Surface Combustion Co., Philadelphia, Pa.

Mr. Lesley

proved itself a safe, reliable and permanent material for engineering and architectural use. In this connection it must not be lost sight of that the improvements in the manufacture and in the testing of portland cements have materially contributed to the confidence that engineers and architects have had in the material, and have led in some cases to its use where it was never intended to be used.

As the first chairman of Committee C-1 on Cement of the American Society for Testing Materials, it has been my privilege to see a standard specification in uniform use by manufacturers, engineers and consumers, and to see the growth of a standard, uniform, reliable and satisfactory portland cement under these specifications. Prior to that time and during some of the investigations into standard cement specifications, as a manufacturer, in various periods of a checkered career, I certainly produced almost fifty-seven varieties of cement, such as natural cement, improved cement, portland cement, selinitic cement, sand cement, silica cement, slag cement, Puzzolan cement and other kinds "when called for." While many of these materials were excellent of their kind and had their particular function in particular fields, my feeling from the manufacturers' standpoint is that if we were at this time to start in manufacturing various types of cement to suit each and every condition of engineering and construction, or to cure this or that particular disease, which some of the papers this evening refer to, we might find that the cure would be worse than the disease and that we had too many varieties of cement to make a good, reliable, standard article. That was our trouble when we were manufacturing fifty-seven varieties of cement to meet almost as many varying types of specifications, a long list of which is given in the paper I read before this Society many years ago.[1] This is a trouble that is likely to come again if we undertake to make cement on the principle of "making the punishment fit the crime." We may even give our patient the wrong medicine by accident, through our fault or through improper diagnosis by the engineering doctor.

I do not mean to say that the *ultima thule* of portland cement manufacture has been reached. Quite the contrary. I must agree with the study Mr. Bates has made, and from the standpoint of one who has assisted at the birth of the cement industry in this country, I feel there is much for us to learn, much for us to improve and much research for us to do; but I am still of the opinion that this portland cement of ours, which has grown in production within my ken from 5000 barrels a year to nearly 125,000,000 barrels in this good year

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[1] R. W. Lesley, "Some Statistics of the Cement Industry in America," *Proceedings*, Am. Soc. Testing Mats., Vol. IV, p. 448 (1904).

Mr. Lesley 1923, cannot be such a poor job lot thing after all. When I come to a Symposium of this kind and find engineers from all parts of the country engaged in all kinds of work speaking on the subject, I find that the burden of their complaint carries but little criticism of the cement itself but much as to the way the concrete is used, and in some cases the improper uses to which it is put.

Looking back again to portland cement made in the late 80's and the early 90's, and looking at and studying the work that has been done with it and the permanence thereof, I am convinced that the portland cement of to-day is a worthy successor to the portland cement of the early days and shows a constant improvement in manufacturing practice. The real test is the test of time; of this test the hundreds of millions, nay possibly billions, of dollars of work in every field bears evidence. While we must admit that in this field, as in every field, as long as the world moves there is improvement to be made, it is well to consider the character of the improvement, the result, and the permanence of the material that is to be made. All this must be thought out before going into radical changes of specifications or into the manufacture of radically new cements.

Now, as was well said by Mr. Bates, there are many investigations that might be made into the various types of grinding apparatus. This is a field in which little research has been done. The question is whether one mill produces a type of crystallin fracture in the fine material produced, and whether another produces a flaky fracture; or whether one pounds and the other tears the material. All this may have some bearing on the manufactured product and is worthy of investigation.

So, too, in research in the matter of kilns. All of the old portland cement was originally made in vertical kilns, which gave excellent results. As evidence of the test of time, this morning I passed over a floor in the train shed of Broad Street Station which was laid with Dycherhoff German portland cement about twenty years ago, over which hundreds of millions of feet have passed, and it is certainly evidence of as good cement as has and will ever be made. On the other hand, no one can visit any of the great work on the Croton aqueduct, the New York subway, or any of the great skyscrapers in our principal cities without seeing evidence of the quality of the rotary kiln cement now manufactured. While I feel that we owe to the engineers who are here and to Mr. Bates for his deeply reasoned paper our fullest appreciation, I still adhere to the proposition and the thought that while material that has shown the gain that cement has shown, that has lived under all the uses that it has lived under, that

Mr. Lesley

has stood all the abuse it has stood, may still be improved on, this fact has got "to be shown."

Mr. Purrington

MR. WALLACE F. PURRINGTON[1] (*presented in written form*).—Discussing the influence of aggregates, the general statement in many specifications is that sand shall be made up of "sound, hard, durable fragments" without trying to define any of these terms. One of the most common rocks is granite. The breaking down of this rock under exposure forms sand composed of grains of quartz and feldspar. A casual glance is enough to show that the feldspar particles in nearly all feldspathic sands are at least partially decomposed and disintegrated, and by no means "sound and hard." The microscope will show cleavage fractures in a great many particles and the dull earthy appearance of most grains, together with their clayey odor, shows that softening due to chemical change or "kaolinization" has set in. Here it would seem is a potential source of trouble for the future.

Actual count of sand particles in many of our New Hampshire sands shows 50 per cent and over of feldspar grains. Concrete made under standard conditions seems to point to the fact that they show an initial weakness. Whether or not this is progressive, time alone will tell. This subject is now being studied by the New Hampshire State Highway Department in cooperation with the U. S. Bureau of Public Roads.

The question of strength of concrete is not apparently the big question of issues here, but one thought has been brought out so far by our study of sand and that is, the *shape* of the grain of sand is a factor in the strength in laboratory specimens of concrete. Sands made up chiefly of slate chips or splinters derived from metamorphosed sedimentary rocks, such as the slaty or ferruginous schists, make concrete that shows higher strength than that made with the granular sands from the igneous rocks, such as the granites and porphyrys. The slate chips appear to act as fibrous material and interlock, while the granular sands do not and consequently show more definite cleavage lines on fracture.

We cannot help but believe that at least as much stress should be laid upon the microscopic and macroscopic count of the various sand particles to determine *what the constituents are*, as upon any arbitrary system of strength tests which have so far been devised.

Mr. Crum

MR. R. W. CRUM[2] (*presented in written form*).—I wish to call attention to the fact that there is a need for more extensive research

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[1] Chemist and Testing Engineer, New Hampshire Highway Department.

[2] Engineer of Materials and Tests, Iowa State Highway Commission.

Mr. Crum into the practices and conditions governing the actual fabrication of concrete. In the last few years extensive laboratory investigations of concrete materials and their combinations have given us much truly scientific information on the design of concrete mixtures, but the art of actually combining and placing the materials is far behind the scientific methods that may be used in designing the mixtures.

For instance, in proportioning the mixture, the stated proportions are based upon certain data concerning the mechanical grading and other properties of the aggregate to be used. Upon a large paving operation using a train load of material every day, it will be found that there is actually a wide variation in the grading of the aggregate from hour to hour. We need either methods for keeping these materials uniform, or methods of use which will obviate lack of uniformity in the concrete on account of lack of uniformity in the aggregates. One solution may possibly be to use the fine and coarse aggregate in such a ratio that variations in grading in the coarse aggregate will have a relatively small effect, due to the fact that the voids are overfilled with mortar.

As to measuring the materials, the almost universal practice of measuring the aggregates by loose volumetric measure is certainly not susceptible of refinement commensurate with the refinement possible in designing the mixture. There is a great possibility that measurements by actual weight can be more accurately made. The Iowa Highway Commission is this season supervising a paving contract in which the aggregates are being actually weighed for each batch. The contract is for twenty-five miles of eighteen-foot 8-in. single-course concrete. The installation is a central mixing plant. Each batch consists of fourteen bags of cement, 2856 lb. of sand, and 2856 lb. of gravel. The conditions are very severe, and the data secured from the season's work should prove of great value.

Every other step in the making of concrete also presents its problems. In order to solve such problems as these, a large amount of patient field work must accompany the laboratory investigations, and therefore the agencies controlling the making of large volumes of concrete must become actively interested in their solution.

I wish also to second the statement by Mr. Bates that we need much more information as to the varying characteristics of the cement itself. Our knowledge of the properties of portland cement is really less than our knowledge of the properties of the other concrete materials and intensive laboratory study is certainly needed.

Mr. Campbell MR. JOHN T. CAMPBELL[1] (*presented in written form*).—While our

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[1] Engineer, The J. N. Chester Co., Pittsburgh, Pa.

Mr. Campbell experience with concrete for exposed structures has in the main been very satisfactory, there have been several instances where concrete in service from seven to ten years has crumbled and disintegrated to such an extent as to endanger the structures and require extensive repairs, in some cases amounting to complete rebuilding.

The first case of this kind which was brought to our attention was that of the floor of a bridge constructed for one of the boroughs in the vicinity of Pittsburgh during 1912 and 1913. This bridge, which was designed by our firm and built under the supervision of our engineers, was of reinforced concrete throughout, the floor being of reinforced slab, beam and girder construction. The concrete consisted of one part portland cement, two parts sand and four parts gravel, and the floor was poured in the spring of 1913. The unit stresses employed were quite conservative and the methods of computation followed the best engineering practice. The sand and gravel were not subject to the tests which are now required, but the cement was tested and successfully passed the requirements of the American Society for Testing Materials. The materials were mixed in concrete mixers, transported in buckets to the site by cable-way, and dumped into the forms, being thoroughly tamped and joggled into place. The concrete was what is known as moderately wet concrete, as at that time wet concrete was considered to give better results than dry concrete.

In 1920, the writer made a detailed examination of the bridge floor and found that a number of the beams, girders and a great portion of the sidewalk, slab and curbs had disintegrated, checked, and spalled off to such an extent that the structure was considered unsafe. This structure had not been subject to any loads which would produce stresses equal to those for which it was designed. An examination of the beams, especially at their ends where they joined the cross girders, showed that the concrete was hollow; when struck with a hammer it gave forth a hollow sound such as produced by beating a drum. Hammering on this concrete caused it to break off in layers which were more or less stratified or separated by white powder consisting largely of hydrated lime and calcium sulfate. This material had crystallized on the inside of the beams and girders and, in the writer's opinion, was the cause of the cleavage planes. That is, the material when crystallizing occupied more space than existed between the particles, and the excessive internal forces thus set up tended to disrupt the concrete. The concrete, after being dug out from the beams and girders, could easily be broken in the hands and the fractures always showed the white crystallin substance

Mr. Campbell along the planes of fracture. The gravel could be picked out from the mass, leaving perfect impressions which in all cases showed this fine white material separating it from the mortar in which it was embedded. The disintegration was pronounced on the outside beams and girders exposed to the weather. At the expansion joints, where the water had an opportunity to come into contact with the structure, the disintegration was even more pronounced and the concrete at these joints could be pulled to pieces with very little effort.

Our opinion is that this condition was due to the concrete being not impervious to water and the water getting into the concrete dissolved out the soluble portions of the cement and crystallized them inside the structure, breaking out the concrete in the process; this being repeated and the condition rapidly growing worse, due to a greater opportunity being afforded for the water to gain access to the interior of the concrete, resulting in faster and faster disintegration until after seven years of service the floor was ready for replacement.

A similar condition was found to exist on the outside walls of a settling basin and of a filtration plant constructed in 1909 in the vicinity of Pittsburgh. The writer made a personal inspection of this work which, however, was not built under the supervision of our firm. From the specifications which were furnished us the walls consisted of 1:2:4 concrete, the cement having been tested. No data were available as to the wetness of the mix or of the character of the sand and gravel. The walls where exposed to the atmosphere could be dug out with a small hammer very readily and the same cleavage lines containing white powder were found to exist. These walls were from 12 to 15 in. thick, with water pressure on one side, and the disintegration extended for a distance of from 6 to 9 in. from the outside toward the inside. The disintegrated portions were cut out with an air hammer and new material put back with a cement gun, reinforcing mesh being firmly secured to the existing walls by means of spikes grouted in place.

In two other filtration plants, the outside walls had begun to disintegrate in a similar manner and have been repaired with the use of a cement gun. We consider that the repairing of these structures in this way will afford a permanent and satisfactory job.

From our experience with disintegrated concrete we believe that any concrete that is not impervious to water will ultimately go to pieces, due to the fact that the water in going through the concrete dissolves out soluble portions of the cement causing crystallization to take place and expansion in the interior of the structure separating it into cleavage planes which allow more moisture to enter. During winter, alternate freezing and thawing will add to the disintegration and its progress will be greatly accelerated. We are attempting to make all our concrete structures as impervious as possible by using a combination of ingredients which will give the densest mixture, that is, a mixture with the least amount of voids. We cannot help but believe that the cement, which up to this time has never been suspected, has something to do with the disintegration in that chemical compounds form when the cement sets and the expansion and contraction which takes place when cement sets undoubtedly causes checks which allow the water to gain access to the structure; and when the path for water through the concrete is once opened up, the structure is doomed. The present tests for cement do not give us sufficient information concerning these characteristics of the material and unless further information is developed along these lines the writer believes that the only way to construct a job that will be absolutely safe is to use some form of hardener or surface finish which will absoultely close the pores of the concrete and prevent water gaining access, for our experience with concrete floors without such hardener has generally demonstrated that the floor will dust and become ragged and rough from usage in a short time, and that the surface thus presented is far from being impervious. In our recent designs we have endeavored to cover all concrete outside walls of structures with earth fill or brick veneer to give it protection against the rain and water, as we can never prophesy that the concrete, even though constructed with the best methods known to date, will not fall to pieces in the next seven to ten years. **Mr. Campbell**

MR. W. E. HART[1] (*presented in written form*).—It is the intention of this paper to place before you what I consider the principal difficulty with the so-called "outdoor" concrete referred to in our program this evening. The information was compiled after a careful study of a large number of early concrete structures and a personal inspection by engineers of the Portland Cement Association. Structures were inspected in all parts of the United States and reports filed. **Mr. Hart**

The Portland Cement Association welcomes a study of "out-door concrete" and will gladly assist those interested in the subject of weathering of concrete exposed to exterior conditions, with the one thought of overcoming the difficulty. Constructive criticism should be welcomed by any industry so long as such criticism brings results in the betterment of its product. Considerable has been said of late on the subject, "What Ails Concrete?" In the proper hands and before technical bodies such as this, a subject of this character is

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[1] Manager Structural Bureau, Portland Cement Association, Chicago, Ill.

Mr. Hart proper for discussion. Unfortunately, this information has fallen into the hands of the uninstructed, or layman, and produced in his mind an entirely different impression from that which was originally intended, that is, one of distrust in the use of concrete. At this point such criticism ceases to be constructive.

During the past five years there has been placed before the engineering profession facts on concrete making that should not be overlooked. This Society is primarily interested in tests and their practical application to construction work. Let us, therefore, interpret one of a series of tests in terms of everyday concrete. I refer particularly to the relation of mixing water to the resulting concrete.

The structures studied in this report were all built prior to 1905, or the period when nearly all concrete was placed very dry. Many of the construction methods used at that time were as far wrong as some of those used to-day. We must, therefore, strike a medium, or average, that combines the good points of each period. Many of the so-called concrete failures of to-day can be obviated by a judicious use of water before and after placing concrete. Fully seventy-five per cent of these failures are directly traceable to the use of too much mixing water and insufficient curing.

The production of good concrete requires work, and hard work at that. Density in concrete can only be produced by hard work, either manual or mechanical. If little or no effort is used in the placing of concrete, serious results may be expected. In order to produce a dense surface it is necessary to make a concrete of a dry consistency or with a very small slump. As the concrete is placed in the forms it must be well spaded and afterward kept wet for a number of days. Density is the particular characteristic we must strive for in the making of a concrete that will resist the action of the elements.

Beginning with the first of May, 1923, an inspection was made of a large number of structures all over the United States. Information on the construction of these structures was obtained first by a study of reports contained in the technical press prior to 1906. With this as a basis our various district offices throughout the country were requested to make a personal examination of the structures and report to us immediately. In the following paragraphs is given the name, location, date built, construction methods used, together with mix and manner of curing, if any, and present condition of the principal structures contained in this list. I will make this as brief as possible and still carry to you the information it should.

BRIDGES

Mr. Hart Between 1900 and 1902, the Illinois Central Railroad built a large number of concrete bridges in southern Illinois. In every case the concrete was of a consistency that when thoroughly rammed it would quake slightly, but not be wet enough to quake in the wheelbarrows. All concrete was placed in 6-in. layers and thoroughly rammed. Abutments and wing walls were mixed 1:2½:6 and arch rings 1:2:5. An inspection of these structures made on June 13 and 14 of this year indicates that they are in excellent condition. In a few cases spalling has occurred and some disintegration in abutment walls where drainage was improperly taken care of. In all cases the concrete is structurally sound. Four bridges were inspected, three of which show little or no disintegration. The fourth, as explained above, shows some disintegration on the surface.

In August, 1894, plans were prepared for a reinforced concrete arch with a 64-ft. span for a new driveway in Eden Park, Cincinnati, Ohio. This structure was built in 1905. The concrete for the footings and walls were 1:3:6 placed in 6-in. layers. The concrete in the arch was mixed 1:2:4 faced with a 2-in. colored mortar. The mix was later changed to 1:4. On May 17, 1923, this bridge was inspected and found to be in excellent condition. A very small amount of spalling has taken place on the arch itself and in a number of places the facing material has cracked. This spalling is on either side of a crack occurring through the center line of the arch, but the remainder of the structure is sound. No indication of weathering appears on the structure other than slight discoloration from water and dirt. The railing of this bridge is not in such good condition. This was pre-cast in units and placed after the structure was completed.

A reinforced concrete arch bridge over West Canada Creek, New York Central Railway at Herkimer, N. Y., was built in 1901–2, consisting of ten arches of 60-ft. span. The concrete was mixed very dry and placed by means of buggies. This structure was inspected on May 14, 1923, and is in very good condition. Little or no weathering is indicated.

DAMS

A dam 12 ft. high and 19 ft. long was constructed in 1904 at Cold Springs Harbor, New York. The concrete was mixed one part cement, 4 parts of sand and 2 parts of gravel. All concrete was mixed fairly wet and tamped into place. An inspection of this structure was made on May 10, 1923. The only point where disintegration has appeared is on the lower face where the apron joins

Mr. Hart the steps. The design used in this structure was of step-down type in order to relieve the rush of water. All other parts of the structure appear to be in excellent condition.

Crystal Springs Dam, San Mateo, Calif., was built between 1887 and 1890. The concrete was mixed approximately 1:2:6 and placed very dry, being rammed into place by heavy iron tampers. An inspection made on May 15, 1923, finds the structure in very good condition with no other deterioration whatsoever.

The Schuyerville Dam on the Battenkill River, N. Y. This structure was built in 1905 varying in mixes ranging from 1:3:6 to 1:4:8. All concrete was rammed into place in 6-in. layers. The concrete was placed during cold weather with temperatures ranging from 4 to 14° F. Salt on the basis of one quart per cubic yard of concrete was used to prevent freezing. An inspection made on May 16, 1923, indicates the structure to be in very good condition. Some scaling is indicated on a portion of the dam but to a very slight degree.

The Waneda Hydro-Electric Co., Warrior Ridge Gap, Huntingdon, Pa. This structure was built in 1905. The mix in the main wall was 1:3:6, and in the deck, crest and apron a 1:2:4 mix was used. The dam is of an Ambursen type. An inspection made on May 12, 1923, finds the dam in good condition with little or no indications of spalling or weathering, except in one or two places which was probably caused by the freezing of the concrete. An inspection of this company's dam No. 2 constructed at practically the same time finds it to be unaffected by weathering conditions.

CANAL LOCKS

The first use of portland cement in the construction of canal locks was in those built by the United States Government in the Illinois-Mississippi Canal. Locks Nos. 30 and 31, located at a point south of Rock Island, Ill., where the canal leaves Rock River, were constructed in 1894. The concrete for these two locks was mixed in the proportions of 1:2:4½. Some changes were made for footings. The concrete was placed in layers approximately 8 in. thick and rammed into place with heavy tampers. During construction, wells 12 in. square were built in all walls. After the concrete was placed, these wells were kept filled with water for two to three weeks or until the concrete refused to absorb further moisture. The holes were then filled with concrete and are traceable to-day in the structure. This system of mixing, placing and curing was consistent throughout the construction, not only of these locks but also locks

Nos. 28 and 32. An examination made of these locks on May 17, 1923, indicates them to be in excellent condition. They have been in constant use and exposed to severe weathering conditions for approximately thirty years. All vertical and horizontal surfaces indicate no disintegration, except the various laminations caused by the system used in placing of the concrete. **Mr. Hart**

RETAINING WALLS

The Fourth Avenue Retaining wall, Seattle, Wash., was constructed by the Great Northern and Northern Pacific Railroads in 1905. The entire wall of reinforced concrete is about 2000 ft. long and varies from 2 to 37 ft. above subgrade. The mix was approximately 1:2:4 laid with a dry mix and rammed into place. An examination of this structure on May 14, 1923, indicates it is in the best condition with no evidence of deterioration whatsoever.

STADIA

The Harvard Stadium built in 1903 and inspected on May 20, 1923, together with a report of the athletic director of Harvard University, indicates this structure to be in excellent condition. In a few places the concrete has spalled off caused primarily by reinforcing being too close to the surface in these places. The athletic director of Harvard University makes the statement that the total expenditure for maintenance on this structure has not exceeded $25. All colonnades and reinforced concrete work were made of a 1:2:4 mix. The seat slabs and risers were pre-cast in sand molds using a 1:4 mix. No coarse aggregate was used in these slabs. Compare with this the concrete used on other large stadia erected following this period. The concrete was made wet to facilitate placing and speed construction, thus sacrificing durability and strength.

BUILDINGS

An examination of approximately twenty-five reinforced concrete buildings ranging in dates from 1896 to 1907 indicates the principal disintegration is due to improper placing of the reinforcing steel in columns, beams and girders. Exposed surfaces on these buildings show little or no disintegration except where the reinforcing steel was placed too close to the surface and caused the concrete to pop off. In the earlier structures, a great deal of care was used in the quantity of mixing water. During this period if it was necessary to add additional water to concrete in order to develop the proper bond and strength between the concrete and the steel, the consistency

Mr. Hart was watched very carefully. Insufficient attention, however, was often given the spacing of the steel from the outside forms which naturally has given poor results. Reports on these buildings indicate the concrete to be in excellent condition.

Permit me to cite a few instances of some of the older structures.

Stanford University, Palo Alto, Calif.—This building was designed by Percy N. Hamilton and constructed by Ernest Ransome in 1896. The mixture is not known, but the construction indicates a very dry mixture was used. This structure withstood the San Francisco earthquake in 1906 and is in use at the present time. No deterioration due to weathering is noted.

Ferry Building, San Francisco, Calif.—The concrete sub-structure of the Ferry Building was placed in 1898 and consists of concrete arches and abutments with a very small amount of reinforcement. Part of the work is below sea level and part above. The mixture is not known. No disintegration is noticeable and it is in apparently as good condition as when placed.

The oldest concrete structure to which we have reference and recent report, is a concrete barn built at Croton on the Hudson. This barn was constructed in 1865. It is 60 by 75 ft., two stories in height. The walls are 18 in. thick. About thirty years ago the interior was completely burned out. This structure has not been in use since that time. The present condition of the structure, however, is exceptionally good. Little or no information is available on its construction other than it was built of a cement and field stone. Some idea of the age of the structure and the time it has been idle is indicated by the fact that four large trees have grown up in front of the entrance. Other buildings constructed at the same time have been remodeled and are in use as summer cottages.

CONCLUSIONS

The general thought to be conveyed by this survey is that the tendency at the present time is to use too much water for mixing and a lack of properly curing after the structure has been completed. Our examination of these structures indicates very conclusively that there is a great advantage to be obtained in the dry mix. Naturally under present labor costs it is impossible for us to expect to place concrete under the old conditions. We should, however, place our concrete as dry as possible and still obtain a firm grip or bond upon the reinforcing metal.

As an indication of what can be done by control of the water in mixing concrete, permit me to cite to you the conditions that have existed in the road building field since 1919. At that time the larger states were entering into very extensive road-building programs. The people's money was being spent in large sums for comparatively short mileages. The highway engineer was immediately placed in a peculiar position. He must draw specifications that would not work a hardship on the contractor, thus increasing his construction cost, and at the same time must be sufficiently rigid to produce a road that would withstand the class of traffic common to his particular community. The result was that practically all highway departments adopted a slump in concrete not to exceed 2 in. The fortunate position of the highway engineer was that as he developed in his knowledge of construction he also developed the construction forces. Therefore, the contractor in bidding on a given project assumed that the rigid inspection enforced on the highways would require him to use a very dry concrete. Machinery manufacturers immediately developed equipment to handle this class of concrete. **Mr. Hart**

After the concrete is placed in the road the contractor is required to keep it wet for at least ten days and not open it to traffic for 21 days. This gives the pavement an opportunity to thoroughly cure before being subjected to severe traffic. The result to-day is that the concrete road stands second to none. It is subjected to conditions equal to and greater than any other class of structure. It must withstand all of the stresses of any building with one additional stress added, that is, abrasion. I feel that these results have been obtained only by a careful observation of when and how to use water in construction.

The construction problem before the building contractor may differ to a great extent, still there are a number of conditions in common. It is unnecessary to use the large amount of mixing water seen on many projects. Many contractors to-day are following the lines of least resistance and use the "water transportation system" of placing concrete. Their argument is for a position of economy that must be taken into consideration on every contract. At present the engineering profession is not in position to take advantage of a reduction in cost of a structure by a possible reduction in section of the structural members through a careful inspection of the concrete. Furthermore, so long as contidions are permitted to go as they are, it will be impossible to ever take advantage of these reductions until drastic changes are made in construction practices.

All of this leads us to the one conclusion. Until such time as we are in position to enforce rigid inspection and a careful analysis of concrete materials, only then will we be in a position to say that

Mr. Hart

this concrete will withstand weathering conditions. When we have regulated this feature we will have overcome 85 to 90 per cent of our difficulty. It is as essential to control the mixing water on a building as it is to control the volume of any material entering into its construction. Mr. Abrams has rightfully said: "I would rather measure the water and guess at the cement than to guess at the water and measure the cement."

Mr. Greenman

Mr. R. S. Greenman[1] (*presented in written form*).—Since "concrete is its own best inspector," we must recognize in its rather slow reports the truths presented in such reports. Appearance, density, impermeability, strength, and permanence are all essential qualities, but the greatest of these is permanence. What, then, is the quality in concrete that is most conducive toward producing the desired permanence?

From extensive examinations it would seem to be a fact that the concrete that will best resist frost action is most likely to be the most permanent. And how can concrete be made that will resist frost action the best?

It is axiomatic that frost will not act in hardened concrete if water is kept out. Will density or impermeability keep out this water? One would expect that either of these characteristics would, but who has not seen some dense concrete that has disintegrated from frost action; or who has not seen some impermeable concrete with no evidences of permeating water that has "gone bad"? Why do copings so frequently disintegrate? They surely are usually exposed only to surface water and moisture. Why does one find frost action so very frequently at seams, or joints, or on corners of the structural sections and at no other place in the structure? Is it not a fact that general practice in placing concrete allows the poorest part of the concrete mixture to *flow* to these places? Is it not also a fact that in any "lift," or course, in placing the concrete the top part of such a lift is the poorest part because the top does not have the compacting that is given the bottom part by the weight of the upper portion? These parts may be dense in appearance but are they composed of the best in the mixtures? And, too, density in the placed mixture is not a guarantee that this concrete is non-absorbent, because frequently the materials in the concrete may be of an absorbing nature.

The writer does not believe that enough attention has been given to producing non-absorbent concrete. Three typical illustrations will explain some of the reasons for this belief.

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[1] Senior Assistant, Engineer in Charge of Tests, Department of State Engineer and Surveyor, Albany, N. Y.

Mr. Greenman

A concrete pier extending into fresh water is disintegrating from frost action without any doubt. Recent examinations of newly exposed surfaces show a dense concrete of well-proportioned materials but the gravel in this concrete contained a large amount of rather soft sandstone pebbles which readily absorb moisture.

A section of concrete paving in the spillway of a reservoir dam—laid in checkerboard fashion with blocks 10 ft. square—showed, about three years after laying, that some of these blocks were disintegrating. In this case the mortar content of the concrete was made of equal parts of limestone screenings and sand. Test specimens made of similar mortar absorbed over 13 per cent of moisture while mortar specimens made only with sand absorbed 7 per cent. Cores taken from this paving showed that this concrete certainly was dense, hard, and strong, but nevertheless it absorbed enough moisture to cause it to disintegrate from frost action.

A structure so constructed—unintentionally of course—that laitance seams are in evidence and through which seams water seeps, has a face that is disintegrating. It is clearly evident that the water seeping through is held against the surface by a film of calcium carbonate formed on this surface, the water is absorbed into the face by the aggregate, frost acts, and then there is the disintegration. The aggregate used was a limestone having natural seams of an argillaceous material which readily absorbs moisture and when the frost acts it breaks up this aggregate. The result is disintegrating concrete. In connection with this structure a gate-house and some other auxiliary structures, subject only to normal atmospheric moistures, are showing the same disintegrations because of this aggregate. This disintegration is not yet serious in its extent as to its effect on the stability of the structures, but it is disfiguring the appearance of these structures.

Non-absorbent concrete is only secured when proper materials are used; and only, too, when the placed concrete is homogeneous and no parts are made up of the over-wet "slush" composed of the finer and lighter particles of the mixture. For permanence in concrete, then, one of the essential characteristics is that it shall *not* be an absorbent concrete. But the fact is that too little attention has been given to this characteristic in making and studying concrete.

Mr. Paine

MR. J. J. PAINE[1] (*presented in written form*).—I should like to ask two questions:

First, what investigations have been made regarding concrete road and street-base failures?

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[1] Director, Bureau of Tests, City of Pittsburgh Laboratory.

Mr. Paine Second, have any peculiar phenomena been noted where these failures have occurred?

The City of Pittsburgh has had a number of base failures and especially with street bases laid since 1916. These concrete bases were made of a 1:3:6 mix and usually covered with asphalt binder and surface. It is true that in some cases we have been able to assign a definite cause for the failure, but in others, where the base was put down under favorable conditions, marked peculiarities have developed. For instance, a 6-in. concrete street base, 1:3:6 mix, was laid during warm and favorable weather, allowed to set for ten days and had every appearance of being excellent concrete before the asphalt top was put on. One year later when the asphalt was stripped off, the concrete was soft, had an offensive odor, and when exposed to the air soon became hard again and developed a white powder throughout.

I have thoroughly investigated this white powder and always found it to be calcium carbonate. Other investigators, with different samples taken outside of Pittsburgh, claim to have found considerable sulfates in their powders. Also, I have been able to produce this white powdered condition in mortar and concrete samples of a very lean mix and poorly graded aggregate. This is what would naturally be expected in a porous concrete.

The apparent set, softening and reset phenomenon, I have never been able to reproduce in the laboratory.

Mr. Atwood MR. W. G. ATWOOD[1] (*presented in written form*).—It is one of the functions of the engineer to select the proper material for use in the structures to be designed and built under his supervision, and it is one of the functions of the investigator to furnish him the data on which to base this selection. Standardization of materials is desirable from an economic standpoint, but it is not desirable to establish a single standard if in so doing it is necessary to give certain factors a relative importance not justified by the requirements of many of the structures in which the material is to be used and neglect other factors of greater importance to those structures. As has been so clearly pointed out in preceding papers, in establishing the standard cement specifications there seems to have been too much stress placed on the strength of concrete and too little on its stability under adverse conditions.

The character of the aggregates, the proportions of the ingredients, the amount of gaging water, and the methods of mixing and placing, all have an important effect on the strength and stability of concrete

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[1] Vice-President, Canadian Car and Foundry Co., Ltd., and Canadian Steel Foundries, Ltd., Montreal, P. Q., Canada.

Mr. Atwood

and if any of them be radically wrong, failure will result. If the best aggregates be used, the best practice followed, and the cementing material be not stable in the conditions under which the structure is to exist, the structure will not serve its purpose.

The greater part of the experimental work done in the development of the cement industry in the United States has been concentrated on the production of a material that would develop maximum strength and successfully withstand a limited series of stability tests. It does not seem to have been generally realized that in many structures stability in the face of attacks by sulfates is of much greater importance than great strength and therefore that, in addition to the production of the best portland cement possible, binding media are necessary to fill requirements which standard portland cement does not satisfactorily meet.

This need has been recognized in other countries for a century or more and over one hundred years ago Vicat, the great French scientist, established his "Hydraulic Index" which has not been superseded by the advance in science since that time. Portland cement which meets the requirements of the A.S.T.M. specifications does not meet the requirements of Vicat's "Hydraulic Index" for use in sea water and practically no cementing materials other than lime and portland cement are available in the markets of the United States except by importation.

The work of Vicat has been expanded and checked, and many of the reasons for his conclusions which could not be made clear with the chemical and microscopic methods of his day have since been developed by Candlot, Michaelis, le Chatelier, Hiroi and others abroad. The work of G. A. Rankin and his associates at the Geophysical Laboratory of the Carnegie Institution of Washington, probably gives the most recent, complete and accurate information on this subject. In his paper on "The Setting of Cements and Plasters" before the Faraday Society, January, 1919, Rankin says:

> "Of the three compounds which thus take part in the setting and hardening of portland cement, the tricalcic silicate appears the best cementing constituent; that is, this compound is the only one of the three which when mixed with water will set and harden within a reasonable time to form a mass which in hardness and strength is comparable to portland cement. The compound dicalcic silicate required too long a time to set and harden in order to be in itself a valuable cementing material. The compound tricalcic aluminate, while it sets and hardens rapidly, is rather soluble in water and is not particularly durable or strong.
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> "From this it would appear that the compound tricalcic silicate is the essential constituent of portland cement. In other words, it is tricalcic silicate which imparts to portland cement its valuable cementing properties.

Mr. Atwood

"In this connection it is interesting to compare the nature of the setting and hardening of tricalcic silicate with an ideal cement which M. Vicat worked out theoretically about one hundred years ago. M. Vicat, who carried on a very interesting series of experiments in regard to the hardening of cements and plasters, seemed to believe that the lime in cement mortar should be in a state of chemical combination and that it were best that it should be so combined with gelatinous silica.

"The similarity between Vicat's theoretical cement and tricalcic silicate is thus apparent."

The tricalcic silicate is not attacked by the sulfates which in both sea water and water in alkali soils are responsible for the deterioration of the cement and therefore a satisfactory cement for use in such waters must have a composition such that its entire lime content will be combined with silica, alumina or some other satisfactory substance, to form compounds insoluble in sulfate-bearing water. There is nothing in the standard cement specifications which provides for this requirement.

The use of petrographic microscopic analyses in conjunction with chemical analyses is responsible for much of the increased knowledge of this problem, and it is doubtful whether a satisfactory material can be produced and identified without the use of the microscope.

As an example of the realization abroad of the necessity for different binding media, attention is called to the experiments conducted by Dr. B. Jeanneret, Engineer-Chemist of Basle, Switzerland. These experiments were made with portland cement and with "electro ciment," one of the names for the high-alumina cement produced in France and Switzerland by the fusion of beauxite and lime. Mortar specimens were made of equal consistency from the two cements and subjected to exposure in concentrated selenious waters, alcoholic liquids and mineral oils. In sulfate solution at the end of four months the specimen made of portland cement was practically destroyed, whereas that made with "electro ciment" was not perceptibly altered. In the alcoholic solution the portland cement appeared only slightly inferior to "electro ciment," while in mineral oil the portland cement was considerably superior. These experiments, as well as many others, demonstrate clearly the necessity for different binding media for use under exposure to the various elements which cause disintegration. In the United States this need has been lost sight of in the use of portland cement for all purposes.

Cement which will withstand the effect of sea-water and alkali has been obtained in Europe and Japan by the addition to portland cement of a siliceous material generally volcanic in origin or by the manufacture of a high-alumina cement with a relatively low silica

Mr. Atwood

and lime content. It is practically certain that similar satisfactory materials are available in this country, but experiment is needed to show the exact qualities required in the silica and the proper proportions for the mixture. On account of the probable differences in the materials existing in various parts of the country, these proportions will differ and the experiments should develop a standard specification for both a high-silica and a high-alumina cement which, in addition to the necessary qualities for any good cement, will also have the quality of resisting the disintegrating effect of selenious waters.

The Committee on Marine Piling Investigations has had very exceptional opportunities for the collection of data relating to the life of structures in salt water. Reports of special inspections have been submitted by the Government departments and many other property owners. In most cases, the history of construction which would be most valuable in drawing conclusions is unobtainable, and therefore conclusions must be deduced from a combination of the information contained in many reports under many conditions, but it is felt that on account of the great number of reports substantial accuracy has been obtained. This committee is endeavoring to place before the engineers, who are designing and building structures subject to attack by sulfate-bearing waters, either sea water or alkali, the facts developed by previous experimentation, many of which are not as generally known as they should be. It is also endeavoring to encourage and coordinate experimentation along the lines which past experience and study have shown to be of promise for the development of binding media that can be depended on to resist the attack of sea water and alkali waters.

Mr. Butterfield

MR. E. E. BUTTERFIELD[1] (*presented in written form*).—Examples of the disintegration of concrete in certain structures exposed to sea water and alkaline conditions have been described. The nature of the chemical and physical changes is not definitely known. The remedy is empirical and along preventive lines: (*a*) Use of the best materials, design and quality of concrete, including workmanship; (*b*) Use of external protective measures.

A reinforced concrete sewer outlet into salt water, Flushing Bay, shows extensive disintegration. This outlet was built of 1:2:4 concrete under our old specifications for reinforced concrete sewers. Another sewer outlet a short distance therefrom, built under special specifications, $1:1\frac{1}{2}:2\frac{1}{2}$ mix, 3500 lb. per sq. in. compressive strength at 28 days, water ratio 0.64, and special workmanship to produce a smooth surface, shows not the slightest sign of deterioration after four years.

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[1] Chief Chemist, Office of President, Borough of Queens, 68 Hunters Point Ave., Long Island City, N. Y.

Mr. Butterfield

In the Jamaica Bay Boulevard, a reinforced concrete structure 4½ miles long of which 8500 ft. traverses salt water, we are using 1:1:2 concrete piles and have furthermore completely sheathed the piles with creosoted wood strips for a distance of 3 ft. above and 2 ft. below mean water level. The concrete itself has a compressive strength of from 4200 to 6000 lb. per sq. in. at 28 days. Special requirements are made as to materials, water ratio, workmanship and the production of a smooth-finished surface. Sections sawed from test cylinders show the concrete to be extremely dense; the surface resembles onyx. I believe this represents as fine concrete as has ever been designed to withstand the action of sea water. The density is 0.875. But even this small percentage of air and water voids is a source of potential danger. In view of the extreme difficulty or impossibility of further reducing the air and water voids in finished concrete, I believe it is time to turn our attention to protective coatings for the surface of concrete exposed to sea water, unusual atmospheric conditions, or alkaline conditions. I have experiments under way with bituminous compounds applied to the surface of concrete cylinders.

The use of the best concrete obtainable under our present knowledge of materials and design, followed, where possible, by protective surface application of suitable material which will prevent the access of water, gases, or chemicals to the air and water voids in the concrete, will, I believe, greatly reduce if not entirely eliminate the instances of the injurious action of sea water, alkali and other agencies upon concrete.

Mr. Mattimore

MR. H. S. MATTIMORE[1]. (*presented in written form*).—My interpretation of the object of this Symposium is to get the experience of engineers who have had the advantage of studying concrete and who have possibly ascertained, to some extent, what special practice of this product requires investigation to avoid failures. We cannot hope to attain short-cut, brief rules for the proper proportioning, mixing and placing of concrete. In the first place the making of concrete is an engineering operation and one that requires study. Concrete is far from being a fool-proof material. Its very broad use in various structures has, perhaps, made us lose sight of this to some extent, and the proportioning, mixing and placing of some concrete structures have been under the supervision of people unskilled in its art or who have attempted to get all their education on the use of this material by reading brief treatises or so-called primers on concrete engineering. Rarely or never does an uneducated or unskilled con-

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[1] Engineer of Tests, Pennsylvania State Highway Commission.

Mr. Mattimore

structor attempt to supervise the erection of a steel bridge or building, but unfortunately in later years, concrete is a product which was considered so universally safe and permanent that on many projects every man was his own architect, engineer or constructor.

This condition of affairs has been brought forcibly to my attention in investigating concrete structures which had partially failed. I have encountered the effect of so much carelessness in the proportioning and placing of concrete that I often wonder that it is used with the success that it is.

It seems to me that we have been and possibly are moving in cycles in concrete engineering. For some years we experienced concrete failures which we attributed to carelessness; this resulted in the study of proportioning and placing of this material, and precautionary measures were taken to try and have proper supervision over concrete construction. Later, during our large construction programs, we took costs into consideration and attempted to reduce prices on concrete construction; first, by broadening the conception of what were proper aggregates to use, and next by publishing brief instructions for the proper proportioning, placing, etc., of concrete structures. We do not have to look far to see some results of this.

When we make an investigation for the reason of so-called failures in concrete, it is difficult and sometimes practically impossible to obtain information regarding the operation of laying this material, its proportioning, mixing, etc. The obvious defects are readily observed, but in the difficult cases it may be practically impossible to tell how much these conditions have been a factor in the so-called failure.

A very conspicuous example of this phase of the question is the so-called failure of the Nashville Concrete Bridge. An account of this failure was first published in the *Engineering News-Record* of December 8, 1921, page 934. There is some discussion on the reasons for this failure but no conclusive evidence is given. This project was discussed pro and con until finally the *Engineering News-Record* published an article in their issue of June 21, 1923, page 1090, which is the result of the investigation made by Freeland-Roberts Co. I have no intention of discussing the merits of this investigation, but it certainly is interesting to analyze their nine main reasons for failure. If the failure is due to these conditions it is an illustration of a flagrant violation of the simpler laws in concrete construction.

1. Disintegration of both fine and coarse aggregates under the action of water due to improper materials and limestone containing shale, rotten stone, etc.

Mr. Mattimore

2. Separation of parts of concrete mix when dropped into column forms.
3. Steel too close to surface of concrete.
4. Porous concrete which allows water to be absorbed, thus rusting the steel reinforcing and causing the concrete to be broken up when this water freezes.
5. The use of limestone dust which balls up in mixing and coats the coarse aggregate so that the mortar will not stick to these particles.
6. There also appears to have been a movement of the structure down the hill causing breaks in the tie beams between columns.
7. Construction joints and temperature cracks allowing the water to run through the slabs and onto the beams underneath.
8. Finishing and surface of handrails thus taking off the waterproof coating of mortar.
9. Laitance.

One point I wish to bring out is how much of our construction at the present day is being handled as carelessly as the project quoted; for instance, the use of limestone dust. Some of our best specifications are recommending limestone screenings as a fine aggregate in concrete construction. Based on past experience, I would recommend caution in the use of this product. Further, I would advise proper control of all aggregates being used in concrete. I know we are in the habit of testing aggregates for concrete, but it is a question whether we proceed far enough in the control of the use of these aggregates.

I realize that careless conditions and the use of poor aggregates do not account for all so-called failures in concrete. I have observed instances of apparent disintegration of concrete surfaces which to date I am unable to explain. One case I have especially in mind is a concrete road surface, which from five to six years after construction, started to show a scaled top. This at first was attributed to improper finishing which leads to a thin scale on the top surface, but in this case the scaling was a forerunner of a progressive disintegration, occurring in layers, to a total depth of from 3 to 4 in. In this case complete data were obtained on the aggregates which were all tested and accepted, together with the records showing the results of tests on cast specimens of concrete which were made during construction. These showed a compressive strength of from 2800 to 4000 lb. per sq. in., which we usually estimate as concrete of at least a durable nature. In order to investigate this condition thoroughly, inspection trips were made over highways built at that same period, with the same aggregates, including the same brands of cement, and many of these road surfaces were in excellent condition. The questionable highways were built in regions where sulfates, alkali and other materials of deleterious nature do not exist in soils or the water.

Mr. Mattimore

I believe an investigation of this kind is a worthy one, and one that is quite vital to the successful use of concrete in highway surfacing. My investigation to date on this question has not led to any definite conclusions. Fortunately, this defect has only been observed in a very small amount of the highways constructed about that same age or later, but it is always a vital problem until we know the reason, as unknown faults are apt to be repeated.

Mr. Bates discusses highway concrete and calls to our attention, instances where trouble has been experienced by the road slab cracking before the final set has been obtained, and he definitely states that the trouble had not been due to excess water or poor choice of aggregates, but that the difficulty has been with the cement. This was brought out especially from the standpoint of showing how much we are at fault in our testing to-day by storing test specimens close to saturated humidity conditions before and during the time of set. A study has been made of this condition on highway work because in certain sections an extensive amount of surface checking was encountered. Sometimes this was continuous and gave the appearance of a crack, but in other cases it was so slight that it was termed "hair checks." I agree with him that the cause was a lack of humid atmosphere, but our experience has taught us that it can be remedied and in most instances we have success in remedying it by applying wet burlap to the road surface soon after laying; in other cases it has been necessary to keep the fine and coarse aggregates well moistened at the proportioning plant; with a combination of these conditions and proper wetting of subgrade, we have succeeded in remedying all but a very few cases. I cannot agree with his conclusion that the difficulty has been with the cement; at least we found that we could continue using apparently the same cement, so far as the standard test determined, and remedy the condition.

It may not be amiss to bring out at this point that road concrete is probably laid under the most severe conditions of any structure. It is not unusual to have an exceedingly dry subgrade, water piped for long distances, obtaining a temperature of 100° F., and then having fine and coarse aggregates which are so dry that they will draw the essential water from the concrete as a sponge. We based our theory on this and actually duplicated, under laboratory conditions, as illustrated in the following experiment, defects which had occurred in practice.

EXPERIMENTAL ROAD SLABS TO DETERMINE CAUSE OF HAIR CRACKING

Materials Used.—

Cement, blend of several brands.
Sand, blend of artificial sands.
Stone, sandstone.

Mr. Mattimore *Mix.*—

Specimens A, B, C, and D—1 : 2 : 3.

Specimens E and F—1 : 3 : 6.

Preparation and Curing of Slabs.—

Section A.—A blast of hot air (120° F.) was kept over the surface of the road seven hours each day for five days. Then the surface of the slab was kept wet for one day with the hot blast continued. After this the slab was sprinkled once each day for five days with the blast still at a temperature of 120° F.

Remarks: Surface cracks and checking observed about six hours after slab was finished. The surface was covered with a fine dust or powder (laitance) which rubbed off easily with the finger. Surface scaled to the bottom of the cracks.

Section B.—Slab dried at room temperature (72° F.) for six hours. Then it was covered with a damp cloth for four days and finally damp sand for an additional seven days.

Remarks: Some surface cracks showed the second day and seemed to be a little deeper than those in Section A. Surface was much harder.

Section C.—The slab was poured on a bed of damp sand arranged so that water could be added to the sand through tubes leading into it. Slab was cured under a blast of hot air for five days (seven hours each day) and on the sixth day for three hours.

Section D.—Poured on damp sand similar to Section C but cured under a damp cloth just as soon as the concrete had set. At the end of twenty-four hours, the slab was covered with moist sand for seven days.

Section E (Mix 1 : 3 : 6).—Made and cured similar to Section C.

Section F (Mix 1 : 3 : 6).—Made and cured similar to Section D.

Remarks on Sections C, D, E and F: These sections showed no cracking or checking. Some air holes developed in all sections. Sections E and F were more porous than C and D.

Mr. Bates, in several parts of his paper, mentions the fact, in one case, that cement is fifty per cent soluble in water and another case where hydrated cement is just as soluble as limestone, in fact more soluble. This is not only a new fact to me but also an interesting one, in that I believe that most engineers consider that the calcium silicate of cement is not as soluble in normal water as the calcium carbonate of limestone.

If I have succeeded in accomplishing nothing else, I would feel perfectly satisfied if I could develop caution in our concrete engineering in the use of the proper aggregates, and bring about proper interpretation of test values on aggregates and a realization that high compression and tension either in mortar or concrete does not guarantee durability in a structure. We must proceed further and test the durability of our aggregates, and at the same time make a further study of cement as recommended by Mr. Bates; but do not let us deceive ourselves in believing that we have solved the question of selecting concrete aggregates.

Mr. Pearson

MR. J. C. PEARSON[1] (*presented in written form*).—In the discussion of this subject I assume that the word "concrete" is to be taken in its broadest sense, including cement mortars and plasters. I wish particularly to refer to cement stucco, because in this material we have one of the most striking and common examples of a product that is rarely 100 per cent perfect owing to an intrinsic and very troublesome property of the cement, namely, expansion and contraction under changing moisture and temperature conditions. It may be that this property is inherent in portland cement, and that without it we would not have a portland cement with its usual strength, hardness, and wear resisting qualities. On the other hand, if portland cement could be so modified that its volume change could be reduced to a degree, it would be a much more satisfactory material for many purposes.

In cement stucco we have a product that suffers greatly from expansion and contraction, and there is not enough knowledge on the part of the very great majority of those who apply it to enable them to *guarantee* a satisfactory result. Just what we mean by a satisfactory result is not very definite, but in general the owner of a stucco house should be satisfied if there are no very conspicuous cracks or unsightly defects which develop in the first round of seasons after application. But cracks are always present to some degree, and they are in general the evidence of that undesirable property which may produce much more serious results if the plasterer is not fully aware of the precautions that must be taken.

As most cracks in stucco are due to shrinkage of the cement, the improvement of stucco must be brought about in two ways, either by improving the cement, or by improving the structure of the stucco itself. So far as I am aware, there is no immediate prospect of changing the nature of the cement, but it might be well worth while to find out whether and to what degree different cements differ in their tendency to shrink or expand under given conditions, and then to discover the causes of these differences. This problem is properly a matter of concern to this Society because it is a problem of a material, but on the other hand we know much more positively that the structure of the stucco, involving not only a properly compounded mixture but especially skill and knowledge on the part of those who are to apply it successfully, is the really determining factor in eliminating or minimizing the faults due to expansion and contraction of the cement. Problems of this nature, it seems to me, belong rather to such an organization as the American Concrete Institute because they are problems

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[1] Physicist, U. S. Bureau of Standards, Washington, D. C.

Mr. Pearson of handling and using materials, and the thought occurs that if we are considering concrete as it exists in all sorts of structures, such faults as have been observed are more likely to be due to inadequate workmanship, or to a lack of knowledge of how to use the materials available in such way as to meet the requirements, than to the materials themselves.

If the foregoing statement is correct, it seems to me that an investigation into the causes of defective concrete should be conducted jointly by this Society and the Concrete Institute, with the burden of the work falling upon the latter organization. The diagnosis of failures belongs as much to the testing engineer and the scientist as to any one, but the avoidance of failures is largely up to the designers and builders.

Mr. Kinney MR. WILLIAM M. KINNEY[1] (*presented in written form*).—The one definite conclusion that may be drawn from this discussion is that there is unlimited field for study in all branches of the application of concrete for structural purposes. Betterment will surely come as our study progresses.

As we learn more of the constitution of portland cement and the effect that variations in its chemical composition and method of manufacture have on its ultimate usefulness, undoubtedly improvements in quality will follow. Further study of the general subject of aggregates, and particularly field study of available sources of supply, will most assuredly lead to improved use and better results. With a better knowledge of aggregates available, proportions may be determined in accordance with scientific principles instead of arbitrarily.

Continued study will without doubt confirm the importance of proper control of water content, so that if we are to reach the goal toward which this Symposium directs us, instead of the customary 1:2:4 proportion, we will find specifications calling, for instance, for 1:1.9:3.8:6 proportions, the last figure being gallons of water required assuming normal condition of aggregates as to water content and grading. Naturally variation in aggregates and water content of aggregates must be allowed for in any such scheme of proportioning.

Some might well contend that the refinement suggested by the above proportions is not justified in the great mass of concrete work. Their contention would be somewhat justified, for despite the reference that has been made to-night to concrete which has proved unsatisfactory, it must be admitted that in most uses it is performing, at least structurally, the service for which it was intended. I say "at least structurally," for we must admit that there can be most marked

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[1] General Manager, Portland Cement Association, 111 W. Washington St., Chicago, Ill.

Mr. Kinney

improvement in the appearance of the average concrete job. In fact, it is not uncommon to class as concrete failures structures which are sound and perfectly capable of performing their structural functions, but which from the appearance standpoint are far from satisfactory.

Mr. Bates' paper may leave with others as it did with me the impression (possibly because he was assigned the subject of "Cement" and therefore discussed that subject alone) that much of our trouble will be overcome by developing for each particular major use a kind or quality of cement best suited for that purpose. Considering the long years of effort expended by representatives of this Society and others to develop and perfect a single standard specification for portland cement, and further efforts that have been made to get manufacturers to make cement to these specifications, and architects and engineers to adopt them, this suggestion is somewhat revolutionary. This would tend to bring back the chaos of former years just as we have reached the goal which we have been seeking, which obviously is not in Mr. Bates' mind to recommend.

Before we take such a step let us analyze some of the practical considerations. In the first place, we have some ninety odd cement manufacturing companies in the United States, with about 115 plants located at various points throughout the country convenient to the markets which they serve. Where demand is heavy, plants are larger and more numerous than where demand is light. These plants have been designed to make most readily, and therefore with lowest cost, cement of a certain fixed standard. Attempts to make special cement at various times have usually proved to be expensive and unsatisfactory. If in a practical solution of the proposed plan, certain plants should devote themselves to the production of one quality and others to other qualities, we would have the resultant necessity of shipping a heavy commodity long distances, and consequently at almost prohibitive freight rates, for freight now represents a very substantial item in delivered cement prices. Plants producing the particular quality of cement desired would in many cases be at prohibitive distances (freight-wise) from the proposed work. Such expense is only justified if present standard cement will not do the work. This has never been shown. As evidence to the contrary, I point to numerous successful concrete structures in practically every field of use for which concrete has been adopted.

Practical and economic considerations seem to urge that within the limits that raw materials available in various sections of the country will permit, portland cement should be a uniform standard

Mr. Kinney article. It seems desirable to have one constant when there are so many variables.

It is human nature to blame someone else for any trouble we may have. It is easy to blame the cement for concrete troubles. Where the principal omission in workmanship is not obvious, perhaps it is natural to do this. When greater respect exists for the need of care in manufacturing concrete, we will get better concrete. In but few cases is it realized that this is a manufacturing operation and does require care.

Let research work on cement and on concrete proceed. It is essential, but do not wait for it, nor rely upon it for better concrete. We already know many things in concrete making which should be done and are not done. Mr. Wight is right in urging that we study concrete structures to get the lessons they have to give. Take care of the "immediate" through greater care in use of standard cement, and the "ultimate" will follow through development of special cement if, and when, such special cement has an economic value.

One branch of the art of concrete construction has received possibly less attention than any other and has quite naturally, as a consequence, been omitted from the outline for this discussion. I refer to the placing of the concrete in the forms. Particularly is this important wherever the concrete is to be subjected to unusual conditions, such as sea water, alkali, freezing and thawing, etc. Much effort expended in testing of cement, selection of aggregates and care in proportioning and mixing is lost because of lack of proper attention to the actual fabrication of the structures. Inexperienced labor, indifferently directed, is the rule rather than the exception on many jobs. "Water transportation" of the concrete saves labor but wastes cement and brings indifferent results. Careless placing and excess water produces poor joints, construction seams, porous surfaces and laitance. A job so constructed is almost sure to look like a failure in five years even if it is still capable of serving its structural requirements. Here, indeed, is a field for extensive research and much improvement.

Mr. Bertin MR. R. L. BERTIN[1] (*presented in written form*).—Concrete is perhaps the most universally used material for construction work, either singly or in combination with steel, and because of its adaptability to the solution of so many diversified structural problems, it is called upon to perform under all sorts of conditions.

Unfortunately, once its use has been decided upon, its abuse in many cases begins, either through lack of knowledge on the part of those who have to do with its making or through slighting for false economical reasons; the general belief that any mixture of cement, water and plenty of whatever happens to be available which will answer to the name of aggregate produces a material proof for all time against attacks of whatsoever nature is still too prevalent. **Mr. Bertin**

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[1] Chief Engineer, White Construction Co., Inc., New York City.

Aside from the failures of concrete which are due to its exposure to conditions which cement is inherently incapable of withstanding, failures are in most cases traceable to carelessness or ignorance. Concrete painted in its darkest shade may be described as follows:

Untested cement is mixed with a poorly graded aggregate, inaccurately measured in accordance with some arbitrary proportion, drowned with water, insufficiently mixed, segregated by chuting, deposited in leaky forms, the mortar in one place, the stone in another, with the accumulation of laitance left in the structure, and then allowed to bake in summer or freeze in winter.

All concrete is of course not subjected to all the abuses just mentioned but there are very few jobs where one or more of them are not present. It is indeed nothing short of marvelous that as good results obtain under the unscientific manner in which concrete is made. From all sides is heard the general conclusion that dense concrete is essential to permanency or at least long life, yet how little is done in actual practice to produce it. If the fact that economy goes hand in hand with density was given more publicity it is probable that better concrete would result. The old stereotype specification calling for 1:2:4 or 1:1½:3 concrete, while gradually disappearing, is still too prevalent. The old methods of measuring aggregates, which are known to yield such variable results, are still in general use and only in few cases are any attempts made to correct for the variations. The attempts made to regulate the amount of water in concrete fall short of their aim in most instances.

The available data relative to the properties of concrete are not finding their way into the hands of the users or else the users are neither keen nor interested to broaden their knowledge, with the result that the advancement of the art remains for the most part academic. It behooves the engineer and contractor who specify or manufacture concrete to acquaint themselves with the investigations that have been made and to devise and use new methods which will at least reduce the variations concrete is heir to as it is handled to-day.

Good concrete may be produced with any kind of structurally sound, clean materials, provided the characteristics of the aggregate are given some consideration in the determination of the proportions. The general idea that a clean, sound and hard aggregate is good or bad concrete-making material because of its grading or sharpness or

Mr. Bertin shape of particles is erroneous; it matters little whether the particles are flat or round, what the sieve analysis of the aggregate is, whether it is all stone or all sand, so long as the material is clean, sound and hard; concrete of equal strength can be made with the so-called good or bad aggregate if enough cement is used.

The present specifications for concrete prescribe at length definite proportions, grading of aggregate, amount of cement and water, and most of these requirements are disregarded because they are contradictory and cannot be fulfilled in practice. How much simpler it would be to state that to a sound, clean, hard aggregate, the largest particle of which shall not exceed a maximum size, depending upon the nature of the work, shall be added sufficient cement and water, in a ratio not to exceed a maximum value depending on the strength required, to produce a homogeneous mass sufficiently plastic for practical depositing in its particular place. Concrete made in accordance with this specification would come much nearer to being perfect than that produced under the variety of present specifications. The concrete manufacturer would soon learn to use a consistency of concrete and an aggregate of such grading as would produce the most economical concrete; in many cases more economical than that produced by present empirical mixtures and more efficient from the standpoint of density and strength.

What is needed perhaps more than anything else is the broadcasting of (1) the limitations of concrete made with the available commercial materials, and (2) simple understandable directions for specifying and producing concrete of the required quality for the least cost.

Mr. Withey MR. M. O. WITHEY[1] (*presented in written form*).—Although in hearty sympathy with the ideas expressed by Mr. P. H. Bates in his discussion concerning needed investigations on methods of making cement, I feel that we should also set ourselves to the task of determining the durability of concrete when exposed to the weather found in various parts of our country. In this connection, attention should be directed to the standardization of a test, or a series of tests, by which the durability of a given concrete or mortar can be quickly and accurately predicted. With a view to securing some information on this subject, we are now starting at the University of Wisconsin, a series of long-time tests on the strength of concrete, subjected to weathering and to dry curing conditions. In this series of tests, it is planned to use several brands of cement, two or more coarse aggregates, a medium and a very wet consistency, three sets of proportions, and to extend the breaking period over a very long period of time—25 years or more. It is also planned to make auxiliary tests on mortars and concretes aged for a month or more, which it is hoped will furnish criteria of value in interpreting the results of the subsequent strength tests. If such relationships are established, then these auxiliary tests can be used as yard sticks for estimating the durability of concrete. **Mr. Withey**

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[1] Professor of Mechanics, University of Wisconsin, Madison, Wis.

Mr. Collier MR. B. C. COLLIER.[1]—I desire to take the position of defending cement. It is a condition and not a theory that confronts us, and while my experience—I have, in carrying out my duties in the last few years, been called upon to visit practically every one of the structures shown on the screen to-night—has shown me conclusively that the disintegration of concrete, especially under salt water conditions, has been rapid, I am firmly convinced that when a condition of perfect density is obtained positive permanence is assured.

One of the speakers has indicated that preservation can be obtained by the use of a timber protection. As a contradiction to this, I desire to cite the case of one of the most prominent structures on the continent where a timber encasing protection was used, but where the concrete is failing rapidly.

It is to be admitted that much has been accomplished in bettering conditions by the extensive campaign that has been carried on against too much water and for proper mixing methods. On the other hand, I feel that too little attention is being paid to securing proper aggregates. Too many engineers and laymen have been led to believe that in order to secure a permanent structure any stone or sand could be used with the proper amount of cement and mixing. Mr. Mattimore has told us to-night of the great stress that is being laid on this subject by his department of the Pennsylvania State Highway Commission, and that many materials that were formerly used are now being rejected. On the other hand, the average builder or contractor for other structures that may be even more important than a concrete road is continuing to use this same material, perhaps in the full belief that his work will be perfect.

I have in mind a number of structures where every evidence tends to definitely prove this. A most notable instance, and one that is perhaps as widely referred to as any, is that of the two large bridges at Nashville, Tenn., where without question the rapid destruction of the concrete is due to faulty stone. Similarly I can point to several structures in Eastern Pennsylvania where a similar condition exists. The general appearence of such structures, especially where shaly limestone is used, seems to indicate that slaking has taken place

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[1] President, Cement-Gun Company, Inc., Allentown, Pa.

Mr. Collier inside the structure, causing the formation of material closely resembling slaked lime, and the consequent "blowing" or splitting open of the concrete along closely connected and irregular lines. I, therefore, feel that one of the greatest advantages that the Portland Cement Association, with its enormous facilities for good propaganda work; can confer will be to start immediately a campaign seriously attacking the use of poor aggregates, as it has in the past attacked the use of too much water and improper mixing.

Another very important feature is that of improper design, especially when structures are subjected to sea water or locomotive blasts and smoke. I feel that it can be said without much chance of refutation that it is almost impossible to get good concrete below the plane of the reinforcing rods in girders or slabs, and still engineers will continue to place the integrity of their entire structure on that portion of the work. It has been pointed out clearly that the rusting of the steel causes expansion and the development of cracks which immediately allow increased entrance of the destructive agents. As an illustration of these statements I would refer to an examination I was called on to make only this afternoon of certain structural parts of the board-walk and adjacent structures, which, even though never in direct contact with the sea water, are being rapidly destroyed. This attack is in most cases below the plane of the steel. If the concrete had been sufficiently dense it is my opinion that these girders would never have been attacked.

I would also cite a case of a concrete bridge nearly a mile long that I was asked to inspect last fall. Although this bridge had been completed less than two years, it was in such bad condition that the concrete had fallen off the bottom of about one-fourth of the girders, and large areas of the slab. Why? Simply because the steel was so close together that no good concrete could get past it. Do not think this is a solitary case. I have seen numbers of them.

Therefore, gentlemen, please let us bear in mind that while cement may need improvement, a much more immediate and practical solution can be obtained by starting an active campaign not only for proper mixing and proper water content, but more essentially for proper aggregates and a proper design and method to insure good concrete at the weakest point, that is, below and outside the plane of the steel.

Mr. Wig MR. R. J. WIG.[1]—We have had a very full evening, revealing the family skeleton, and I want to make only one comment. A wrong impression has been left regarding the behavior of cement concrete

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[1] Vice-President, Celite Products Co., Los Angeles, Calif.

Mr. Wig

in sea water. There is not a single structure that I know of anywhere in the world that was constructed according to ordinary approved methods, and we have been in touch with several hundred, that shows any indication of chemical disintegration below low water mark. There are structures on the North American Continent, in Nova Scotia, that were built in 1888; there are some twelve or fifteen of them, built by the Provincial Engineers. These are subjected to severe tidal action; on the Bay of Fundy, and on some of the structures you can still see the form marks. They are 35 years old, and will last many more years. Reinforced concrete in sea water is a problem of design. There are no reinforced concrete structures in sea water that I know of that show any disintegration below low water, except where they have been subjected to severe abrasion.

There is one error in a paper presented this evening in regard to the starting of disintegration in reinforced concrete in sea water. The corrosion starts at a point above high water, not below high water, —usually some inches, or a foot or more, above high water,—and then it progresses in both directions vertically. It may crack after the corrosion starts, to a point below low water. In some of the early observations which I made some eight or ten years ago, we drew the conclusion—and it was true at that time—that practically all reinforced concrete that we observed showed disintegration if it was more than three or four years old, with the exception of concrete in San Francisco Harbor, and one or two structures in New York Harbor. There are now some reinforced concrete piles in San Francisco Harbor that should be twelve or fifteen years old, and these do not show any cracking due to the corrosion of the embedded reinforcement. Apparently the condition in that harbor is not quite as severe as it is in some other locations. But we do have satisfactory structures in sea water, and I think instead of starting an investigation in the laboratory, it should be started in the field and we should learn how those structures that have proved satisfactory were made and duplicate them. It can be done, because we have the structures.

The Chairman

THE CHAIRMAN (PAST-PRESIDENT A. N. TALBOT).[1]—The discussion has been a most interesting and important one. As was stated by Mr. Wight in the beginning, this is a time for taking stock. There have been great changes, perhaps not always developments in the proper sense of the term, in the making of concrete in the last ten, twenty, thirty, forty years, as I can remember; and there is still quite a question in the minds of many whether some of the processes, some of the materials, some of the present methods are the best. It seems

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[1] In charge of Theoretical and Applied Mechanics, University of Illinois.

The Chairman

to me that the slogan proposed by Mr. Bates, "Concrete for Study," is still a good one, and that we shall find out in the next ten, twenty, or thirty years who has been right in some of the diverse suggestions and discussions given to-night. On behalf of the Society, I wish to thank the many who have taken part in this symposium and those who have prepared discussions.

Mr. Kitts

MR. J. A. KITTS[1] (*by letter*).—An important property of an aggregate, indicating its quality, is its ability, in proportion to its cost, to produce density, flexural strength, compressive strength, impermeability or other desired physical values in concrete. This suggests economics of aggregate production.

Do we make economical separations and re-combinations of concrete aggregates? It is my observation that much river gravel is separated into fine and coarse aggregates and re-combined without benefit to the concrete and at added cost for screening, for handling two materials instead of one and for a portion of the material wasted. Maximum size of gravel aggregate is often limited unnecessarily and with detriment to the resulting concrete. A similar condition prevails in the production of crushed rock. In determining the quality and combination of aggregates we have been concerned almost wholly with the question of what qualities and proportions give the physical values desired in the concrete, rather than by the question of what qualities and proportions give the maximum physical values in proportion to cost. In the mechanical processes of production of aggregates we have been satisfied with producing certain sizes with little consideration of the determination of processes of production most economically beneficial to the resulting concrete.

The study of methods of making concrete should comprehend methods of production of aggregates with consideration to the cost of the physical units obtained.

Mr. Witt

MR. J. C. WITT[2] (*by letter*).—In his introduction to this Symposium, Mr. Wight contrasts the complex nature of concrete with the simplicity of steel, wood, and glass, stating that "concrete . . . is an obscure compound, the chemical nature of which is debatable, and whose reactions are neither understood nor measurable." It is true that concrete is a complex material. Cement itself is a mixture of complex chemical compounds and when it is combined with water and aggregates, a complex mixture naturally results. Our ignorance of cement and concrete, while admittedly great, is perhaps slightly exaggerated by Mr. Wight. Some of the reactions are both understood and measurable.

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[1] Concrete Technologist, Pacific Portland Cement Co., Gerlach, Nev.

[2] Chemical Engineer, Chicago, Ill.

Mr. Witt

Concrete differs from some building materials in that it is made in place, but it has no exclusive claims for complexity. Steel, wood, and glass are also complex materials. The rusting of steel and the decaying of wood are not slow processes unless protection of some sort is afforded, and these processes are not nearly so simple chemically as they may seem. The reputation of Venetian glass for beauty and constancy is well known, but glass in general is not always unchangeable from the time it cools in the mold, as anyone in the laboratory who has tried to work with devitrified glass laboratory apparatus can testify.

Mr. Bates mentions the solubility of hydrated cement in water. For the past two years, at the Structural Materials Research Laboratory, we have been studying the mechanism of the reactions by which cement is attacked by sulfates. In the immense amount of material that has been published in connection with the effect of alkali water and sea water on concrete, two points have been seldom, if ever, mentioned. These are the effect of water itself on concrete, and the influence of the product of certain reactions on the velocity of these reactions. There is no question but that pure water may attack hydrated cement and when conditions are favorable to this action it may become serious. But, fortunately, conditions are rarely favorable.

There is a definite relation between velocity of solution and surface of contact between solvent and solute. With dense concrete the solution activity is confined almost entirely to the surface. Even if there were no other inhibiting factors, considerable time would be required under most conditions seriously to affect mass concrete. The presence of carbon dioxide in the atmosphere, and of carbon dioxide and carbonates in water, acts as a protecting agent for concrete by converting calcium hydroxide into calcium carbonate, which is considerably less soluble in water. A number of other inhibiting factors could be mentioned. The result is that the velocity of the action of most natural waters on well-made concrete in place is negligible.

Concrete subjected to percolating waters represents a very severe condition, but even in this case the concrete is not necessarily doomed. When hydrated cement is attacked by fresh water, calcium hydroxide goes into solution and gelatinous silica and alumina remain undissolved. These serve to free the pores, and either prevent more water from percolating through the concrete or cause the water to open new channels. I have found that even hydrochloric acid does not penetrate far into neat cement paste, because of the protection offered by the layer of gelatinous silica deposited on the surface.

LATE DATA ON THE EFFECT OF FINER GRINDING AND A HIGHER SO_3 CONTENT UPON THE PHYSICAL PROPERTIES OF PORTLAND CEMENT[1]

BY P. H. BATES[2]

A paper was presented at the 1915 meeting of this Society[3] which gave the results of an investigation on the effect of adding more SO_3 to and grinding approximately 12 per cent finer, as measured on the No. 200 sieve, ten commercial cements. At that time, the Society's Committee C-1 on Cement was discussing the question of higher SO_3 and finer grinding. Since then new specifications[4] have been adopted permitting the use of greater amounts of gypsum and requiring a greater degree of fineness. Hence, while at the present time neither of these questions is of as great interest as then, data from long-time tests bearing on these questions are of interest as giving confirmation of the desirability of having made the changes in the specifications. As the 1915 paper gave no later results than those obtained by testing the 26-week specimens, it was thought advisable to present the later data in a paper which will give the results of the one, three and five-year tests. In addition, there are presented the results showing the amount of "flour" (particles smaller than 0.001 in. in diameter) as determined by the Pearson method of separating fine particles in an air current.

It is not desirable to repeat here several of the tables or any great part of the matter presented in 1915. The interested reader should refer to the paper for detailed information in regard to the brands of cement used, the method of finer grinding and adding the SO_3, the chemical analysis, time of set, kind of specimen made, and the discussion of the results of the early test data.

Briefly, the original outline of the work comprised the purchase on the open market of ten cements chosen for their known rather widely different characteristics. Each lot was divided into four portions. Test specimens were made from one portion without any further treatment, the data obtained from this are given in the tables under headings "as received" or a letter and the numeral "1." A second portion was mixed in a ball mill with sufficient plaster-of-Paris to bring the SO_3 content to 2.5 per cent. No grinding was done in the mill. The data obtained from the cement in this case are referred to under the headings "plastered" or a letter and the numeral "2." A third portion was ground in a ball mill to the fineness indicated in Table I and the specimens made therefrom are referred to as "ground" or a letter followed by the numeral "3." The fourth portion was ground in the ball mill with the same amount of plaster-of-Paris as used with the second portion, to the fineness indicated in Table I. The data obtained from this are referred to under the headings "ground-plastered" or a letter followed by the numeral "4."

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[1] Published by permission of the Director of the U. S. Bureau of Standards.

[2] P. H. Bates, Chemist, U. S. Bureau of Standards, Washington, D. C.

[3] P. H. Bates, "The Effect of Finer Grinding and a Higher SO_3 Content upon the Physical Properties of Portland Cement," *Proceedings*, Am. Soc. Testing Mats., Vol. XV, Part II, p. 128 (1915).

[4] 1921 Book of A.S.T.M. Standards, p. 530.

TABLE I.—COMPARISON OF FINENESS DETERMINED BY AIR ELUTRIATION WITH FINENESS DETERMINED BY SIEVING.

"As Received"						"Ground"						"Ground-Plastered"					
Cement	Flour, 0.001 in.	Cement	Passing No. 200 Sieve	Cement	Passing No. 100 Sieve	Cement	Flour, 0.001 in.	Cement	Passing No. 200 Sieve	Cement	Passing No. 100 Sieve	Cement	Flour, 0.001 in.	Cement	Passing No. 200 Sieve	Cement	Passing No. 100 Sieve
J	33.7	G	82.2	C	98.8	J	42.9	J	92.7	K	100.0	J	43.6	J	93.6	K	99.8
D	32.4	K	81.8	K	98.4	A	41.3	K	92.4	G	99.8	A	41.8	K	92.2	J	99.7
A	32.2	J	81.4	A	96.6	D	38.0	G	91.2	H	99.6	D	39.2	A	91.6	E	99.6
H	30.8	B	79.4	J	96.4	C	37.3	H	91.0	E	99.6	B	38.2	G	91.0	G	99.6
C	29.8	A	79.2	G	96.4	H	37.2	F	90.8	F	99.4	C	37.8	E	91.0	H	99.6
G	29.6	F	79.0	B	95.8	E	37.2	E	89.8	C	99.4	G	36.8	F	90.0	D	99.4
F	29.6	D	77.8	H	95.6	G	37.1	B	89.2	A	99.4	H	36.3	H	90.0	F	99.3
B	29.6	H	77.6	E	95.0	F	36.2	A	88.8	B	99.2	E	35.8	D	90.0	B	99.2
E	27.4	E	75.8	D	94.8	B	35.5	D	88.6	D	99.2	F	34.9	B	89.8	C	99.2
K	26.0	C	75.4	F	93.8	K	34.2	C	86.8	J	99.2	K	33.4	C	88.6	H	99.0

Table I presents the fineness data as given in Table I of the original paper and in addition the results obtained by separating out the "flour" by means of a current of air. The significant feature of this table is the fact that in increasing the No. 200-sieve fineness of a group of cements by further grinding the percentage of flour of the different cements is not increased in the same ratio. For instance, cement J when "ground-plastered" had the No. 200-sieve fineness increased 12.2 per cent and the "flour" increased 9.9 per cent; whereas cement H had the No. 200-sieve fineness increased 12.4 per cent but the "flour" increased only 5.5 per cent. Other examples of the same character may be found in this group and the "ground" group. The reason for this lies in the fact that the different cements were from different mills and different mills produce different clinkers. In reducing a hard tough clinker to a greater No. 200-sieve fineness, it is quite possible that more "flour" will be produced, relatively, than in reducing a soft one. This may be due to the longer time that a tough clinker would be in the mill and hence the possibility of more of the fine material just passing a No. 200 sieve being reduced still further and partly to "flour." Also the tough clinker is reduced more by abrasion than by crushing and hence also the possibility of the finer portions from the former containing more "flour." If, however, the clinker is hard and glassy, it possibly reduces to a No. 200-sieve fineness quicker than a soft clinker and would hence contain less "flour."

Table II is presented as a means of readily comparing the effect of the additional treatments of the cements on the compressive strength of the concretes. In this latter table the cements are placed in a descending order of the percentage increase in the "flour" content produced by the grinding (without any addition of SO_3). It is noticeable that a majority do not show as great an increase in strength produced by the subsequent treatment at the end of five years as they do at the end of 28 days. This would appear to confirm the opinion held by some that finer grinding imparts a higher early strength, but that with time the strength of the coarser cements will approach those of the finer ground ones. However, this difference is not very marked when averages are considered. At the end of 28 days the cement specimens made from the cement "as received" gave an average strength of 2415 lb. per sq. in., while those made from the "ground" cement gave a strength of 3100 lb. per sq. in., and those made from the "ground-plastered" cement gave a strength

TABLE II.—COMPARISON OF INCREASED FINENESS AND INCREASED STRENGTH OF CONCRETE

Cement	Increase by "Grinding"				Cement	Increase by "Grinding-Plastering"			
	Fineness, per cent		Strength, lb. per sq. in.			Fineness, per cent		Strength, lb. per sq. in.	
	Flour	Passing No. 200 Sieve	28 days	5 years		Flour	Passing No. 200 Sieve	28 days	5 years
E	9.8	14.0	1025	−280	J	9.9	12.2	700	1205
J	9.2	11.3	1020	565	A	9.6	12.4	1520	1380
A	9.1	9.6	675	900	B	8.6	10.2	−295	−355
K	8.2	10.6	790	840	E	8.4	15.4	210	275
C	7.5	11.4	440	1735	K	7.4	10.8	415	300
G	7.5	9.0	1185	700	C	7.4	13.2	675	1710
F	6.6	11.8	505	−1270	G	7.2	8.8	1330	210
H	6.4	13.4	625	−30	D	6.8	12.2	730	785
B	6.3	9.8	−275	−420	H	5.5	12.4	445	345
D	5.6	10.8	865	820	F	5.3	11.0	395	−820
Average..	7.6	11.2	685	355		7.6	11.8	615	505

TABLE III—STRENGTHS DEVEL

All values, which are in pounds per square

Kind of Specimen		Tested at end of	Cement No.				Cement No.				Cement No.				Cement No.				Cement No.			
			A1	A2	A3	A4	B1	B2	B3	B4	C1	C2	C3	C4	D1	D2	D3	D4	E1	E2	E3	E4
NEAT SP																						
Stored in Water	Neat Tension	24 hours	232	257	272	445	205	442	347	409	430	534	349	515	377	442	367	481	346	259	440	399
		7 days	645	639	667	844	720	840	882	863	629	769	847	927	662	679	574	818	773	875	978	864
		28 days	708	883	670	736	625	900	697	887	833	988	861	941	727	738	716	823	839	875	821	878
		90 days	650	668	691	655	702	745	720	618	677	876	884	778	712	779	695	722	803	757	927	1 073
		180 days	660	798	621	677	652	767	622	775	752	895	715	807	712	727	657	757	844	818	819	864
		1 year	608	705	687	626	699	763	626	732	719	811	757	676	772	751	667	711	752	833	782	820
		3 years	499	609	599	687	673	812	650	572	644	728	732	722	819	653	719	740	852	875	902	867
		5 years	573	633	550	618	488	550	513	572	735	652	687	643	740	....	712	685	637	690	723	700
	Neat Compression	7 days	6 330	6 830	8 045	9 160	7 135	7 705	7 110	7 800	8 015	9 035	8 705	9 690	7 395	7 560	8 390	8 315	8 110	8 200	11 045	11 285
		28 days	9 825	10 370	12 050	11 825	8 480	8 565	8 150	9 940	10 395	12 070	9 540	12 020	9 235	10 485	11 410	10 075	12 360	12 570	14 855	14 380
		90 days	9 940	11 615	14 615	17 915	10 950	10 765	10 620	14 080	12 360	14 750	13 810	11 485	14 265	12 340	14 015	13 955	13 875	14 970	17 645	18 020
		180 days	8 720	10 710	11 860	11 010	9 295	10 685	9 315	10 290	10 430	11 980	10 420	11 805	11 890	13 050	13 510	14 550	17 730	16 170	17 410	20 170
		1 year	13 540	11 590	16 000	15 780	12 170	13 190	13 435	16 535	14 660	14 715	15 715	16 330	14 335	14 010	14 765	17 065	14 140	13 300	16 515	18 185
		3 years	8 110	7 470	6 825	10 040	8 085	8 885	7 010	9 170	8 295	10 100	10 330	9 870	8 930	9 750	9 645	9 730	13 630	11 835	12 995	13 385
		5 years	8 490	7 720	9 320	15 770	12 955	17 410	11 930	15 520	14 690	10 020	17 910	12 870	....	13 055	12 980	13 390	10 625	10 745	11 055	11 130
Combined Storage	Neat Tension	28 days	540	565	....	302	405	365	303	253	596	469	341	286	481	507	285	465	419	533	423	400
		90 days	571	694	....	662	669	427	407	622	635	750	566	616	718	718	666	669	866	818	487	528
		1 year	804	771	....	717	784	674	803	821	949	828	668	715	1 065	817	832	810	972	878	557	541
		5 years	617	560	....	580	783	763	643	753	773	707	867	760	800	717	810	653	823	743	853	607
	Neat Compression	28 days	9 610	10 935	....	....	10 320	13 050	10 345	11 030	11 785	11 165	9 910	12 400	9 890	10 890	10 850	11 240	11 645	11 290	17 055	15 760
		90 days	12 590	12 640	....	....	13 235	14 540	11 110	15 230	13 790	12 605	13 455	15 590	....	12 890	13 985	13 030	15 280	14 890	17 665	16 190
		1 year	13 950	14 185	....	....	15 560	16 500	17 590	12 915	15 705	17 710	16 695	17 305	....	17 345	16 335	15 685	16 945	16 245	19 340	20 395
		5 years	10 275	10 660	....	....	10 350	10 855	10 610	11 475	10 730	11 785	10 055	11 375	....	10 975	11 540	10 270	10 495	9 680	11 940	12 910
MORTAR AND																						
Stored in Water	Sand Tension	7 days	196	246	251	338	314	351	325	408	274	329	296	411	235	268	221	305	300	376	306	268
		28 days	312	381	344	431	394	381	422	481	316	398	392	424	329	384	400	473	427	412	455	453
		90 days	343	425	373	445	417	463	450	483	414	405	458	497	407	461	474	498	430	401	562	506
		180 days	327	402	399	462	462	425	463	500	423	465	491	502	331	502	498	480	460	436	444	453
		1 year	325	492	507	428	385	403	409	445	385	424	429	472	433	505	442	547	353	423	508	499
		3 years	398	346	389	415	390	427	438	444	379	446	414	449	418	465	469	456	406	376	436	483
		5 years	273	299	275	367	363	358	315	435	365	410	342	520	390	415	422	473	377	340	473	437
	Sand Compression	7 days	1 515	1 930	2 000	2 800	2 520	2 865	2 960	3 380	2 105	2 255	2 850	3 200	1 835	2 125	1 860	2 480	1 980	1 940	2 795	2 885
		28 days	2 395	3 405	2 910	4 175	3 445	3 715	4 270	4 470	3 225	3 375	4 355	4 155	2 915	3 460	3 660	3 880	3 525	3 085	4 310	3 820
		90 days	2 845	4 835	4 895	4 685	4 390	3 870	4 690	4 700	3 800	4 945	4 785	5 420	3 510	4 135	4 310	5 570	4 225	3 605	5 665	4 700
		180 days	2 885	3 240	3 845	3 655	3 310	3 775	3 635	3 530	3 585	3 245	3 970	3 935	3 445	3 805	4 710	4 290	4 740	4 400	6 240	5 035
		1 year	3 865	4 210	4 765	5 040	4 395	4 020	5 065	5 870	4 785	4 285	4 650	5 490	4 580	5 240	5 390	5 625	4 000	4 240	5 905	5 270
		3 years	2 685	3 590	3 100	3 690	3 040	2 980	3 630	3 940	3 025	3 200	3 950	4 395	3 255	3 360	3 460	3 750	4 070	3 215	3 960	4 700
		5 years	2 845	3 420	3 610	4 605	3 590	4 195	3 570	4 965	4 755	3 710	5 250	4 830	....	3 710	4 130	4 165	3 240	....	4 255	4 860
Combined Storage	Sand Tension	28 days	467	403	452	517	513	600	624	637	483	444	485	572	391	520	491	552	482	486	545	542
		90 days	504	496	653	689	563	633	446	693	563	570	603	643	565	546	513	651	562	526	587	613
		1 year	576	540	622	630	506	591	593	757	578	548	612	578	558	590	651	708	587	578	506	620
		5 years	750	733	567	420	693	733	553	567	730	717	670	503	703	573	580	590	733	723	560	517
	Sand Compression	28 days	3 345	3 450	4 460	5 725	4 690	5 215	5 305	5 115	3 865	3 870	4 450	4 585	3 015	3 720	3 950	4 065	2 890	3 140	6 265	4 915
		90 days	4 280	4 555	5 455	7 010	5 630	6 160	6 040	7 440	4 360	5 490	5 645	6 380	4 310	4 505	5 170	6 425	4 040	4 205	6 535	6 355
		1 year	5 460	5 255	6 215	6 640	6 160	6 310	6 735	7 325	5 985	6 915	6 635	7 170	....	6 685	7 300	7 785	5 790	6 495	8 065	6 715
		5 years	4 075	4 230	5 145	5 970	4 800	4 865	5 450	6 335	5 450	5 195	5 440	6 930	....	5 675	6 310	6 475	5 640	5 395	5 820	6 180
Damp Closet	Concrete	7 days	1 045	1 390	1 600	2 235	1 655	1 980	1 890	1 865	1 620	1 745	2 355	2 265	1 590	1 655	1 720	1 895	1 460	1 205	2 095	1 985
		28 days	1 760	2 360	2 435	3 280	2 675	2 740	2 400	2 380	2 420	2 610	2 860	3 095	2 285	2 760	3 150	3 015	2 595	2 485	3 620	2 805
		90 days	2 590	3 025	3 525	3 910	3 425	2 835	2 630	3 925	3 330	3 220	3 470	3 850	3 300	3 550	3 555	3 980	3 470	2 940	4 060	4 290
		180 days	2 110	3 100	3 015	3 960	3 005	3 180	2 880	2 970	3 415	3 575	4 065	4 295	3 390	3 640	4 250	4 345	4 415	3 325	4 040	4 665
		1 year	2 630	3 470	3 315	4 305	3 170	3 745	3 050	2 925	3 560	4 145	4 585	4 240	3 780	3 870	4 590	4 665	4 135	3 405	4 165	4 365
		3 years	2 590	3 875	3 740	4 610	3 895	4 180	3 400	3 540	4 330	4 440	4 900	4 015	4 280	5 100	5 140	4 800	2 860	3 320	5 100	5 450
		5 years	2 820	4 070	3 720	4 200	3 665	4 860	3 245	3 310	3 310	4 030	5 045	5 020	3 660	4 750	4 480	4 445	4 990	3 960	4 610	5 265

PLATE I.
AM. SOC. TESTING MATS.
VOL. 23, PART II.
BATES ON SO_3 CONTENT AND FINENESS OF CEMENT.

EVELOPED BY VARIOUS SPECIMENS

square inch, are the average of three specimens.

	Cement No.				Cement No.				Cement No.				Cement No.				Cement No.				Tested at end of	Kind of Specimen	
E4	F1	F2	F3	F4	G1	G2	G3	G4	H1	H2	H3	H4	J1	J2	J3	J4	K1	K2	K3	K4			
EAT SPECIMENS																							
399	500	216	423	432	326	170	428	291	403	455	403	451	287	143	322	376	318	165	298	209	24 hours	Neat Tension	Stored in Water
864	672	692	714	729	772	663	807	793	709	859	810	798	647	596	532	845	542	394	557	610	7 days		
878	671	760	767	766	768	909	782	866	763	768	732	816	698	848	667	703	616	700	623	712	28 days		
1 073	721	870	687	843	880	918	833	927	857	723	672	770	751	859	719	853	708	787	737	650	90 days		
864	769	881	761	795	801	700	631	793	787	733	667	805	696	785	668	819	778	715	699	708	180 days		
820	729	836	773	802	852	836	836	965	767	772	745	885	812	931	821	823	815	732	845	687	1 year		
867	817	812	772	968	751	817	671	723	698	713	737	707	566	545	483	675	680	499	653	502	3 years		
700	680	790	607	700	647	650	707	710	653	743	670	673	600	740	600	660	567	627	660	713	5 years		
285	8 980	8 450	9 855	10 390	8 970	5 370	9 755	10 330	6 695	7 865	7 800	8 210	6 995	7 935	8 125	10 160	6 545	5 620	6 570	8 870	7 days	Neat Compression	
380	11 165	10 685	11 660	12 835	11 245	11 800	11 605	11 665	7 695	9 525	8 045	9 150	10 890	11 900	11 040	13 780	8 790	9 200	8 425	11 300	28 days		
020	12 810	14 695	11 905	14 735	11 165	12 685	13 085	14 790	9 710	10 835	10 780	11 170	11 255	12 135	12 290	15 435	10 045	12 770	10 090	11 640	90 days		
170	14 225	14 760	16 650	17 870	15 460	14 870	17 490	15 830	11 640	12 820	12 220	12 455	13 605	16 345	16 410	18 160	12 080	15 810	13 240	17 580	180 days		
185	14 730	13 915	15 790	16 175	13 860	15 960	14 685	13 670	10 985	12 935	11 655	14 110	14 720	14 055	17 265	19 255	12 735	14 520	12 945	16 580	1 year		
385	12 675	11 855	11 610	12 045	10 315	11 755	9 410	12 800	10 050	12 675	12 130	....	13 000	13 955	13 525	13 885	10 790	11 825	11 575	14 940	3 years		
130	10 330	11 495	11 930	11 240	10 110	8 555	11 900	8 525	12 210	12 120	11 050	11 640	8 170	8 365	8 205	9 420	10 600	10 170	8 975	11 790	5 years		
400	329	360	375	283	444	485	282	383	407	337	225	400	687	575	847	740	486	513	392	471	28 days	Neat Tension	Combined Storage
528	566	687	650	561	762	851	638	714	707	804	592	650	668	828	806	731	556	742	632	513	90 days		
541	733	640	643	608	863	678	661	709	721	717	652	674	704	634	526	536	710	661	494	426	1 year		
607	887	753	847	827	690	800	660	633	810	747	753	787	513	585	580	783	745	617	660	600	5 years		
760	13 445	12 190	14 435	12 125	11 410	12 420	13 760	15 480	8 355	9 925	10 790	9 990	9 235	12 110	12 190	11 465	9 000	9 160	10 860	13 075	28 days	Neat Compression	
190	15 075	14 050	16 085	16 205	12 720	13 270	13 580	15 255	10 380	9 780	11 620	11 600	10 780	13 885	15 275	14 565	11 260	12 640	10 730	10 900	90 days		
395	18 300	15 875	18 865	20 295	15 530	17 920	16 790	17 985	12 830	13 605	13 555	14 605	14 660	15 320	18 435	21 075	14 410	14 840	15 920	19 750	1 year		
910	11 060	8 810	10 950	11 850	9 115	9 970	10 290	10 115	8 560	10 440	9 130	9 365	10 125	11 510	10 745	8 355	9 925	10 435	9 690	9 280	5 years		
AR AND CONCRETE																							
268	259	293	347	376	303	278	363	324	327	378	310	391	211	282	324	372	270	156	258	278	7 days	Sand Tension	Stored in Water
453	429	445	464	472	411	389	466	479	373	440	423	476	293	330	381	348	331	352	435	445	28 days		
506	458	473	527	592	499	454	529	481	412	529	460	547	429	483	472	482	423	439	560	428	90 days		
453	478	455	497	490	438	451	503	481	461	498	504	506	470	451	523	575	402	442	510	486	180 days		
499	428	475	476	439	401	455	405	460	405	439	484	500	489	468	524	505	406	410	516	458	1 year		
483	387	420	459	485	414	394	425	442	391	439	445	468	365	391	388	433	388	363	428	393	3 years		
437	395	437	373	380	357	397	340	387	307	350	410	363	347	393	370	373	383	357	413	373	5 years		
885	2 580	2 000	2 670	2 600	2 615	1 885	3 220	3 260	1 930	2 385	3 120	2 780	1 870	2 050	2 780	3 080	1 785	1 385	2 385	2 340	7 days	Sand Compreseion	
820	3 835	3 090	3 800	3 800	4 225	3 040	4 475	4 830	3 080	2 960	3 685	3 360	2 780	3 105	3 810	4 735	2 850	2 630	3 485	3 580	28 days		
700	5 015	4 850	5 375	5 265	4 350	4 330	4 670	5 665	3 825	3 655	4 400	4 250	3 960	3 975	5 445	5 465	4 160	3 020	4 535	4 865	90 days		
035	4 885	4 735	5 655	4 920	5 130	4 090	6 375	4 945	4 165	4 405	5 995	5 350	4 890	4 695	5 645	6 280	3 990	4 740	5 445	6 850	180 days		
270	4 810	4 705	5 090	6 040	5 215	4 550	5 715	5 655	3 955	4 235	5 745	5 440	4 140	4 995	6 760	7 550	3 770	3 495	5 380	5 345	1 year		
700	4 240	3 980	4 750	5 015	4 820	3 925	4 515	4 285	3 635	4 100	4 915	....	3 895	4 470	5 490	5 735	2 980	3 250	3 635	3 970	3 years		
860	....	3 645	4 060	2 950	4 600	3 395	4 465	4 370	4 100	3 685	5 210	3 680	3 800	3 320	4 190	4 420	3 525	3 290	3 740	4 950	5 years		
542	491	542	583	550	605	503	599	501	587	484	564	607	529	568	510	605	439	406	533	503	28 days	Sand Tension	Combined Storage
613	566	554	664	687	537	574	640	602	490	604	613	749	427	503	507	552	453	416	558	634	90 days		
620	646	586	634	656	592	577	676	641	636	667	647	729	606	634	634	666	513	629	619	656	1 year		
517	523	603	740	610	527	465	473	530	603	656	547	537	480	....	650	673	750	693	580	553	5 years		
915	4 275	4 370	5 715	6 060	4 360	4 570	5 990	6 845	4 390	4 230	5 875	5 475	4 155	4 210	4 650	4 570	3 590	3 315	4 870	4 130	28 days	Sand Compression	
355	4 665	4 745	6 270	6 590	5 540	4 840	6 765	7 475	4 845	4 915	5 790	6 750	4 460	4 980	5 415	6 190	4 590	4 560	5 550	4 485	90 days		
715	6 450	6 585	6 205	9 475	6 615	6 815	8 020	7 145	5 140	5 755	7 250	7 095	5 360	5 145	5 660	6 980	4 115	6 140	7 595	7 785	1 year		
180	4 940	5 760	6 045	7 150	5 265	6 140	6 265	5 325	5 035	5 110	5 640	6 570	4 910	5 190	5 070	6 345	4 975	4 650	6 010	6 060	5 years		
985	1 725	1 660	1 940	1 785	1 460	1 805	2 380	2 585	1 910	2 100	2 200	2 385	1 370	1 550	2 240	2 390	1 325	1 110	1 975	1 515	7 days	Concrete	Damp Closet
805	2 750	2 715	3 255	3 145	2 370	2 750	3 555	3 700	2 590	2 965	3 215	3 035	2 635	2 810	3 655	3 335	2 085	2 155	2 875	2 500	28 days		
290	3 790	3 530	4 275	4 035	3 345	3 270	4 640	4 240	3 775	3 755	4 525	4 225	3 020	2 910	3 900	3 460	2 585	2 700	3 700	2 685	90 days		
665	4 135	3 780	4 295	4 175	3 655	3 730	4 625	4 900	4 100	3 855	4 385	4 565	3 835	3 870	4 990	4 995	2 835	3 420	4 480	3 630	180 days		
365	4 165	4 645	4 200	4 285	3 625	4 435	5 245	5 580	4 250	4 670	4 790	4 985	4 315	4 815	5 170	4 810	3 910	3 185	4 920	4 160	1 year		
450	4 000	4 930	4 210	5 380	4 080	4 020	5 270	5 920	4 400	4 150	4 800	4 455	4 370	4 435	5 190	5 070	4 300	3 840	4 850	4 310	3 years		
265	5 060	4 795	3 790	3 930	4 090	4 305	4 790	4 300	4 840	3 780	4 810	5 185	5 120	5 230	5 685	6 325	4 300	4 080	5 140	4 600	5 years		

of 3030 lb. per sq. in. At the end of five years the average strengths of the concretes from the same cements were 4185, 4535 and 4690 lb. per sq. in., respectively. Hence, there had been produced at the end of 28 days an average gain of 685 and 615 lb. per sq. in., or 28 and 25 per cent, respectively, by the respective treatments as against 355 and 505 lb. per sq. in., or 8 and 12 per cent, at the end of five years.

Although averages are interesting and instructive, the deportment of the individual cements should not be lost sight of. Attention was directed in the original paper to the fact that there is a marked difference in the reaction of the different cements to the same treatment. This is further exhibited in the later tests as shown in the tables. The strengths of the concretes at five years show that one of the cements produced the better results if used "as received"; two if used with the addition of SO_3 only; three if further ground; and five if "ground-plastered." The concretes at the same age show that if the investigation had been restricted to the addition of SO_3 only, four cements would have been improperly treated, as they had their strengths reduced by so doing. The same number of cements would have been deleteriously affected, at the same age, if finer grinding alone were considered. Attention is called to these facts solely to bring out the point that each cement must be treated as an individual case. So far as strength is concerned, there is a different optimum to be reached with each cement in the use of SO_3 and grinding, and it is obvious in four out of the ten cases in this investigation, that the manufacturer had reached or possibly exceeded this optimum.

A close study of the data in the tables will show that the treatments affect the cements differently as shown by the results obtained from the different types of specimens. Thus, a cement as a mortar in tension may show slight changes produced by any treatment, but as a mortar in compression or as a concrete, it may show that marked changes have been produced. This emphasizes the difficulty of recommending for adoption as part of a standard any particular type of test piece or mixture of cement and aggregate on the basis that the results to be obtained by its use will be entirely indicative of the value of any particular cement in all its possible uses. The data show distinctly that the different cements have different properties as a neat cement, or as a mortar or as a concrete. A certain average relation may be obtained in any series of tests between the mortar strength of a group of cements and the concrete strength, but any analysis of the complete data will show that individual cements may depart widely from the average in some particular and approach it in another.

CONCLUSIONS

In drawing conclusions, it has been thought to be desirable to present (in quotations) those given in the original paper, adding parenthetically to each any changes or new matter which has resulted from a study of the long time tests.

1. "So far as time of set is concerned, no disadvantageous results have been produced. The fine grinding produces a quicker set; the additional SO_3 produces a quicker initial but slower final set. Few of the changes however are such as to place the treated cement beyond specification limits."

2. "Either fine grinding, the addition of SO_3, or fine grinding plus addition of SO_3, generally improves the strength of neat specimens. The neat specimens, however, do not give very consistent results, and not much value should be placed upon them. The indications are that at later periods, the results for all neat specimens, including those made from the coarse cements, will be very close together."

(The late tests show that this type of specimens gives just as erratic results as it did at the end of the earlier periods. However, the indications that for the late periods, the results would be rather close together, have not been borne out. Maximum strength was apparently obtained at one year, since when there has been a marked retrogression in strength.)

3. "The strength of the standard sand mortars in tension shows the advantage of all of the three treatments of the original cements. In the compression test specimens the addition of SO_3 to the coarse cement has caused a falling off of the rate of gain of strength, which might be assumed to be caused by steadily increasing disruptive action. Whether this will appear later with the finer-ground cements, to which the addition was made, cannot be foretold from the present results. It may appear with the long-time specimens, and again it may be that the finer grinding has produced sufficient strength to entirely counterbalance any disruptive action due to too much SO_3."

(The mortar specimens show that the maximum strength was developed sometime before the three-year period.)

4. "The 'combined-storage' method of aging the small specimens has given results which are of interest to American manufacturers and users of cement, since but very few results of tests conducted in this manner on American cements are available. These results are of more interest in this one particular, than in giving a satisfactory conclusion in regard to the value of finer grinding or further addition

of SO_3. However, the neat specimens broken in tension do not show that the cements are improved by either of the treatments used; and the results with these compression specimens are not very conclusive; the sand tension test specimens do not show that they have been as markedly improved by the treatments as do the similar specimens normally stored; the sand compression test specimens are more conclusive and do show the value of finer grinding with or without addition of SO_3."

(This condition of aging shows that it is of value only in the earlier periods. In general, it gives strengths of the same character as the long-time water storage, but considerably in excess of the latter.)

5. "At the end of 28 days the grand average strength of all the concrete specimens, made from the cements as received, was 2417 lb. per sq. in.; of the specimens made of the cement with the additional plaster, 2635 lb. per sq. in.; of the specimens made from the finer-ground cement, 3103 lb. per sq. in.; and of the specimens made from the finer-ground cement further plastered was 3028 lb. per sq. in.; the strength of the concrete of these specimens at the end of 90 days was 3263, 3175, 3828 and 3760 lb. per sq. in., respectively. From these figures it appears that at the end of 28 days, by the above-mentioned treatment, the original amounts have been improved about 10, 28 and 25 per cent, respectively; and at the end of 90 days about 0.0, 17 and 15 per cent, respectively. It would appear, therefore, from these grand averages that at later periods the original cements may give as good strength values in concrete as the treated cements. It might even be assumed that the further SO_3 in the coarse cements was not desirable from the standpoint of strength. It is, however, very evident that finer grinding is very advantageous for early strength, and that the addition of more SO_3 to such finer-ground cements to more properly control their set, is not disadvantageous at this period."

(Comparisons of this character are given in the present paper, showing the relation between the 28-day and 5-year strengths of concretes.)

6. "The results of expansion measurements on neat specimens stored in water show large increases in length for the specimens made from the cements containing more SO_3. In the sand specimens this is not so apparent. At present these specimens do not furnish much aid in studying the problem under investigation. It may be, however, that these neat specimens are foretelling the ultimate results which will be noticed in the strength specimens."

(The making of expansion measurements was not conducted after the 26-week period. The data that were being obtained were not of a character to warrant further expenditure of the time required to make the measurements.)

7. "The results are not conclusive as to the effect of the composition upon finer grinding or further SO_3. Neither can any satisfactory conclusion be drawn from the constitution of the cements."

8. "What results have been obtained in the study of the relative amounts of hydration, produced by the treatments in the 13-week neat tension test specimens, are not sufficient to make any positive statement. The study of this phase of the subject is being continued with the 26-week concrete specimens."

(The amount of hydration was obtained at the 26-week period. In the case of cement H, that portion which had been further ground, showed less than 1 per cent increased hydration. The portion which had been used as received had hydrated about 6 per cent more. In the case of cement E, the portion which had been further ground had increased in hydration about 11 per cent, while the portion which had been used as received had increased about 15 per cent. These results indicate rather clearly the slowing up of the rate of hydration of the fine cements and the continued greater rate of hydration of the coarse ones. Such information cannot be obtained from a study of he strengths.)

BLAST-FURNACE SLAG AS AN AGGREGATE IN CONCRETE

By Raymond Harsch[1]

The increase in the use of blast-furnace slag for aggregate in nearly all types of concrete construction has been far more rapid and more general than the development of a suitable or standard specification for this material. There are available and in use at the present time about fifteen state specifications for slag aggregate for concrete. No two specifications require the same limits for the material. Just what data were used as a basis for determining the limits stated in these specifications is not known. It must have been quite conflicting, however, to warrant so many variations in the specifications from neighboring states.

The U. S. Bureau of Public Roads started an extensive investigation in the summer of 1919 to obtain information relative to blast-furnace slag as an aggregate in concrete. This investigation consisted in obtaining plant data from the field inspection of about thirty crushing and screening plants which were at that time producing slag in commercial sizes. Samples were selected from the bins or stock piles of these plants at the time of this inspection, and shipped to the laboratory for further tests. The plant data were obtained by two inspectors, each visiting alone one-half of the plants, with the exception of the first three. These were visited by both inspectors at the same time to standardize the procedure to be followed at the other plants. Although the original data secured contained detailed information of all plant equipment and methods, together with analyses of the ingredients and product as determined by the steel plant chemist, this paper will be confined chiefly to a discussion of the general methods of production and a detailed study of slag when tested as an aggregate in concrete.

Blast-furnace slags are essentially silicates of alumina and lime. The main constituents are silica, alumina, magnesium and lime, with a small percentage of sulfur and other compounds. Each of these elements has a particular effect on the physical characteristics of the slag, depending mainly upon the relative percentages of each contained therein. The relation of the chemical composition of the slag to its physical characteristics has been studied in great detail by Wallace G. Imhoff.[1] The general effect of each main constituent upon the physical character of the slag is taken from the results of this study.

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[1] Junior Assistant Testing Engineer, U. S. Bureau of Public Roads.

Silica tends to make what is known as a "lean" slag. Such a slag is very fluid and thin when hot and upon chilling has a glassy appearance and fracture. The percentage of silica may vary considerably even between pourings from the same furnace The blue or blue-gray color of slag is attributed to the presence of alumina. Lime occurs as silicate and sulfides and is responsible for the stony appearance of the slag. Magnesium is present in varying quantities depending upon the temperature condition of the furnace. It influences the color of high-silica slag but does not affect high-lime slags, except to possibly prevent unsoundness upon cooling. Sulfur occurs as sulfides and sometimes in the free state.

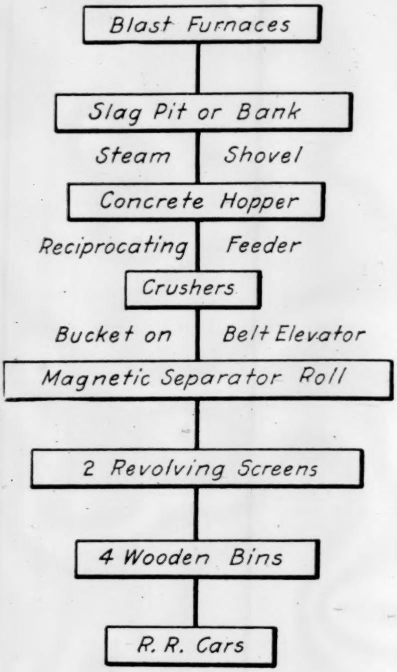
Blast Furnaces
Slag Pit or Bank
Steam Shovel
Concrete Hopper
Reciprocating Feeder
Crushers
Bucket on Belt Elevator
Magnetic Separator Roll
2 Revolving Screens
4 Wooden Bins
R. R. Cars

FIG. 1.—Diagram or Flow Chart of Typical Slag Crushing and Screening Plant

An impurity which is now being controlled to a very high degree is the free iron occurring in lumps of some size and also as small shot. This affects the weight of the slag and also its strength.

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[1] *Blast-Furance and Steel Plant Magazine*, March, 1917.

PRODUCTION OF BLAST-FURNACE SLAG

At the time of the inspection of the thirty-two plants, they were producing or could produce a total of 31,000 tons of crushed and screened aggregate per day. The capacities of the plants ranged from 300 to 4000 tons per day. The processes used by the different producers varied between wide limits, but the general route of the material for all the plants is shown in Fig. 1. This is the typical flow chart of an average plant which performed all the operations necessary in the production of the crushed and screened aggregate.

There exist today three general methods of disposing of the molten slag, practiced by the slag or steel companies.

1. *The Pit Method.*—This method is in use by many of the companies and generally by those where there is room for a pit close to the furnaces. The molten slag runs from the furnace in a small stream restrained by a cinder trench. The slag runs into one end of the pit and flows by gravity toward the lowest points of it. The slag pits (usually two) are surrounded on three sides by embankments of earth or old slag or in many cases by brick or concrete walls. The dimensions of the pits vary with the amount of slag produced and the room available.

2. *The Bank System.*—In the bank or dump system, the molten slag is drawn from the furnace into large ladles of 15-tons capacity, mounted on standard railway trucks. These ladles are hauled, in trains composed of two to seven ladles, to dumps or extensive banks, and emptied while the slag is still in the fluid condition. The length of haul varies from $\frac{1}{4}$ mile to a maximum of 6 miles. The maximum time from assembling of the train to pouring the slag over the bank is about three hours.

3. *The Modified Pit System.*—This system is a combination of the pit and bank systems, and is claimed to have the advantages of both without the disadvantages of either. The main features of this method are common to both the previous methods. The pits are very much larger and resemble the banks or dumps, except that the slag is poured into one pit beginning at one end and advancing toward the other as the pit becomes full, while the cold slag is being removed from the adjacent pit. It is fundamentally a pit system of proportions approaching the large banks or dumps and is made possible by the hauling of the slag in ladles as in the bank system.

After cooling, the slag is quarried with steam shovels or other suitable digging equipment. The crushing and screening is accomplished by methods similar to those used in the production of crushed stone. A magnetic separator for extracting the iron is an additional feature of many plants.

TABLE I.—WEIGHT PER CUBIC FOOT OF SLAG DETERMINED AT THE PLANTS

Plant	Nominal Size, in.	Weight per Cubic Foot, lb.	Condition of Samples
No. 1............	$3 - 4$	76.0	Damp
	$1 - 3$	78.0	"
	$\frac{1}{2} - 1$	77.0	"
	$\frac{1}{4} - \frac{1}{2}$	78.0	"
	Dust	83.0	"
No. 2............	$1\frac{1}{4} - 3\frac{1}{2}$	75.5	Damp
	$\frac{1}{2} - 1\frac{1}{4}$	78.9	"
	$\frac{3}{16} - \frac{1}{2}$	75.0	"
No. 3............	4	88.8	Wet
	$1\frac{1}{2} - 2\frac{1}{2}$	80.2	"
	$\frac{3}{4} - 1\frac{1}{2}$	81.2	"
	$\frac{5}{8} - \frac{3}{4}$	83.0	"
	$\frac{1}{4} - \frac{5}{8}$	84.7	"
	$\frac{3}{16} - \frac{7}{16}$	85.2	"
	Dust	86.4	"
No. 4............	4	85.0	Wet
	$1\frac{1}{2} - 2\frac{1}{2}$	87.4	"
	$1 - 1\frac{1}{2}$	89.6	"
	$\frac{5}{8} - 1$	93.3	"
	$\frac{1}{2} - 1$	91.5	"
	$\frac{3}{16} - \frac{5}{8}$	97.7	"
	$\frac{3}{16} - \frac{1}{2}$	91.9	"
	$0 - \frac{3}{16}$	99.6	"
No. 5............	$2\frac{1}{2}$	73.2	Damp
	$1\frac{1}{2}$	72.3	"
	$\frac{3}{4}$	76.3	"
	Roofing	75.4	"
	Grit	84.8	"
	Sand	101.6	"
No. 6............	3	60.0	Damp
	$1\frac{1}{2} - 2\frac{1}{2}$	62.0	"
	$\frac{1}{4} - 1\frac{1}{2}$	67.0	"
	$0 - \frac{1}{4}$	86.0	"
No. 7............	$1\frac{1}{2} - 4$	73.0	Dry
	$\frac{1}{4} - 1\frac{1}{2}$	81.3	"
	$0 - \frac{1}{4}$	87.3	"
No. 8............	$3 - 4$	74.5	Dry
	$2 - 3$	70.5	"
	$1 - 2$	73.0	"
	$\frac{1}{2} - 1$	72.0	"
	$\frac{1}{4} - \frac{1}{2}$	75.0	"
	$0 - \frac{1}{4}$	97.0	"
No. 9............	$2\frac{1}{2} - 4$	67.0	Dry
	$1\frac{1}{2} - 2\frac{1}{2}$	67.0	"
	$\frac{3}{4} - 1\frac{1}{2}$	68.5	"
	$\frac{1}{2} - \frac{3}{4}$	73.0	"
	$\frac{1}{4} - \frac{1}{2}$	68.5	"
	$0 - \frac{1}{4}$	94.5	"
No. 10...........	$1\frac{3}{4} - 2\frac{1}{2}$	82.0	Dry
	$1 - 1\frac{3}{4}$	80.0	"
	$\frac{3}{8} - 1$	80.5	"
	$0 - \frac{3}{8}$	92.5	"
No. 11...........	$1\frac{3}{4} - 3\frac{1}{2}$	101.6	Dry
	$\frac{3}{4} - 1\frac{3}{4}$	95.6	"
	$\frac{3}{8} - \frac{3}{4}$	91.1	"
	$0 - \frac{3}{8}$	92.2	"
No. 12...........	$1\frac{1}{2} - 2\frac{1}{2}$	72.8	Dry
	$\frac{3}{4} - 1\frac{1}{2}$	81.4	"
	$\frac{1}{4} - \frac{3}{4}$	76.8	"
	$0 - \frac{1}{4}$	90.3	"
No. 13...........	$1\frac{1}{2} - 3$	68.8	Dry
	$\frac{3}{4} - 1\frac{1}{2}$	76.5	"
	$\frac{1}{4} - \frac{3}{4}$	82.1	"
	$\frac{1}{4} - \frac{1}{2}$	75.0	"
No. 14...........	$2 - 3\frac{1}{2}$	68.5	Damp
	$1\frac{1}{2}$	64.0	"
	$\frac{3}{4}$	66.0	"
	$\frac{1}{4} - \frac{1}{2}$	74.5	"
	$0 - \frac{1}{4}$	78.5	"
No. 15...........	$2 - 3\frac{1}{2}$	77.0	Dry
	$1 - 1\frac{1}{2}$	85.0	"
	$\frac{5}{8} - 1$	88.0	"
	$\frac{3}{16} - \frac{5}{8}$	86.5	"
	$0 - \frac{3}{16}$	85.0	"
No. 16...........	$2 - 3$	74.7	Damp
	$1\frac{1}{2}$	80.2	"
	$\frac{3}{4}$	81.9	"
	$\frac{1}{4} - \frac{1}{2}$	90.5	"
	$0 - \frac{1}{4}$	94.8	"
No. 17...........	$1\frac{3}{4} - 3$	78.8	Dry
	$1\frac{1}{4} - 1\frac{3}{4}$	67.4	
	$\frac{3}{8} - 1\frac{1}{4}$	84.2	"
	$0 - \frac{3}{8}$	92.6	"
			"
No. 19...........	$1\frac{3}{4} - 2\frac{1}{2}$	67.6	
	$1\frac{1}{4} - 1\frac{3}{4}$	69.8	
	$\frac{3}{8} - 1\frac{1}{4}$	75.7	
	$0 - \frac{3}{8}$	90.1	
No. 21...........	$1\frac{1}{2} - 2\frac{1}{2}$	78.9	
	$\frac{7}{8} - 1\frac{1}{2}$	79.6	
	$\frac{3}{8} - \frac{7}{8}$	79.3	
	$\frac{5}{16} - \frac{3}{8}$	77.0	
	$0 - \frac{5}{16}$	78.5	

TABLE I.—WEIGHT PER CUBIC FOOT OF SLAG DETERMINED AT THE PLANTS—*Continued*

Plant	Nominal Size, in.	Weight per Cubic Foot, lb.	Condition of Samples
No. 22	1½ - 2½	80.8	Wet
	¾ - 1½	73.5	"
	⅜ - ¾	83.9	"
	¼ - ⅜	77.0	"
	0 - ¼	79.5	"
No. 23	3¾	75.7	Wet
	2½ - 3¾	78.3	"
	1⅜ - 2½	71.1	"
	⅝ - 1⅜	65.4	"
	¼ - ½	71.0	"
	0 - ¼	82.8	"
No. 24	2¾ - 3¾	65.4	Dry
	1½ - 2¾	67.3	"
	⅝ - 1½	69.0	"
	⅜ - ⅝	69.0	"
	0 - ⅜	75.0	"
No. 25	1¾ - 3	95.3	
	1 - 1¾	99.8	
	½ - 1	100.5	
	0 - ½	101.6	
No. 26	2¾ - 3¾	68.7	Dry
	1½ - 2¾	68.2	"
	⅝ - 1½	77.8	"
	⅜ - ⅝	70.5	"
	0 - ⅜	86.8	"
No. 27	2¼ - 3	77.0	Wet
	2¼	78.9	"
	1¼	89.0	"
	⅞	92.1	"
	⅜	89.6	"
No. 28	2½ - 4	68.3	Damp
	1 - 2½	74.5	"
	½ - 1	82.0	"
	⅜ - ½	90.6	"
	0 - ⅜	86.7	"
No. 29	1½ - 2½	79.1	Damp
	¾ - 1½	81.9	"
	½ - ¾	89.1	"
	¼ - ½	104.8	"
No. 30	1½ - 2½	87.8	Damp
	¾ - 1½	82.9	"
	½ - ⅝	83.9	"
	0 - ¼	77.1	"
No. 32	2½	70.5	Damp
	1½	71.9	"
	1¼	76.7	"
	¾	92.5	"
	0 - ½	84.8	"
	0 - ¼	93.4	"

FIELD TESTS MADE AT THE PLANTS

Samples were selected from the bins and stock piles of each plant. These samples were representative of the material on hand and all were more than one cubic foot in size. Tests for mechanical analysis, specific gravity and weight per cubic foot were made upon each size of material produced by the plant.

Mechanical Analysis.—The screens used for the mechanical analysis were 8-in. diameter laboratory screens, having circular openings from ¼ to 3 in. in diameter. As the grading is only important in its relation to the weight per cubic foot the detailed results are not included in this paper.

Specific Gravity.—This was determined by weighing about ten pounds of air-dry slag in air and also in water, the specific gravity being computed from the dry weight and the difference in weight in air and in water.

Weight per Cubic Foot.—A cubic foot box was used in determining the weight per cubic foot. The box was filled in four equal layers and the box rocked on its edges 20 times between each layer introduced. This was equivalent to shaking to refusal. Table I gives the results of the field determination of unit weight of slag.

To verify these results and for making additional tests on the material from each plant, samples were taken from bins, cars or stock piles which were as nearly as possible representative of the product sold to the contractors. The samples were selected as follows:

TABLE II.—ROUTINE TESTS ON SLAG SAMPLES.

Plant	Serial Number	Specific Gravity	Weight per Cubic Foot, lb.	Absorption		**Percentage of Wear**
				lb. per cu. ft.	per cent	
Number 1	18 737	2.16	135	5.16	3.82	19.5
Number 2	18 734	2.00	125	5.80	4.64	15.4
Number 3	18 729	2.24	140	3.78	2.70	13.7
Number 4	18 861	2.39	149	2.54	1.71	10.1
Number 5	18 736	2.07	129	6.15	4.74	11.9
Number 7	18 733	1.88	117	6.76	5.78	....
Number 8	18 732	2.19	137	5.63	4.11	13.0
Number 9	18 738	1.97	123	5.34	4.34	....
Number 10	18 728	2.17	136	3.95	2.92	8.6
Number 11	18 717	2.82	176	0.39	0.22	4.5
Number 12	18 731	1.97	123	5.71	4.64	24.3
Number 13	18 727	2.08	129	6.33	4.91	14.5
Number 14	18 862	1.83	114	8.08	7.08	18.8
Number 15	18 863	2.26	141	3.24	2.30	9.5
Number 16	18 735	2.13	133	4.83	3.61	14.4
Number 17	18 730	2.18	136	5.12	3.76	21.0
Number 19	18 718	2.25	141	4.09	2.94	15.6
Number 21	18 864	1.91	119	5.63	4.73	18.3
Number 22	18 865	2.16	135	3.51	2.60	15.7
Number 23	18 866	2.17	135	2.35	1.74	16.5
Number 24	18 719	1.93	120	6.32	5.27	15.8
Number 25	18 739	2.02	126	1.17	0.93	19.9
Number 26	18 720	2.05	128	5.72	4.47	15.4
Number 27	18 740	2.14	134	5.65	4.22	13.1
Number 28	18 867	1.95	121	5.10	4.22	19.5
Number 29	18 721	2.19	137	4.44	5.24	14.7
Number 30	18 722	2.22	139	1.30	1.29	11.0
Number 32	18 723	1.83	114	8.53	7.48	17.6

NOTE.—The sodium sulfate test for soundness of coarse aggregate (Tentative Test No. 39, Department of Agriculture *Bulletin No. 949*) showed all of the samples to be durable, free from checking, cracks, or signs of disintegration

Plant No. 1.—116 cu. ft.;
Plants Nos. 2, 3, 4, and 28.—35 cu. ft. each;
All Other Plants.—5 cu. ft. each

The samples were shipped to the laboratory in used cement sacks, identified by tags bearing the plant number and the size of the material.

ROUTINE TESTS

Slag samples from each plant were subjected to the routine tests for road-building rock as described in U. S. Bureau of Public Roads

Bulletin No. 347. Results of these tests are shown in Table II. A close study of this table shows how inadequate the usual tests for road-building rock are to determine the suitability of material such as slag for road construction. Although the specific gravity of slag varied from 1.83 to 2.88, there seems to be no relation between the density of the material and its wearing properties as determined in the Deval abrasion cylinders. Furthermore, the specific gravity and absorption tests are not true indicators of the porosity of slag. The presence of many vesicular openings on the surface of the slag pieces influence the results.

TABLE III.—CHEMICAL ANALYSES OF SLAG

Plant	Chemical Composition, per cent								
	SiO_2	TiO_2	Al_2O_3	FeO	MnO	CaO	MgO	S	H_2O CO_2
No. 1	36.25	0.60	13.61	0.86	0.98	38.25	6.48	1.60	1.60
No. 2	34.95	0.36	14.41	0.86	0.82	42.30	3.51	1.59	1.30
No. 3	36.85	0.60	13.85	1.00	0.83	40.00	3.46	1.46	2.20
No. 4	35.95	0.50	18.46	1.08	0.49	35.35	4.36	1.16	2.15
No. 5	34.50	0.60	20.27	1.08	0.73	36.65	3.57	1.65	1.25
No. 7	34.90	0.36	14.37	0.79	0.34	43.05	3.64	1.51	1.40
No. 8	34.70	0.45	16.10	1.58	0.74	41.15	2.43	1.48	1.25
No. 9	35.50	0.48	11.88	1.73	0.87	41.05	5.61	1.48	0.85
No. 10	39.05	0.40	13.55	1.15	0.47	39.30	3.51	2.30	....
No. 11	40.30	0.32	18.13	0.86	0.09	33.75	4.96	1.32	0.45
No. 12	36.30	0.52	15.98	1.37	0.88	37.60	5.96	1.33	....
No. 13	31.65	0.40	12.46	1.22	0.33	41.00	2.55	2.46	7.60
No. 14	29.90	0.60	15.93	3.17	0.35	33.60	6.82	1.92	7.63
No. 15	34.00	0.60	12.29	1.22	0.80	40.65	6.66	1.77	2.20
No. 16	34.20	0.50	13.68	1.15	0.69	39.00	5.79	1.70	3.80
No. 17	34.25	0.40	13.41	1.08	0.69	43.63	4.42	1.21	1.30
No. 19	35.55	0.38	16.58	1.08	0.59	40.60	3.36	1.82	....
No. 21	36.55	0.45	18.97	1.08	0.43	39.00	2.86	1.14	0.20
No. 22	33.70	0.48	16.02	2.74	0.56	34.05	6.25	1.07	4.60
No. 23	37.80	0.46	18.28	0.94	0.57	37.60	2.73	1.37	0.40
No. 24	34.70	0.30	15.00	2.74	0.99	40.05	3.95	1.58	0.90
No. 25	39.10	0.40	10.63	1.00	0.61	40.75	4.98	1.23	1.10
No. 26	34.90	0.48	14.10	1.44	0.41	41.30	4.25	1.48	2.40
No. 27	33.80	0.60	9.86	2.88	0.29	42.60	3.19	1.61	5.95
No. 28	33.65	0.88	16.00	2.02	0.44	32.40	9.56	1.58	3.60
No. 29	39.60	0.36	8.82	0.94	0.43	41.60	6.55	1.06	1.10
No. 30	48.05	0.46	19.02	2.59	1.29	16.60	11.64	0.12	0.50
No. 31	38.15	0.24	7.33	1.22	0.62	40.30	9.72	0.55	1.75
No. 32	32.95	1.32	16.64	2.95	0.51	32.90	9.63	1.06	2.20

A chemical analysis was made of the slag from each plant to determine whether there were any wide variations in chemical composition and their effect upon the physical properties of the material. Table III contains the compiled data from these tests.

A determination of the weight per cubic foot of crushed slag was made of each size: $\frac{1}{4}$ to $\frac{1}{2}$-in., $\frac{1}{2}$ to $\frac{3}{4}$-in., $\frac{3}{4}$ to 1-in., 1 to $1\frac{1}{2}$-in., and a uniformly graded sample consisting of 25 per cent by volume of $\frac{1}{4}$ to $\frac{1}{2}$-in., 25 per cent of $\frac{1}{2}$ to $\frac{3}{4}$-in., 17 per cent of $\frac{3}{4}$ to 1-in., and 33 per cent of 1 to $1\frac{1}{2}$-in. material.

The method used in determining the weight per cubic foot is as follows: The sample of crushed slag which had previously been screened to the required size was placed in three layers in a tared, ½-cu-ft. cylindrical measure. Each layer was tamped twenty-five times with a ⅝-in. steel rod, 18 in. long, having a bullet-shaped point. After the top layer had been tamped, the measure was filled to overflowing and then struck off with the rod. Two determinations were made on each sample of the individual sizes and three on each composite grading. Whenever there was a difference of more than ½ lb. in the two determinations, an additional weight was obtained and an average of the readings taken. In very few cases did the two determinations on a given sample vary more than ½ lb.

TABLE IV.—UNIT WEIGHTS OF AGGREGATES FROM VARIOUS PLANTS, LB. PER CU. FT.

Plant	¼ to ½-in. Size	½ to ¾-in Size	¾ to 1-in. Size	1 to 1½-in. Size	"C" Grading
No. 1	72.2	73.5	73.4	68.0	76.6
No. 2	74.4	79.6	80.4	79.1	85.4
No. 3	76.8	81.3	81.8	79.8	88.4
No. 4	89.6	88.6	89.7	88.5	96.2
No. 8	70.8	74.0	72.7	74.4	79.4
No. 9	70.6	76.8	73.7	67.4	78.4
No. 10	77.8	87.0	85.0	85.1	92.6
No. 13	80.8	78.2	77.4	76.6	83.5
No. 15	80.8	84.0	81.8	82.1	88.4
No. 16	81.4	83.3	80.8	78.4	86.4
No. 17	80.1	84.2	77.6	74.4	85.6
No. 21	76.7	81.0	79.2	82.0	89.2
No. 23	61.9	66.6	68.2	73.8	72.9
No. 26	92.5	83.4	80.6	75.4	90.6
No. 25	93.0	94.8	92.1	81.0	95.1
No. 27	86.4	84.1	83.1	77.4	90.6
No. 28	83.7	81.2	81.4	78.8	87.8
No. 29	81.0	80.1	79.3	76.1	86.8
No. 30	74.7	79.6	82.6	87.8	87.7
Pottstown "A"	86.2	85.8	84.2		89.3
Wharton "B"	83.4	83.6	80.2		86.3
Composite	78.4	82.0	79.3	79.6	84.6
Gravel	99.9	101.9	104.0	103.5	110.5
			105.0		
Limestone	92.8	94.8	94.0	98.3	102.5
			99.1		

Potomac River sand, weight per cubic foot	111.2 lb.
Slag sand, weight per cubic foot	91.8 lb.
Granulated slag, weight per cubic foot	44.6 lb

The author expected to find that the unit weight obtained with a cylindrical measure of ½-cu-ft. capacity would be different from that found when using a cubical measure of 1-cu-ft. capacity. Comparative tests were tried with the two types of measures on the same samples. Tamping and shaking the material to refusal was also tried. It was found that with the material below 1½-in. in size, the cylindrical measure gave as consistent and correct results as the 1-cu-ft. box and that the rod method of compacting material was equal to shaking the material to refusal.

Table IV gives the weight per cubic foot of all the sizes of slag, limestone and gravel used in the proportioning of the coarse aggregate and also the weight per cubic foot of the fine aggregates used. A comparison of the unit weights of slag as determined in the field with those obtained in the laboratory shows a little variation. It will be noted, however, that the weight is dependent upon the size of the material. In general, the smaller sizes weigh more per cubic foot than the larger sizes and a slag of uniform grading gives the highest unit weight.

This data proves that in specifying the unit weight of slag the limits of size must be stated. The average requirement of weight in existing specifications is 70 lb. per cubic foot. Every test on uniformly graded slag met this requirement. Only five tests on individual sizes show weights below this limit. A specification requiring a unit weight of 70 lb. for uniformly graded aggregate would be satisfactory and could be met by every plant without difficulty.

CONCRETE TESTS

A series of concrete tests was made which included six 6 by 12-in. compression cylinders and five 8 by 8 by 5-in. Talbot-Jones rattler specimens for the slag from each plant, the slag being used as coarse aggregate. The mix was proportioned by volume, using one part of cement, two parts of Potomac River sand, and three parts of slag. The sand showed the following grading and mortar strength tests:

Retained on ¼-in.	2 per cent
Passing ¼-in., retained on 10-mesh	33 "
" 10-mesh, " " 20-mesh	19 "
" 20-mesh, " " 30-mesh	13 "
" 30-mesh, " " 50-mesh	17 "
" 50-mesh, " " 100-mesh	9 "
" 100-mesh, " " 200-mesh	3 "
" 200-mesh	4 "
Total	100 "

	STANDARD OTTAWA SAND	SAMPLE SAND
Tensile Strength 1:3 briquettes, 7-day	295 lb. per sq. in.	401 lb. per sq. in.
	300 "	407 "
	305 "	390 "
	300 "	399 "
Strength Ratio		133 per cent

	STANDARD OTTAWA SAND	SAMPLE SAND
Tensile Strength 1:3 briquettes, 28-day	422 lb. per sq. in.	484 lb. per. sq. in.
	405 "	460 "
	360 "	515 "
	396 "	486 "
Strength Ratio		123 per cent

Character of material: Subangular grains of quartz, sandstone and mica with a little clay.

Organic matter: Satisfactory.

Silt and clay: 2.6 per cent.

The cement for this series was prepared by thoroughly mixing together 15 sacks of a standard brand of portland cement and storing it in galvanized iron cans until used. The tests on the cement are given in Table V.

In preparing the coarse aggregate, the material was uniformly graded by combining 25 per cent by volume of $\frac{1}{4}$ to $\frac{1}{2}$-in. size, 25 per cent of $\frac{1}{2}$ to $\frac{3}{4}$-in., 17 per cent of $\frac{3}{4}$ to 1-in., and 33 per cent from 1 to $1\frac{1}{2}$-in. size. The proportions were by volume but the amount was determined by weight. Coarse aggregate for the first four mixes from each plant was obtained by weighing out the proper amount of the composite slag used in making the determination of weight per cubic foot of uniformly graded aggregate to make one mix.

The procedure used in the fabrication and testing of specimens was that adopted by the American Society for Testing Materials[1] or the American Association of State Highway Officials.

Each specimen molded represented a single mix of concrete composed of aggregates weighed separately to insure uniform proportions of cement, fine aggregate and a uniform grading of the coarse aggregate. No two like specimens were made with material from the same plant, on the same day, that is, one block and one cylinder only were made in one day. The sand and cement were mixed dry until of a uniform color and then coarse aggregate was added and mixed. Mixing was performed by two operators using square ended 8-in. trowels and galvanized mixing pans. Each operator mixed separate mixes, alternating for cylinders and wear blocks.

The consistency of the mix was kept as uniform as possible by use of both the slump test and the flow test. The flow test, being considered the more sensitive of the two, was made first and a slump test on the same batch immediately afterward. The amount of water used was measured with a 2000-cc. graduated cylinder. A crater of the dry, well-mixed aggregate was formed and the first 1000 cc. of water added. Additional water was added during mixing until the proper consistency was obtained.

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[1] Tentative Methods of Making Compression Tests of Concrete (C 29 – 21 T), *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 579 (1921).

Cylinders were molded by placing the concrete in the molds in three layers of 4 in. each and tamping 25 times for each layer with a $\frac{5}{8}$-in. round steel rod, 21-in. long, having a bullet-shaped point. Blocks for the Talbot-Jones wear test were made in two layers and tamped 25 times per layer with the addition of puddling the sides and smoothing the surface of the block with the trowel. After the cylinders had finished shrinking but before the set, a top capping of neat cement paste was added and a glass plate worked down to the top of the mold to give a plane surface for testing.

TABLE V.—CEMENT TESTS

Specific gravity	3.11
Retained on 200-mesh sieve	16.6 per cent
Steam test for soundness	O. K.
Time of set (Gillmore needle):	
Initial	2 hr. 40 min.
Final	4 hr. 30 min.

Tensile strength (1:3 Ottawa sand), lb. per sq. in.:

	Sample				Blend
	A	B	C	D	
	AGE, 7 DAYS				
	276	285	250	264	290
	....	270	280	240	267
	240	261	245	254	268
Average	258	272	258	253	275
	AGE, 28 DAYS				
	374	341	375	373	340
	326	316	323	305	...
	310	353	342	348	333
Average	337	337	347	342	336

The molds were removed after 20 hours and the specimens marked, weighed and stored in damp sand. Two days before the date for testing, the blocks were removed from the sand and weighed. They were allowed to dry in the air until tested. The cylinders were not removed from storage until the date tested and after weighing were kept damp by covering with wet burlap bags.

Compression Tests.—Tests for modulus of elasticity and ultimate strength of concrete under compression were made on three of the six cylinders representing each plant at the age of 28 days. (The use of a faulty compressometer made the 28-day modulus tests unreliable and these are not reported.) Cylinders of gravel and limestone aggregate were tested at the same age for comparison.

Previous to testing, the diameter of the cylinder was measured in two directions at right angles, and the mean diameter was used in computing the cross-sectional area. The height of the cylinder was also taken to compute the volume and weight per cubic foot of the concrete.

The cylinder to be tested was placed with its axis vertical, having one end resting on the plane surface of an 8-in. cylindrical steel block. On the upper surface was placed a spherical bearing block 6 in. in diameter against which the moving head of the testing machine acted. The loads were applied at a machine speed of 0.0673 in. per minute.

TABLE VI.—COMPRESSION TESTS FOR THE COMPARISON OF FINE AGGREGATE

Mix: 1:2:3:
Coarse Aggregate: Slag $\frac{1}{4}$ to 1 in.

	Compressive Strength, lb. per sq. in.					
	Potomac River Sand		Slag Sand		Granulated Slag	
	28 days	6 months	28 days	6 months	28 days	6 months
	2555	4374	2830	2800	2164	2135
	2425	4320	2152	1985	1923	1871
	2840	4275	1823	2280	1542	2160
	2700	4510	1763	2710	1422	2006
	....	4280	....	2265	....	2160
Average	2630	4351	2142	2368	1763	2066

Talbot-Jones Rattler Test.—All five specimens of 8 by 8 by 5-in. blocks corresponding to the compression cylinders in this series were tested at the age of 28 days. As only one block representing any one plant was made on a given day, the ten blocks which formed the ring for each run represented coarse aggregate from ten plants.

The rattler tested twenty blocks on each run and was in operation every other day to allow a full set of blocks to be tested each time. The charge consisted of 200 lb. of cast-iron spheres, 10 of which were $3\frac{3}{4}$ in. in diameter and the rest $1\frac{7}{8}$ in. in diameter, said spheres conforming to the specifications for spheres used in the standard rattler test of paving brick. The test consisted of exposing the surfaces of the blocks to the action of the shot through 1800 revolutions of the rattler at about 30 r.p.m. The machine was run 900 revolutions in each direction. The blocks had each been weighed and measured previous to testing and by weighing after testing, the loss in grams was determined. This is converted in turn to a percentage loss and then to inches of wear, basing the computations on an initial height of 5 in.

SLAG AS FINE AGGREGATE

It was thought that possibly the product of the slag crushing and screening plants which passed $\frac{1}{4}$-in. screens would be suitable as a fine aggregate for concrete when crushed slag was the coarse aggregate. Also, granulated slag which is produced by pouring molten slag into water might prove satisfactory when used as a fine aggregate. Table VI plainly shows that sand is superior to slag screenings or granulated slag. The absence of sufficient fine material in the slag screenings causes a very porous concrete not wholly unlike that which would be produced with cement and coarse aggregate alone. A concrete roadway using this material as a fine aggregate would be very difficult to finish to a smooth tight surface. This porosity may account in a large measure for the low strengths obtained in compression tests of concrete cylinders with slag screenings as the fine aggregate.

TABLE VII.—RESULTS OF TALBOT-JONES RATTLER TEST FOR COMPARISON OF FINE AGGREGATE

Mix: 1:2:3
Coarse Aggregate: Slag $\frac{1}{4}$ to 1 in.

	Loss, per cent					
	Potomac River Sand		Slag Sand		Granulated Slag	
	28 days	6 months	28 days	6 months	28 days	6 months
	11.2	8.79	12.6	8.92	15.9	11.42
	13.3	8.33	13.6	8.89	15.3	13.26
	12.3	8.90	13.7	8.86	14.8	13.80
	12.8	10.20	14.9	10.05	14.9	16.10
	15.7	.....	14.0	10.45	13.3	14.95
Average......	13.1	9.06	13.9	9.43	14.8	13.91

The granulated slag, although sufficiently fine in size, does not make a good fine aggregate for two reasons: First, the grains themselves are weak and brittle, they may be pulverized between the fingers and contain a high percentage of voids. The particles of this granulated slag resemble sponges of silica having thin glassy walls and many vesicular cavities. This structure is responsible for the light unit weight and high void space. Secondly, the glassy nature of the material prevents it from retaining the surface moisture necessary for smooth mixing and good bond between cement and fine aggregate.

The concrete specimens made with granulated slag as a fine aggregate lost a large amount of the mixing water immediately after being molded. The specimens also had the appearance of being made with saw dust as an aggregate.

TABLE VIII.—RESULTS OF COMPRESSION AND WEAR TESTS

Mix: 1:2:3
Cylinders: 6 by 12 in.
Blocks: 8 by 8 by 5 in.
Age: 28 days
Average of 3 to 5 Specimens

Plant	Aggregates	Compression Test		Talbot-Jones Test	
		Average Strength, lb. per sq. in	Weight per Cubic Foot, lb.	Wear, per cent	Weight per Cubic Foot, lb.
No 1	Slag—river sand	3359	139	11.2	139
No. 2	" " "	3316	138	10.1	139
No. 3	" " "	3510	140	9.2	138
No. 4	" " "	3219	144	8.6	146
No. 8	" " "	3353	136	10.1	136
No. 9	" " "	3223	136	8.5	137
No. 10	" " "	3159	141	10.3	142
No. 13	" " "	3422	139	7.0	140
No. 15	" " "	3547	142	9.5	141
No. 16	" " "	3565	142	9.8	141
No. 17	" " "	3265	141	8.3	140
No. 21	" " "	3069	138	9.2	136
No. 23	" " "	3051	132	10.8	135
No. 25	" " "	2936	143	8.7	143
No. 26	" " "	3064	140	8.9	144
No. 27	" " "	3231	141	9.2	145
No. 28	" " "	3656	141	9.3	142
No. 29	" " "	3409	137	9.3	141
No. 30	" " "	2906	140	11.1	140
G	Gravel—river sand	2809	146	11.6	147
L	Limestone—river sand	3301	152	11.8	150
A	Slag, $\frac{1}{4}$ to 2 in	2979	140	9.4	138
B	Slag, $\frac{1}{4}$ to 1 in	3345	142	10.1	138
C	Slag, $\frac{1}{4}$ to $1\frac{1}{2}$ in	2914	138	9.6	137
G–A	Gravel, $\frac{1}{4}$ to 2 in	2648	147	10.6	147
G–B	Gravel, $\frac{1}{4}$ to 1 in	2455	146	11.5	145
G–C	Gravel, $\frac{1}{4}$ to $1\frac{1}{2}$ in	2733	147	10.8	146
L–A	Limestone, $\frac{1}{4}$ to 2 in	2791	133	11.2	148
L–B	Limestone, $\frac{1}{4}$ to 1 in	3001	154	13.8	149
L–C	Limestone, $\frac{1}{4}$ to $1\frac{1}{2}$ in	2931	150	12.6	151
P	Slag—Potomac River sand	2462	134	13.1	140
S	Slag—slag sand	2029	127	13.7	127
G	Slag—granulated slag	1670	121	14.7	121

Table VII shows the resistance to wear of concrete having the three types of fine aggregate. This table shows that the slag sand and the Potomac River sand produced concrete that had nearly equal wearing resistance within the limits of measurement by this type of test. The granulated slag on the other hand showed much greater loss.

Objections to this test as a true indication of resistance to wear will be discussed in another part of this paper. It is sufficient to say that in view of the results obtained in the compression and the wear tests, slag sand and granulated slag are inferior to the average quartz concrete sand.

RESULTS OF CONCRETE TESTS

The results which give the greatest amount of information necessary for the proper conclusions as to the suitability of blast-furnace slag as an aggregate in concrete were found by the tests of specimens representing the aggregates from the various plants.

In these tests, everything was held constant except the coarse aggregate. This varied from the very porous, honey-combed material produced by some plants to the black, glassy slags of others. The consistency of the mix was kept constant throughout the entire investigation. A slump of 1 in., corresponding to a flow of 160, was used. All results shown in Table VIII are averages of tests on 3 to 5 specimens made and tested on different days and are the results of the compression and wear tests outlined above. These are represented by the part of the table down to and including limestone from Plant L. The tests on slag from Plants A, B, and C were made to determine the effect of maximum size of coarse aggregate.

Comparing the results of compression tests on concrete containing coarse aggregate from Plants Nos. 1 to 30 with those containing gravel and limestone, the highest strength obtained was 3656 lb. per sq. in. with slag aggregate, and the lowest 2809 using Potomac River gravel. The lowest strength of the slag concrete is 2906, which represents a high-silica, low-lime slag of glassy texture. The strength of limestone concrete is practically equal to the average strength of the slag concrete. The results of the wear tests show a lower percentage of wear for slag concrete than for either gravel or limestone. The differences, however, are small in most instances, and the test does not warrant any specific conclusion other than the fact that the slag concrete is as satisfactory as gravel or limestone concrete when tested by the Talbot-Jones rattler.

The lower part of the table contains results of tests on slag, gravel, and limestone concrete, when the variable was the maximum size of the coarse aggregate used. Group A refers to ¼ to 2-in. aggregate, B is ¼ to 1 in., and C from ¼ to 1½ in., all being proportioned between the sieves in amounts necessary to give a straight-line grading. The results seem to indicate that the ¼ to 1-in. slag and limestone make the strongest concrete, while the reverse is true for the gravel used.

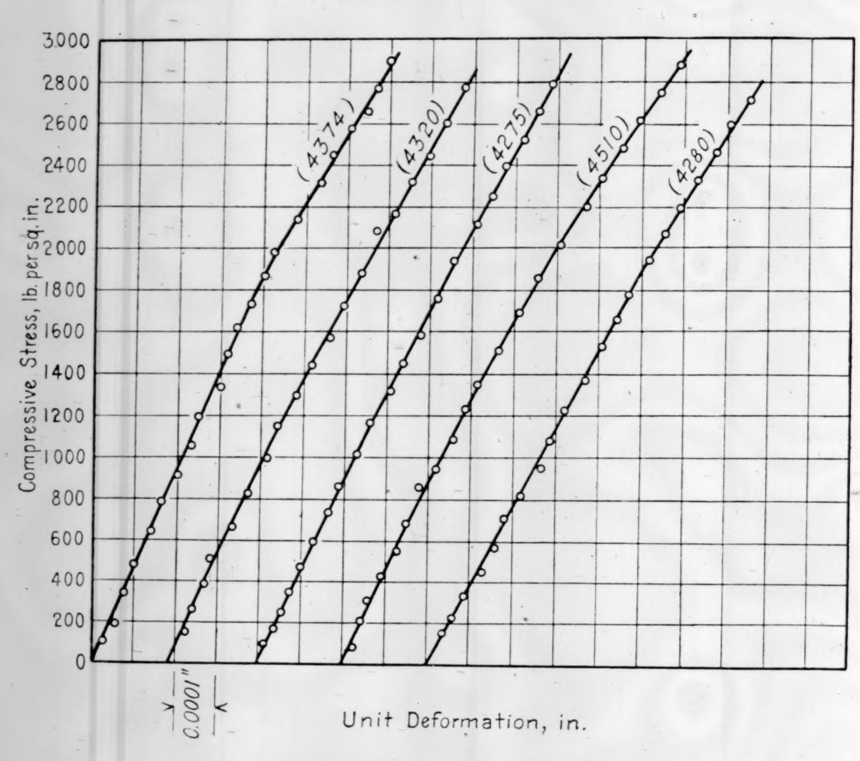

FIG. 2.—Modulus of Elasticity of Slag Concrete
1:2:3 Slag Concrete, Age 6 months

The slag shows a strength equal to that of limestone and superior to the gravel in every case.

As in the preceding results, less wear was found for the slag concrete than for the concrete containing either gravel or limestone. The wear tests results show the strongest slag concrete to have a greater loss in wear than the weaker concrete. The ¼ to 1-in. slag concrete (the strongest slag concrete), however, still shows less loss than either the gravel or limestone concrete of any sizes of aggregate.

CONTROL SPECIMENS, CIRCULAR TRACK WEAR INVESTIGATION

An investigation of the suitability of many doubtful aggregates has been in progress for some time, consisting of circular track wear tests. The results of tests on the control specimens, made in connection with this investigation, throw some additional light on the relative value of slag as an aggregate in concretes. At the time the test sections of this experimental pavement were placed, three compression cylinders, three Talbot-Jones wear blocks and two 6 by 8 by 48-in. plain concrete beams were molded for each type of surface.

Tests were made on these specimens at the age of ninety days, after being cured two weeks under a damp earth covering and the remaining period in air. The cylinders and blocks were tested in the regular manner. The beams were tested with a center-point loading and a span of 42 in. Deformation and deflection measurements were obtained from the cylinders and beams, respectively, during the tests. The deformations in compression were obtained with a two-ring, two-dial compressometer having several improved features which eliminate initial lag in showing the deformation under very small loads. Fig. 2 shows the curves of the stress-strain relation for five 6 by 12-in. compression cylinders of slag concrete—composed of slag and Potomac River sand—which were tested at the age of six months.

TABLE IX.—RESULTS OF TESTS OF CONTROL SPECIMENS—CIRCULAR TRACK WEAR INVESTIGATIONS

Mix: 1:1½:3
Fine Aggregate: Potomac River sand
Slump: 1 in.
Time of mix: 1 minute
Age: 90 days

Each value in the compressive strength and modulus of elasticity in compression is an average of 3 cylinders; modulus of rupture and modulus of elasticity in bending is the average of 2 beams; wear tests are the average of 3 Talbot-Jones wear blocks

Section of Track	Coarse Aggregate	Compressive Strength, lb. per sq. in.	Modulus of Rupture, lb. per sq. in.	Wear, per cent	Modulus of Elasticity, lb. per sq. in.	
					Compression	Bending
1	Martinsburg limestone	4668	480	3.48	5 100 000	4 735 000
11	Potomac River gravel	4163	527	7.73	3 300 000	3 265 000
20	Martinsburg limestone, $\frac{1}{4}$ to $1\frac{1}{2}$ in.	3902	600	6.52	5 150 000	4 310 000
35	Blast-furnace slag, $\frac{1}{4}$ to $2\frac{1}{2}$ in.	4827	589	5.23	4 450 000	5 020 000
36	Blast-furnace slag, $\frac{1}{4}$ to $1\frac{1}{2}$ in.	4987	690	5.48	5 000 000	4 545 000
37	Blast-furnace slag, $\frac{1}{4}$ to $\frac{3}{4}$ in.	4772	597	4.29	4 600 000	4 030 000
38	Blast-furnace slag, $\frac{1}{4}$ to $1\frac{1}{2}$ in., with slag sand	4827	773	4.00	3 950 000	3 600 000
39	Blast-furnace slag, hand picked, 56 lb. per cu. ft.	4333	729	5.73	4 350 000	3 525 000

A few of the results of the control tests made for the above-mentioned investigation are given in Table IX. Only those values which represent the concretes containing slag, limestone or gravel are included. These results bear out the conclusions derived from the other investigations. They show slag concrete to be superior to the gravel concrete and also to the limestone concrete used in these compression and transverse-bending tests. Slag concrete wear blocks showed a lower percentage of wear than either the limestone or gravel except in the case of Section 1.

Attention is directed to the result of tests under Section 39. The coarse aggregate in this case was blast-furnace slag which had been hand picked from the lightest commercial slag available. In selecting this material, only the honey-combed pieces were included. The unit weight was only 56 lb. per cu. ft. when the material was uniformly graded from $\frac{1}{4}$ to $1\frac{1}{2}$ in. This represents the type of slag which is the cause of most of the unfavorable comment regarding its use as an aggregate in the wearing surface of concrete pavements.

An analysis of the test results on this material shows that the strength and wearing quality is not greatly impaired by the lightness or porous nature of the aggregate.

SUMMARY

The data presented do not indicate any new development in the knowledge about slag used in concrete. They merely represent the work of unbiased operators endeavoring to find the value of slag as compared to two other commonly used concrete aggregates, namely, limestone and gravel. The results of the tests made in this investigation confirm those made by several other investigators whose reports have been published from time to time.

Specimens have been made for testing at the age of one, two and five years. A series of corrosion tests have been planned to determine the relative corrosive effect of slag, gravel and limestone aggregates upon reinforcing steel.

The following general conclusions are interpreted from the data resulting from this investigation:

1. The unit weight of blast-furnace slag as produced commercially has little if any effect upon the structural strength or the wearing property of the resulting concrete.

2. A limit of 70 lb. per cu. ft. would admit practically every well-graded crushed slag product.

3. The gravel used in these tests is one of accepted quality and is considered to be of average strength. The limestone used for comparison with the slag is somewhat superior to the average limestone used for concrete. The strength of all of the slag concretes

exceeded that of the gravel concrete. The average strength of the slag concretes is equal to the strength of the limestone concrete.

4. Because of the excessive chipping at the edges of the wear blocks, the wear test is not considered a very favorable indication of the resistance of concrete to wear. It is a means of comparing the value of various types of concrete subjected to the action of the shot, and is interpreted as such by the author. On this assumption, the wear-resisting properties of slag concrete are equivalent to those of the limestone concrete and superior to those of gravel concrete tested in comparison.

5. The physical properties of the slag when tested by the usual rock tests do not seem to be an important factor in determining the quality of the concrete made with slag as the coarse aggregate.

DISCUSSION

Mr. Mattimore MR. H. S. MATTIMORE.[1]—There is just one point regarding slag concrete that I believe has been overlooked in the investigation by Mr. Harsch. I refer to comparative absorption of concrete using dense stone and slag as coarse aggregate, respectively. I have been interested in this phase of the question from the standpoint of the volumetric change in different kinds of concrete after being laid. The author has performed absorption tests on the slag itself and obtained high values; I believe this should be followed out by absorption tests on the slag concrete itself compared with concretes using a stone and gravel aggregate. This may give us some line of investigation to follow regarding cracking on concrete road slabs.

Such few tests as I have made on the absorption of slag concrete indicate that it is higher than the absorption of a good limestone concrete.

Mr. Woolson MR. IRA H. WOOLSON.[2]—I should like to ask Mr. Harsch if he found that in his judgment it was necessary to make any variation in the amount of mortar used in making the concrete when he used the very highly honey-combed aggregate.

Mr. Harsch MR. RAYMOND HARSCH.—The fact that an excess of mortar was needed was noticed in construction work and the mix was so designed as a 1:2:3 mix in this investigation to take care of that, but the concrete that was put in the circular track wear sections was of a 1:1½:3 mix and the tests show it to be very satisfactory. It is more difficult to work slag concrete smoothly if there is not an excess of mortar present.

Mr. Woolson MR. WOOLSON.—That naturally adds to the expense of that particular type, does it not?

Mr. Harsch MR. HARSCH.—Yes.

Mr. Howe MR. HENRY L. HOWE, JR.[3]—I should like to ask the author if he has made any experiments to show the relative strength of slag concrete made from dry slag and from slag wetted before it is mixed?

Mr. Harsch MR. HARSCH.—No, we have not.

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[2] Consulting Engineer, National Board of Fire Underwriters, New York City.

[3] Mechanical and Electrical Engineer in Charge of Municipal Testing Laboratory, Department of Engineering, City of Rochester, N. Y.

Mr. Howe

Mr. Howe.—In some of our experiences with slag in pavement base, we found that it absorbed water from the concrete mixture very badly, causing surface checking. We conducted a few experiments in our Municipal Testing Laboratory, and found that there was considerable increase in the compressive strength of slag concrete due to wetting the slag in order to fill its pores, before it was mixed with sand and cement. I wonder if any work has been done that checks our results?

Mr. Harsch

Mr. Harsch.—The specimens were buried in damp sand immediately after removing the molds, the morning after they were made. They were weighed before and after storage and it was found that they absorbed approximately a pound of water apiece by remaining 28 days in damp sand. In other words, a 25-lb. specimen was about one pound heavier at the end of 28 days storage due to the absorption of water from the damp sand.

Mr. Yeoman

Mr. R. C. Yeoman.[1]—In proportioning the cement and aggregates for these tests, was the yield for each mixture determined?

Mr. Harsch

Mr. Harsch.—Yes, we measured the yields and found them to be between 1 and 1.2.

Mr. Yeoman

Mr. Yeoman.—Was the yield different for the angular aggregates than for the rounded aggregates?

Mr. Harsch

Mr. Harsch.—We did not find enough difference between the yield values to draw any conclusions. The variations between individual tests of the same aggregate were larger than the variations noticed between the aggregates.

Mr. Mattimore

Mr. Mattimore.—In defense of slag concrete, I should like to say that the effect that Mr. Howe noticed on concrete slabs is not characteristic of slag concrete. I have experienced this checking and cracking in concrete road slabs where aggregates other than slag were used.

Mr. Goldbeck

Mr. A. T. Goldbeck.[2]—I should like to bring out the fact that this investigation also will include the matter of expansion and contraction due to moisture absorption and to drying out, and will also include tests for the corrosion of steel in concrete of various kinds.

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[1] Extension Engineer, Indiana Sand and Gravel Producers' Association, Indianapolis, Ind.

[2] Chief, Division of Tests, U. S. Bureau of Public Roads, Washington, D. C.

A PENETRATION TEST FOR THE WORKABILITY OF CONCRETE MIXTURES WITH PARTICULAR REFERENCE TO THE EFFECTS OF CERTAIN POWDERED ADMIXTURES

J. C. Pearson[1] and F. A. Hitchcock[2]

Introduction

It is not easy to define the terms "workability," "plasticity," "consistency," etc., as applied to concrete mixtures. Some authorities use these terms more or less synonymously;[3] others prefer to distinguish between workability and consistency,[4] considering the latter as a property directly related to water content and measurable by the slump, flow, and other tests, whereas workability is regarded as more intrinsically a characteristic of a given mixture, dependent not only on the water content, but essentially upon the other ingredients of the mixture. We incline to this latter view, and without attempting to give an exact definition of workability, we consider one concrete mixture more workable than another when the processes of mixing, handling and placing are accomplished with less effort in the one case than in the other. The direct quantitative measurement of this effort, for example, in placing a concrete mixture in any sort of a form or mold is a very difficult thing, chiefly because there is no telling when one arrives at a definite condition that may be taken as a stopping point in the placing operation. We have made many attempts to measure the workability of various concrete mixtures in this way, but thus far without success.

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[1] Physicist, U. S. Bureau of Standards, Washington, D. C.

[2] Associate Engineer Physicist, U. S. Bureau of Standards, Washington, D. C.

[3] See the Tentative Specifications for *Workability* of Concrete for Concrete Pavements (D 62-20 T), *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part I, p. 692 (1920); *Workability* or *Relative Consistency*, D. A. Abrams, "Effect of Hydrated Lime and Other Powdered Admixtures in Concrete," *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part II, p. 156 (1920); *Consistency* or *Relative Plasticity*, Progress Report of the Joint Committee on Standard Specifications for Concrete and Reinforced Concrete, Section 2, Definitions, *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 221 (1921); Slump Test for *Plasticity*, Report of Committee C-9 on Concrete and Concrete Aggregates, *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part I, p. 304 (1920); *Mobility* or *Flowability*, F. E. Giesecke, *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part II, p. 228 (1920); *Mobility* or *Workability*, A. N. Talbot, *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 992 (1921); *Consistency* or *Mobility*, A. N. Talbot, *Proceedings*, Am. Soc. Testing Mats., Vol. XIX, Part II, p. 482 (1919).

[4] Discussion on Distinction between *Consistency* or *Flowability*, and *Workability*, by Watson Davis, *Proceedings*, Am. Soc. Testing Mats., Vol. XX, Part I, p. 443 (1920); J. C. Pearson, *Proceedings*, Am. Soc. Testing Mats., Vol. XIX, Part II, p. 414 (1919).

During the past two or three years we have given a great deal of thought to the possibilities of measuring the workability of concrete by some indirect method. If one has had considerable experience in handling concrete mixtures in the laboratory, one gets the impression that those mixtures are the most workable which tend to preserve their homogeneity in greatest degree as they are being handled and placed in forms. Thus a relation is suggested between the workability of a mixture and its tendency to segregate, and we felt that the development of a satisfactory method of measuring segregation would help to solve the problem. After trying and discarding numerous schemes we developed a direct segregation test which was briefly as follows:

A concrete mixture was brought by trial to a given consistency (flow) and then placed in a 6 by 12-in. cylindrical mold made of two 6 by 6-in. sections, the lower section having a water-tight bottom. The mold was fastened upon the flow table, and the latter was raised and dropped for a definite number of times, causing the concrete to settle in the mold. The amounts of coarse aggregate in the upper and lower sections of the mold were then determined by washing the fine material through a No. 3 sieve, and weighing the residues. Comparing different concretes in this manner, we found that large differences in segregation were clearly indicated, but the uncertainty of the determination was such that small differences were obscured. After doing a great deal of work in the endeavor to improve the accuracy of this test, we discarded it with a feeling that some simpler solution must be found.

In the course of the segregation experiments the impression grew upon us that the most noticeable characteristic of the harsher working mixtures was a greater tendency to settle and pack in the container—this was noticeable even in the mixing pan and on the flow table, as well as in the segregation mold. While this tendency to pack seemed to be due to segregation, the test described above indicated that segregation was not the only factor involved. Thus we were led to devise some comparative measure of the packing, and following the usual procedure of starting with complex schemes and proceeding to the simple, we eventually came to the penetration test which is the subject of this paper.

THE PENETRATION TEST

Apparatus.—The apparatus used in the penetration test is shown in Fig. 1. *M* is a 6 by 12-in. pipe mold with a water-tight bottom, fitted with slotted lugs, *L*, by means of which the mold is fastened to the small drop table *T*. The latter is in reality an ordinary flow table with a special top carrying two clamp nuts, *B*, which engage the lugs, *L*, of the pipe mold. The table top can be raised and dropped through a height of $\frac{1}{8}$ in. by means of a cam on the end of the shaft *P*. Fitting the top of the pipe mold closely is a detachable brass spider *F* carrying a fixed sleeve *S*, which, when the frame is in position on top of the mold, is exactly in alignment with the axis of the mold. The sleeve *S* is $\frac{3}{4}$ in. inside diameter and 6 in. long. A steel rod, *R*, $\frac{3}{4}$ in. diameter, 20 in. long, weighing 1100 g. completes the apparatus. The lower end of the rod *R* is hemispherical, and the upper portion is graduated in 0.1 in. in such manner as to read directly the depth of the lower end below the top of the mold when inserted in the sleeve *S*.

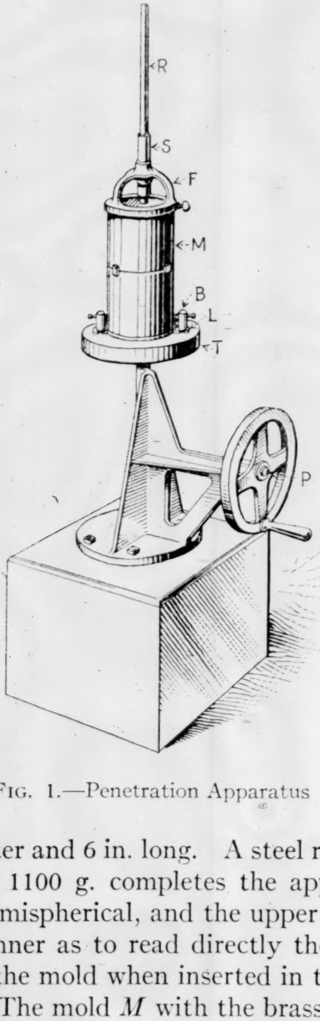

FIG. 1.—Penetration Apparatus

Method of Test.—The mold *M* with the brass spider *F* clamped to the top is fastened to the table by means of the clamp nuts. The batch of concrete to be tested is scooped into *M* until the latter is filled to the brim. No attempt is made to puddle the concrete into the mold, but a rod is used to spread the concrete uniformly as it falls from the scoop. After the mold is filled, the table top is raised and dropped a given number of times to subject the concrete to a definite amount of compacting force. Our practice throughout has been to give the concrete thirty $\frac{1}{8}$-in. drops. At the completion of the preliminary jolting or settling, the rod R is inserted in the sleeve S and lowered gently into the concrete until it comes to rest under its own weight. The table is then raised and dropped, and from the force of the impact the rod penetrates a certain distance into the concrete, indicated by the reading of the scale. The impacts are repeated and the successive penetrations of the rod are noted until the latter has reached nearly to the bottom of the mold. In practically all of our tests the penetration has been carried to a depth of 11 in.

In order to obtain comparable results in the penetration tests, we felt it necessary to use the same grading of coarse aggregate in all mixtures. This aggregate was selected from the results of preliminary experiments, and was composed of Potomac River gravel, 50 per cent $\frac{1}{4}$ to $\frac{1}{2}$ in., and 50 per cent $\frac{1}{2}$ to $\frac{3}{4}$ in. We should have preferred to use coarser material as more typical of commonly used aggregates, but it is obvious that the penetration of the rod into the concrete involves the displacement of aggregates in an annular space of $2\frac{5}{8}$ in., if anything too small, rather than too large, for the free movement of the aggregate. We did not wish to increase the size of the mold, and consequently the size of the batches, unless it seemed necessary to do so; on the other hand, the aggregate as selected was entirely suitable for certain types of reinforced concrete in which the working qualities are perhaps of greatest importance. It was especially important also that the sand be as uniform as possible throughout the tests wherein the effects of other variables were being studied. In order to insure the uniformity of the sand without going to the very great labor of screening and recombining in definite proportions, the entire lot (about five tons) was mixed on the floor of the laboratory in a damp condition, sacked in that condition and allowed to dry in the sacks. When the mixing was completed, five widely separated samples were taken for sieve analysis. The results of the sieve tests are given in Table I. The very close agreement in the sieve analyses shows that the method is quite satisfactory. In weighing out the batches, the sand was scooped from the sacks, thus minimizing any tendency toward segregation in the individual sacks.

Significance of the Test.—Even if one has never seen or used the penetration test it is quite obvious that different mixtures should show different resistances to the penetration of the rod, the more plastic mixtures allowing the rod to penetrate readily, the harsher mixes offering high resistance. As stated in the introduction, the property that we were admittedly trying to measure was the tendency of a mixture to pack under a definite amount of settling or compacting. This we seemed to have accomplished, judging from the results of a considerable number of preliminary experiments, but a more complete interpretation was lacking. It was primarily to furnish an interpretation of this test that the present investigation was undertaken; incidentally the investigation was so planned as to contribute some interesting information regarding the effects of certain powdered admixtures.

TABLE I.—MATERIALS USED IN THE PENETRATION TESTS

<table>
<tr><th>Material</th><th>Weight per Cubic Foot, lb.</th><th>Specific Gravity</th><th colspan="9">Gradation</th></tr>
<tr><td colspan="12">AGGREGATES</td></tr>
<tr><td>Potomac River Gravel</td><td>101.2</td><td>2.65</td><td colspan="9">¼ to ½ in., 50 per cent
½ to ¾ in., 50 per cent</td></tr>
<tr><td rowspan="8">Potomac River Sand</td><td rowspan="8">109.1</td><td rowspan="8">2.65</td><td rowspan="2">Sample</td><td colspan="6">Retained on Sieve, per cent</td><td rowspan="2">Fineness Modulus</td><td rowspan="2">Surface Modulus</td></tr>
<tr><td>No. 4</td><td>No. 8</td><td>No. 16</td><td>No. 30</td><td>No. 50</td><td>No. 100</td></tr>
<tr><td>No. 1</td><td>2.5</td><td>15.8</td><td>26.7</td><td>40.4</td><td>70.3</td><td>90.4</td><td>2.46</td><td>30.0</td></tr>
<tr><td>No. 2</td><td>1.9</td><td>15.2</td><td>26.8</td><td>41.0</td><td>71.1</td><td>90.9</td><td>2.49</td><td>29.5</td></tr>
<tr><td>No. 3</td><td>2.2</td><td>15.5</td><td>26.8</td><td>40.8</td><td>71.0</td><td>91.0</td><td>2.47</td><td>29.5</td></tr>
<tr><td>No. 4</td><td>2.2</td><td>15.5</td><td>26.9</td><td>41.0</td><td>71.2</td><td>91.1</td><td>2.48</td><td>29.3</td></tr>
<tr><td>No. 5</td><td>2.1</td><td>15.7</td><td>28.1</td><td>42.5</td><td>72.2</td><td>91.3</td><td>2.52</td><td>28.7</td></tr>
<tr><td>Aver.</td><td>2.2</td><td>15.6</td><td>27.1</td><td>41.2</td><td>71.2</td><td>91.0</td><td>2.48</td><td>29.4</td></tr>
<tr><td>Mixed Aggregate</td><td>120.5</td><td>2.65</td><td colspan="9"></td></tr>
<tr><td colspan="12">ADMIXTURES</td></tr>
<tr><td rowspan="2"></td><td rowspan="2"></td><td rowspan="2"></td><td colspan="9">Percentage finer than</td></tr>
<tr><td colspan="3">0.01 mm.[a]</td><td colspan="3">0.02 mm.[a]</td><td colspan="3">No. 200 Sieve</td></tr>
<tr><td>Cement</td><td>89.4[b]</td><td>3.15</td><td colspan="3">21.0</td><td colspan="3">31.9</td><td colspan="3">82.7</td></tr>
<tr><td>Hydrated Lime</td><td>31.8[b]</td><td>2.25</td><td colspan="3">79.8</td><td colspan="3">95.1</td><td colspan="3">98.8</td></tr>
<tr><td>Kaolin</td><td>34.5[b]</td><td>2.55</td><td colspan="3">63.8</td><td colspan="3">90.0</td><td colspan="3">99.6</td></tr>
<tr><td>Celite</td><td>10.4[b]</td><td>2.00</td><td colspan="3">100[c]</td><td colspan="3">....</td><td colspan="3">....</td></tr>
</table>

[a] Determined by air elutriation

[b] Determined by the method specified for fine aggregates of the Standard Method of Test for Unit Weight of Aggregate for Concrete (C 29 – 21), 1921 Book of A.S.T.M Standards.

[c] Determined by water sedimentation.

RESULTS OBTAINED FROM THE PENETRATION TEST AS APPLIED TO DIFFERENT CONCRETE MIXTURES GAGED TO THE SAME CONSISTENCY—SERIES I

The first series of experiments selected to check the indications of the penetration test was performed on a range of concrete mixtures with and without additions of high-calcium hydrated lime, Delaware kaolin, and "celite," a variety of diatomaceous earth furnished by the Celite Co., of Los Angeles, California. This series involved 25 different concrete mixtures, each tested on a number of different days, and all gaged as nearly as possible to the same consistency as measured by the flow table.[1] The consistency selected for these experiments was such as would be suitable for good reinforced concrete, and gave a flow of 110, corresponding to a cone slump of 7 to 8 in.

The 25 concrete mixtures used in Series I are listed in Table II, together with a summary of the test data. In explanation, it may be stated that the series was first planned and carried out with an arbitrary selection of admixtures, in amounts 5, 10, and 15 per cent, respectively, (by weight of cement) for the 1:3:6 mixtures, and 5 and 10 per cent for the 1:2½:5 and the 1:2:4 mixtures. The unexpectedly large effects produced by the celite rendered it advisable to reduce the quanity of this admixture to one-third of these amounts in order that the effects might be more nearly comparable with those produced by the kaolin and the hydrated lime. The results obtained with the larger additions of celite are listed at the bottom of Table II.

After considerable thought, the proportions of the ingredients of the concretes were selected on a weight basis, partly for convenience, chiefly to insure the use of definite quantities of the admixtures. There is no standard method of determining the weights of unit volumes of fine powders, and such determinations are subject to great variations depending on the method used. However, certain values

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[1] For a description of the flow table as used in this investigation, see "Inundation Methods for Measurements of Sand in Making Concrete," by G. A. Smith and W. A. Slater, *Proceedings*, Am. Concrete Inst., Vol. 19 (1923).

For the information of those interested in details, the method of making the flow test is as follows: The dry ingredients of the concrete are mixed by hand in a shallow pan, using a blunt brick mason's trowel. The water is added at a noted time and the mixing is continued for two minutes. The concrete is spread evenly over the entire bottom of the pan and is then scooped uniformly into the truncated cone flow mold, 6¾ by 5 by 10 in., which is placed on the center of the flow table. The scoop is square ended, in order that each scoopful of concrete may be as nearly representative of the concrete in the pan as possible. This procedure is especially necessary in the case of wet mixes. At exactly 3 minutes from the time of adding the water the flow mold is lifted from the concrete, and the table is then raised and dropped 15 times. The speed is so regulated that any obvious flow in the mass of concrete resulting from a given impact has ceased before the next impact is given. The drop has been reduced to ⅛ in., as it has been found that this gives greater sensitiveness, greater range and greater accuracy in the determination. Four diameters of the mass of concrete after jolting are carefully measured, and the flow is expressed as the percentage increase of the average diameter over the original 10-in. diameter of the mold.

TABLE II.—SUMMARY OF TEST DATA FROM SERIES I

Number	Mix, by Weight	Admixture,[1] per cent by weight of Cement	Mix, by Volume	Admixture, per cent by volume of Cement	Admixture, by absolute volume, per cent	Number of Tests	Water to Dry Materials, per cent[2]	Water-Cement Ratio	Flow[3]	Slump[3]	Compressive Strength at 28 days,[4] lb. per sq. in.	Workability Figure
1	1:3:6	0	1:2.5:5.3	0	0	14	9.9	1.42	111	7.2	1044	183
2	....	1.7 C	....	14.4	2.6	5	10.2	1.46	109	7.5	1155	162
3	....	3.3 C	....	28.7	5.3	5	10.7	1.53	107	7.4	1129	144
4	....	5.0 C	....	43.1	7.9	8	11.1	1.59	110	7.7	1002	84
5	....	5.0 K	....	13.0	6.2	8	10.0	1.43	111	7.4	1068	143
6	....	10.0 K	....	25.9	12.4	8	10.3	1.48	113	8.0	1080	112
7	....	15.0 K	....	38.9	18.5	6	10.8	1.55	110	7.7	1022	123
8	....	5.0 L	....	14.1	7.0	8	9.8	1.40	110	6.9	1222	169
9	....	10.0 L	....	28.1	14.0	8	10.1	1.45	108	7.5	1202	140
10	....	15.0 L	....	42.2	21.0	6	10.5	1.50	116	7.4	1156	140
11	$1:2\frac{1}{2}:5$	0	1:2.0:4:4	0	0	13	9.7	1.18	110	7.5	1583	138
12	....	1.7 C	....	14.4	2.6	5	10.4	1.27	111	7.8	1681	125
13	....	3.3 C	....	28.7	5.3	5	11.1	1.35	108	7.8	1559	91
14	....	5.0 K	....	13.0	6.2	7	10.1	1.23	108	7.3	1474	101
15	....	10.0 K	....	25.9	12.4	7	10.5	1.28	110	7.9	1242	86
16	....	5.0 L	....	14.1	7.0	7	10.0	1.22	110	7.6	1454	141
17	....	10.0 L	....	28.1	14.0	7	10.2	1.24	108	7.4	1410	129
18	1:2:4	0	1:1.6:3.5	0	0	14	9.9	0.99	111	7.8	2307	82
19	....	1.7 C	....	14.4	2.6	5	10.8	1.08	112	8.2	2168	57
20	....	3.3 C	....	28.7	5.3	5	11.6	1.16	110	8.4	2125	48
21	....	5.0 K	....	13.0	6.2	6	10.4	1.04	110	8.0	2018	56
22	....	10.0 K	....	25.9	12.4	6	11.0	1.10	110	7.8	1844	44
23	....	5.0 L	....	14.1	7.0	6	10.1	1.01	109	7.6	2046	97
24	....	10.0 L	....	28.1	14.0	6	10.6	1.06	109	7.7	1974	62
25	$1:1\frac{1}{2}:3$	0	1:1.2:2.6	0	0	6	10.2	0.80	109	8.4	2622	57
1	1:3:6	10.0 C	1:2.5:5.3	86.1	15.8	9	12.6	1.81	108	7.7	936	53
2	....	15.0 C	....	129.2	23.7	5	14.5	2.08	111	7.9	778	30
3	$1:2\frac{1}{2}:5$	5.0 C	1:2.0:4.4	43.1	7.9	7	11.5	1.40	111	7.8	1272	55
4	....	10.0 C	....	86.1	15.8	7	13.4	1.63	109	7.8	1116	43
5	1:2:4	5.0 C	1:1.6:3.5	43.1	7.9	6	12.0	1.20	107	8.0	1866	32
6	....	10.0 C	....	86.1	15.8	6	14.2	1.42	106	8.2	1578	22

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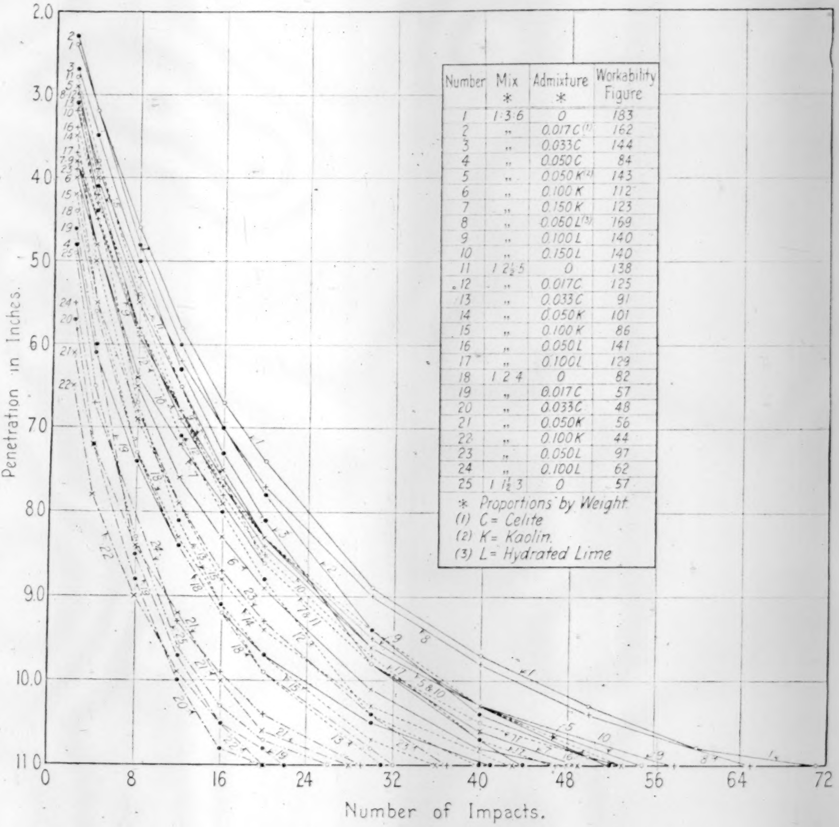
[1] C = Celite, K = Kaolin, L = Hydrated Lime.

[2] Exclusive of Admixture.

[3] Flow figures, the average from all tests; Slump figure, from a single determination.

[4] Strength, the average from 5 tests except for 1:3:6, $1:2\frac{1}{2}:5$ and 1:2:4 plain concrete, in which case the average is from 10 tests.

were obtained by using the method prescribed for measuring fine aggregates, and on the basis of these values, the proportions by volume were determined and are included in Table II. As there are certain arguments for comparing the effects of different admixtures on the basis of absolute volumes, the proportion of cement to admixture on this basis for each concrete mixture is also given in Table II.



Number	Mix *	Admixture *	Workability Figure
1	1:3:6	0	183
2	"	0.017C(1)	162
3	"	0.033C	144
4	"	0.050C	84
5	"	0.050K(2)	143
6	"	0.100K	112
7	"	0.150K	123
8	"	0.050L(3)	169
9	"	0.100L	140
10	"	0.150L	140
11	1 2½ 5	0	138
12	"	0.017C	125
13	"	0.033C	91
14	"	0.050K	101
15	"	0.100K	86
16	"	0.050L	141
17	"	0.100L	129
18	1 2 4	0	82
19	"	0.017C	57
20	"	0.033C	48
21	"	0.050K	56
22	"	0.100K	44
23	"	0.050L	97
24	"	0.100L	62
25	1 1½ 3	0	57

* Proportions by Weight.
(1) C = Celite
(2) K = Kaolin.
(3) L = Hydrated Lime

FIG. 2.—Penetration-Impact Curves for 25 Concrete Mixtures Gaged to the Same Consistency (Flow)

The penetration tests on the 25 mixtures given in Table II yielded the set of curves shown in Fig. 2, which is included mainly for the purpose of illustrating the method of plotting penetration against the number of impacts, and showing the range of results obtained on concrete mixtures gaged to the same consistency. The interesting feature is that with very few exceptions, these curves occur in the order of the *workability* of the concretes to which they correspond, so far as we were able to judge by the behavior of the mixtures during the operations of mixing, handling and placing in the molds. Thus the nearer a curve is to the left-hand side of the diagram, the more workable the concrete from which it is obtained, or, in other words, the fewer the number of impacts required to cause the rod to penetrate to a given depth, the more workable the mixture. This general relation immediately suggests that the number of impacts required for the rod to reach a given depth, say 11 in., in a given concrete mixture might be taken as a workability figure for that mixture. The objection to this is that the curves do not all show the regularity of a mathematical family, and it is, therefore, advisable to find some workability figure which depends, not on any one point of a curve, but upon the curve as a whole. We have tried a number of methods of doing this, but none seems more satisfactory than the computation of the area under each curve as shown in the diagram, that is, the area enclosed by the curve, the axis of penetration and the 11-in. penetration line. The order of these areas agrees very satisfactorily with our estimate of

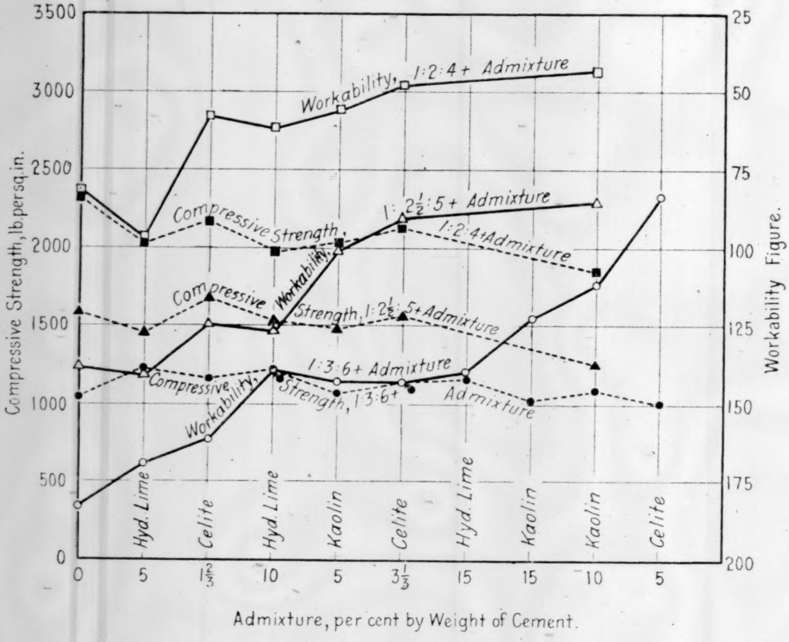


FIG. 3.—Effect of Different Quantities of Celite, Kaolin and Hydrated Lime on Strength and Workability of Concrete Mixtures. Workability Figures Connected by Full Lines; Strength Values Connected by Dotted Lines

the order in which the curves occur, and these are the figures given in the last column of Table II. It is not to be inferred that these empirical figures are *directly* proportional to the actual workability of the concrete mixtures, but they do serve to indicate the order of workability and to permit comparisons to be made of different mixtures in this respect.

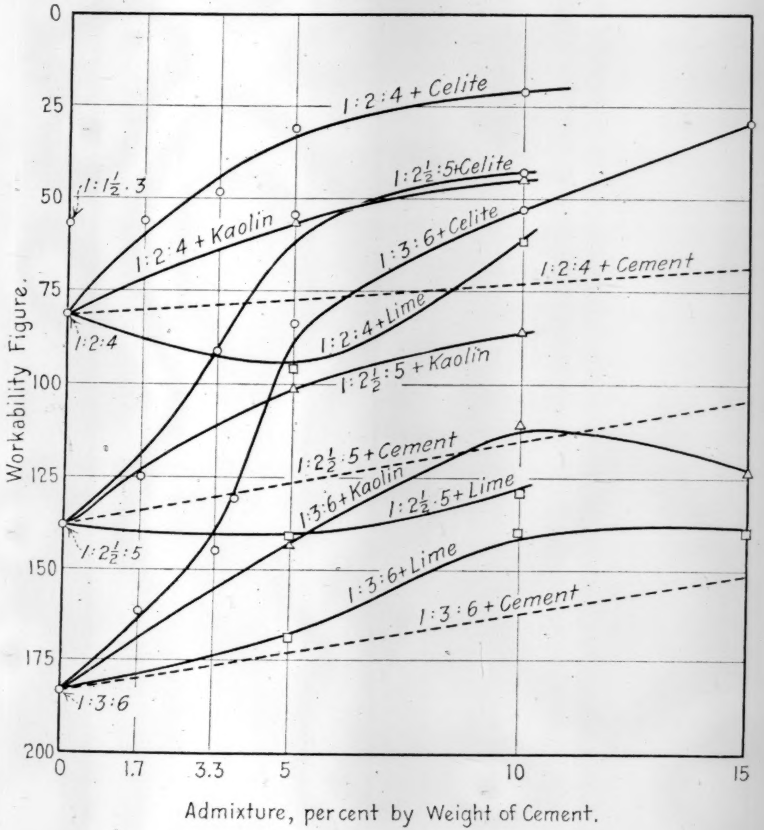


FIG. 4.—Relation Between Quantity of Admixture and Workability of Concrete Mixtures

In Fig. 3 the workability figures and the strength values from Table II are plotted opposite the various concrete mixtures arranged approximately in order of increasing workability. It is to be noted that the workability *figures* decrease as the *workability* increases, hence, these figures increase from the top to the bottom of the diagram in order that the workability curves may have the usual significance, that is, the curves rise as the workability increases. It is seen that the compressive strengths tend to drop with increasing proportions of the admixtures, and that *as a rule* increased workability involves a sacrifice in strength. In the case of the 1:3:6 mixtures, however, practically no loss in strength resulted from any of the admixtures, but on the other hand a gain in strength was usually obtained, up to nearly 20 per cent for the 5-per-cent and 10-per-cent admixtures of hydrated lime. It will be observed here and subsequently that the workability figures for the lime admixtures are noticeably more erratic than the others, due, we believe, to the fact that the lime admixtures produced very rapid changes in consistency during the first four or five minutes after the concrete was mixed, within which period both the flow and penetration tests were made. The highest workability for the 1:3:6 mixtures was obtained with 5 per cent of celite, which gave a value about equal to that of the 1:2:4 mixture, with practically no loss in strength. In general, the strengths of the richer mixtures drop to some extent when admixtures are used, but it should be remembered that the cylinders are cured under damp conditions, whereas under job conditions the effects of the admixtures might, and probably would, be very much more favorable. This is a very important consideration, and should be adequately covered in any subsequent investigation of the effects of admixtures upon the strength of concrete. There is also an increased yield in the concretes containing the admixtures, and this fact should not be overlooked in the final balancing of accounts.

In Fig. 4 the workability figures for the mixtures of Table II are plotted against percentages of admixture (by weight of cement). Curves are thus obtained which show clearly the relative effects of the various admixtures. Incidentally, the curves show where apparently inconsistent results have been obtained; for example, there is no explanation of the fact that 15 per cent of kaolin gave a lower workability than 10 per cent of kaolin in a 1:3:6 concrete mixture, nor that an addition of 5 per cent of hydrated lime lowered the workability of a 1:2:4 mixture. On the other hand, the curves indicate with certainty that the effect of celite upon workability is greatest for all mixtures, and that the effect of kaolin is intermediate between that of celite and hydrated lime. It is possible also to get roughly approximate comparisons of the amounts of the various admixtures which give the same workability. For example, 10 per cent of hydrated lime in the various mixtures has about the same effect, on the average, as 5 per cent of kaolin, 2.5 to 3 per cent of celite and about 20 per cent of cement. The workability figures are not determined with sufficient accuracy, however, to justify putting too much weight on direct quantitative comparisons of this sort.

It appears from the foregoing tests that the effect of any admixture on the workability of concrete is dependent mainly upon fineness, not necessarily upon the fineness of the powder in the dry state, but the fineness of the material as measured by the completeness of its distribution in the mixture. Thus, the celite, which is exceedingly fine in the dry state, appears to be very thoroughly dispersed in the concrete mixture, requiring a considerable increase in the amount of mixing water. Both the kaolin and the lime, if thoroughly soaked in water before being added to the concrete, would undoubtedly have a greater effect upon workability than they do under the conditions of these tests, but in the few minutes which are available for wetting them when added as dry powders, they do not have opportunity to break up and thus produce the degree of plasticity obtained with the celite. The increased plasticity of the matrix which the admixtures bring about seems to have a double effect in that it tends to preserve the homogeneity of the concrete mixture as a whole, and at the same time to have a distinct lubricating effect. Any tendency to lessen segregation must facilitate the movement of the penetration rod as it goes deeper into the concrete mixture, but where previous experience, as stated in the introduction to this paper, has shown that the segregation is not appreciably lessened, the easier penetration of the rod must be accounted for by easier displacement of the aggregates.

RELATION BETWEEN WORKABILITY AND CONSISTENCY AS INDICATED BY THE PENETRATION AND FLOW TESTS—SERIES II

In the tests of Series I every effort was made to maintain the flow of the various concrete mixtures at a figure of 110. In Series II an equal amount of effort was devoted to using such quantities of mixing water as would produce flows of 80, 95, 125 and 140 in a number of typical mixtures. For this purpose 15 of the mixtures used in Series I were employed, namely, the $1:1\frac{1}{2}:3$, $1:2:4$ and $1:3:6$ mixtures without admixtures, and the $1:2:4$ and $1:3:6$ mixtures each with two percentages of celite, lime and kaolin, respectively. Flow and penetration tests were made on each mixture and on each consistency on five different days. The results of these tests, together with the results obtained from the same mixtures in Series I, are shown graphically in Fig. 5. In this diagram the workability figures obtained from the penetration tests are plotted against flows.

The irregularities of the curves in Fig. 5 indicate very clearly that the penetration test is not a precision test, but in spite of this fact some very interesting general relations are evident. In the first place it is apparent that the effect of change in consistency on the penetration is much less marked than the effect of changes in the character and proportions of the solid ingredients. Thus the 1:2:4 and the 1:3:6 mixtures each show a range in workability of about 40 points from the driest to the wettest mixtures, whereas the 1:2:4 curve is roughly 80 points above the 1:3:6 curve. We are led to the conclusion, therefore, that the workability of a concrete mixture, as indicated by the penetration test, is dependent not so much upon the water as

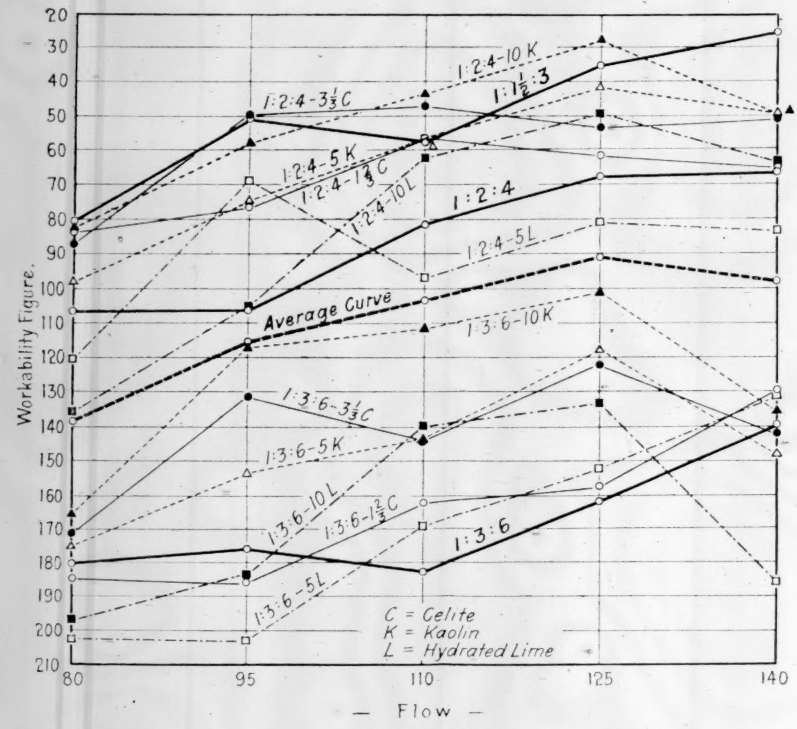


Fig. 5.—Relation Between Workability and Flow of 15 Concrete Mixtures

upon the solid materials in the mixture. If this is actually the case, it calls for a somewhat different conception of workability than that indicated in the introduction to this paper. It must be admitted upon first consideration that the workability as indicated by the results of these tests is not in line with our former notions of workability, but recalling our intimate contact with the four or five hundred batches upon which the curves in Fig. 5 are based, we believe there is a point of view from which we may see a direct and valuable relation between the results of the penetration tests and workability. If we use the term "workability" to mean, not merely the degree of effort required in handling and placing concrete, but the degree of effort required in handling and placing concrete *in such manner as to give a uniform and homogeneous finished product,* then we may declare that the results of the penetration tests confirm our judgment on the workability of concrete mixtures.

It seems to us that this is quite an important conception and one which emphasizes the proper course to pursue in selecting the type of concrete mixture for any particular purpose. Within the limits of accepted practice the consistency of reinforced concrete will generally correspond to a flow somewhere between 110 and 140. Without taking into account the increase in strength obtained by the use of less water, the penetration test indicates, as shown by the "average" curve in Fig. 5, that there is not a great difference in *workability* between these consistencies if we take into account the work required to get an equally good distribution of concrete in the finished job. In other words, if one wants a *good* job of concreting, he can get it with a flow of 110 about as easily as with any wetter mixture. The over-wet mix may be chuted with a minimum of labor, but excessive segregation gives poor distribution unless additional labor is supplied to take care of this. On the other hand, if too dry a mixture is used, the lack of lubrication lowers the workability, and additional work must be done to insure elimination of pockets and voids.

Perhaps the most interesting indication from these tests, aside from that which has just been discussed, is that a maximum workability is indicated at a flow of 125. This maximum would be somewhat more pronounced on the "average" curve were it not for the inexplicable rise of the three lowest curves between flows of 125 and 140. This again emphasizes that dependable quantitative values of workability cannot always be obtained from a small number of penetration tests as made with the present apparatus. In passing, it may be noted that the curves for the lime admixtures tend to be more erratic than the others and that the position of the curves confirms the relative effectiveness of the various admixtures as obtained from the tests of Series I. It is interesting to note also that the effects of the admixtures on the 1:2:4 concrete are on the whole as marked as on the 1:3:6 concretes.[1]

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[1] This conclusion does not agree with Abrams' deductions regarding the effects of admixtures on workability. See "Effect of Hydrated Lime and Other Powdered Admixtures in Concrete," *Proceedings,* Am. Soc. Testing Mats., Vol. XX, Part II, p. 202 (1920).

THE WORKABILITY OF 1:2:4 CONCRETES AS AFFECTED BY A VARIETY OF ADMIXTURES AND CHANGES IN FINE AGGREGATE—SERIES III

Having carried the investigation to a point where a reasonable interpretation of the penetration test could be made, we decided to check its indications on a number of additional mixtures. These were compounded on a 1:2:4 basis with a number of new admixtures, and included two modifications of the fine aggregate. The consistency or flow was held throughout at very nearly 110, as in the first series. Test cylinders were also made from these mixtures for the determination of compressive strength at 28 days. In all cases, penetration tests and cylinders were made for each mixture on five different days, the results being obtained from the average of five tests.

The cement used was a different brand from that used in Series I and II, but both fine and coarse aggregates were the same as before, except as noted below for mixtures Nos. 2 and 3. The admixtures were selected from materials close at hand, some of them probably representing nearly the attainable limit in respect to fineness. For brevity, the following list of mixtures names only the admixture in most cases, it being understood that 5 per cent of the admixture by weight of cement was added to the basic 1:2:4 mixture, No. 1, unless otherwise stated.

No. 1.—Normal 1 : 2 : 4 mixture, the same in all respects as the corresponding mixture in Series I, except in brand of cement.

No. 2.—Same as No. 1, except that the fine aggregate was prepared by removing the portion (29 per cent) of the normal fine aggregate passing the No. 50 sieve.

No. 3.—Same as No. 1, except that the fine aggregate was prepared by removing the portion (27 per cent) of the normal fine aggregate retained on the No. 16 sieve.

The approximate sieve analyses of the fine aggregates in these first three mixtures was as follows:

	PERCENTAGE PASSING RESPECTIVE SIEVE		
SIEVE	No. 1	No. 2	No. 3
No. 4	98	97	100
No. 8	84	78	100
No. 16	73	62	100
No. 30	59	42	81
No. 50	29	0	40
No. 100	9	0	12

4. *Basic Magnesium Carbonate.*—A very light powder, comparable with celite in appearance and weight per unit volume. This material produced an unexpectedly quick set in the concrete, the mixture being quite hard within 30 minutes after mixing.

5. *Short Fiber Asbestos.*

6. *Asbestine.*—Finely pulverized asbestos, used as an extending pigment for paints.

7. *Carbon Black.*—Used commercially as a rubber filler, and purchased on an order specifying "best grade gas black."

8. *Zinc Oxide.*—Used commercially as a rubber filler. This material interfered seriously with the set of the concrete, which was barely hard enough to remove from the molds 24 hours after mixing.

9. *Flake Graphite.*

10. *Caustic Magnesia.*—Low-burned magnesite which hydrates rapidly when fresh. The sample used had been stored for some months and was not noticeably active.

11. *Pulverized Talc.*

12. *Pulverized Blast-Furnace Slag.*

13. *Calcium Chloride.*—A 4-per-cent solution of commercial chloride was used as gaging liquid.

14. *Accelerator "A"*—A proprietary material containing calcium chloride and hydrated lime in the form of calcium oxychloride.

15. *Accelerator "B."*—A proprietary material in liquid form said to contain calcium chloride and organic compounds and known to have a lower surface tension than a plain calcium chloride solution.

16. *Aluminum Chloride and Hydrated Lime.*—An aluminum chloride solution was prepared containing an amount of anhydrous salt equal to 2.5 per cent of the weight of the cement. An equivalent amount of hydrated lime was stirred into this solution producing a gelatinous precipitate of aluminum hydroxide. This was poured into the concrete mixture and enough additional water was added to give the desired consistency.

17. *Bentonite.*—A highly colloidal clay, added to the mixture as a thin slip containing 1.25 per cent of the dry material by weight of cement.

18. *Celite.*—Similar to the material used in Series I and II, but from an old lot which had been stored in the laboratory for a year or more.

19. *Crude Oil.*—A heavy residuum oil, incorporated in the wet concrete mixture by vigorous mixing.

The results of the penetration and compression tests of this series are shown graphically in Fig. 6. The mixtures are plotted approximately in the order of their workability as indicated by the heavy line curve. It is seen that the strength tends to decrease with increasing workability and that the water-cement-ratio curve tends to reflect the strength curve. There are numerous irregularities, however, and in some cases these are of considerable significance.

The results of the variations in fine aggregate are particularly interesting. The normal 1:2:4 mixture gave a workability considerably higher, and a strength about 20 per cent lower, than the corresponding mixture in Series I. This indicates a considerable difference in the characteristics of the cements used in the two series of tests. The coarse sand mixture, requiring a water ratio of 0.94 for the medium consistency, was very harsh and gave a lower workability than the normal 1:3:6 mixture in Series I. The fine sand mixture requiring a water ratio of 1.09 gave the highest workability obtained, but the strength of this mixture, and also of the coarse sand mixture, was practically identical with that of the normal mixture.

The authors consider the results of the tests on these three sand mixtures as almost convincing evidence of the fallacy of specifying gradation limits for a fine aggregate without regard to the concrete mixture in which it is to be used. The fine sand, which is unquestionably the best for the concrete used in these tests, would be rejected as unsuitable for fine aggregate on most existing specifications.

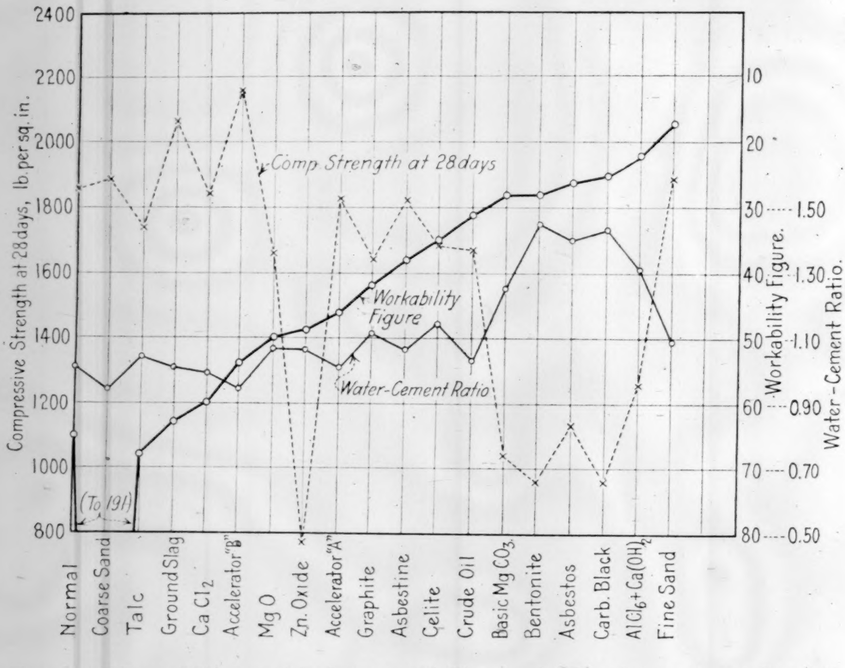

FIG. 6.—Effect of Various Admixtures and Changes in Fine Aggregate on Strength and Workability of 1 : 2 : 4 Concrete Mixtures

What is more important in connection with this investigation, however, is the fact that the fine sand mixture surpassed all others in workability as indicated by the penetration test. The explanation of this unexpected result is the fact that the fine sand matrix almost completely prevented segregation, this apparently having greater effect on workability than the higher lubrication afforded by such admixtures as bentonite and aluminum hydroxide.

Space is not available for a description of the behavior of all the admixtures, and a brief reference to the outstanding results obtained will conclude the paper. Basic magnesium carbonate and zinc oxide gave low strengths, each of these interfering with the normal setting and hardening of the concrete. Short fiber asbestos and carbon black gave very high workability, but low strength due to excessive quantity of mixing water required. Pulverized blast-furnace slag did not greatly improve workability but increased the strength about 11 per cent. Bentonite and aluminum hydroxide seemed to give the highest degree of lubrication, and consequently high workability, but serious reduction in strength resulted from the excess of mixing water required. Crude oil gave good lubrication, but the great objection to this material is the difficulty of incorporating it thoroughly in the mixture. Its tendency to rise to the surface of the concrete is also a serious objection to its practical use. The celite used in this series was somewhat less effective in increasing the workability than that used in the first series of tests; nevertheless, in this series also it leads the admixtures that might be considered from a commercial standpoint. Under the conditions of these tests it reduces the strength about 10 per cent. The results from the accelerators are interesting in confirming to some slight extent our theories of workability and strength. Accelerator "A" is a calcium oxychloride which quickly decomposes to calcium chloride and hydrated lime when mixed with water. The hydrated lime thus freed is much more effective upon workability, as indicated by the results of these tests, than the dry hydrated lime alone, which seems to confirm our belief that in the duration of the mixing process the dry material is not effectually dispersed. Accelerator "B" gave the highest strength obtained and an appreciable improvement in workability. Whether or not the reduced surface tension of this compound is responsible for these effects we cannot say, but the fact that a smaller amount of mixing water was required for the desired consistency seems to point to this conclusion.

SUMMARY AND CONCLUSIONS

The penetration test described in this paper is the first worthwhile solution of the problem of measuring the workability of concrete which the authors have found in a research extending over a period of several years. As used in this investigation, the test is lacking in desirable precision; it furnishes a measurement of workability in arbitrary units only, and it is not applicable to the entire range of concrete mixtures. On the other hand, these faults can probably be eliminated, and even in its present stage of development, its indications seem correct and in accord with the best judgment of a number of experienced operators. This is true if workability is defined as that property of a concrete mixture upon which depends the amount of work required to handle and place the concrete *uniformly*, under the ordinary conditions of use.

As indicated by the penetration test, the workability of a concrete mixture seems to depend upon two factors, segregation and lubrication. It appears that both of these factors can be controlled by a suitable selection of ingredients, an increasingly fine aggregate reducing the segregation, and powdered admixtures reducing segregation and promoting lubrication. This increase in workability brought about by the use of finely divided admixtures seems to be proportional to the fineness as measured by the completeness or extent of dispersion of the material in the mixture. Thus, celite was found to have greater effect in improving the workability of concrete mixtures than kaolin, and kaolin a greater effect than hydrated lime.

In studying the relation between workability and consistency, as indicated by the penetration and flow tests, respectively, the interesting conclusion was reached that workability depends more on the character and proportions of the solid ingredients in the concrete than on the amount of mixing water used. Although this conclusion may appear open to question, the authors believe it to be correct, and they also believe that false notions in regard to workability of concrete have been created by reference to chuting operations. The facility with which concrete flows down a chute is probably dependent both upon workability *and water*, the effect of the latter predominating in the over-wet mixes. Whatever the facts may be, the indications of the penetration test substantiate the authors' opinion that there is no premium upon, and rarely an excuse for, employing a consistency higher than that giving the maximum workability obtained in these tests.

Under damp storage conditions, small quantities of admixtures generally increase the strength of lean mixtures, even though they require an increase in the amount of mixing water for a given consistency. The richer mixtures under the same conditions suffer a slight loss in strength. It is important, however, that further work be done to establish the effects of admixtures under drying conditions, where it is quite probable that strength, as well as workability, will be benefited. It is important also that further work on the subject include studies of the relative yield and shrinkage of concretes with and without admixtures, since these factors must be taken into account in determining the resultant effect in the finished product.

Acknowledgment.—The authors desire to acknowledge the valuable assistance and helpful suggestions of Messrs. J. R. Dwyer and G. A. Smith of the U. S. Bureau of Standards in carrying out the work of this investigation.

DISCUSSION

Mr. Chapman

MR. CLOYD M. CHAPMAN.[1]—In the addition of hydrated lime, was there any experiment involving the wetting of the lime and adding it as a cream instead of as a dry powder?

Mr. Pearson

MR. J. C. PEARSON.—We did not do that in the case of the lime, but one of the materials, bentonite, a highly colloidal clay, was added in the form of a thin slip. I have no doubt that both the kaolin and the lime would have shown up somewhat better if they had been prepared and added in this form.

Mr. Chapman

MR. CHAPMAN.—Experiments I have made for a number of years have indicated that it is much easier to incorporate lime if it is added with the mix in water instead of adding it as a dry powder. Is there any intention to try the effect of pre-wetting, because it is a very much easier way to add lime to the mixture?

Mr. Pearson

MR. PEARSON.—I think in this particular problem, that is, if it has any commercial application, it would be rather inconvenient to add these materials in slip or cream form. Personally, I believe that if there is any field for admixtures at all they have got to be in the form of a powder that can be added and handled like cement. We are going to continue the experiments, however, and we may try cream of lime in comparison with the dry powder.

Mr. Ingberg

MR. S. H. INGBERG.[2]—I should like to ask the author to what extent he thinks the results are applicable to other aggregates than the Potomac River aggregate that was used? Gravel aggregate, as a rule, is more workable than stone aggregate, and there is a considerable difference between stone in their resistance to penetration by shovels and handling generally, depending upon their gradation and surface characteristics.

Mr. Pearson

MR. PEARSON.—I have no doubt that the type of coarse aggregate will have a very marked effect, but before we can determine this we shall have to go to a larger batch. We have not room in this apparatus to use commercial sizes of aggregate up to $1\frac{1}{2}$ in. The whole thing must be enlarged in order to get comparisons of the coarse aggregates. I think we shall do that, but we shall probably have to use machine mixes because of the large sizes of the batches required.

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AN INVESTIGATION IN THE USE OF CALCIUM CHLORIDE AS A CURING AGENT AND ACCELERATOR OF CONCRETE

By H. F. Clemmer[1] and Fred Burggraf[2]

In 1921, the Illinois Division of Highways began an extensive investigation in the use of calcium chloride both as a curing agent and as an accelerator in the setting of concrete. The need for a method of curing concrete pavements without the use of water became evident when a number of road builders adopted the central mixing plant method of construction. The cost of transporting water, either by tank or pipe line, for the purpose of curing alone, proved excessive. Curing under these conditions was extremely costly in many cases. The absence of sufficient water in certain localities also created a demand for a new method of curing. At the same time, the importance of developing a satisfactory and safe means of securing a quick set and early strength was appreciated, when it was considered that at certain times it is necessary to place a small stretch of pavement in low temperatures, or open a section of road in less time than is usually the custom. This paper reviews the investigations conducted by this department, the results of which have led to the adoption of both the external and internal usage of calcium chloride in concrete highway construction.

External Treatment

Over 500 concrete slabs, 8 by 12 by 30 in., were subjected to various surface treatments and tested for transverse strength. Cylinders, 6 by 12 in., were also made to serve as a check on the compressive strength of the concrete making up the transverse specimens. The same volumetric mix of 1:2:3½, which is the standard adopted by the state of Illinois for concrete highways, was used in all specimens. Portland cement was used in all cases and the aggregates consisted of sand and washed limestone which passed all state specifications.

As Illinois was interested primarily in securing a satisfactory curing at the end of 28 days or earlier, more emphasis was placed on these short-time tests than on 60 or 90-day specimens. For the purpose of checking the strength of the untreated specimens at the older ages, a number of 60 and 90-day specimens were made. At the time of testing, the specimens were removed from their forms and broken in a universal testing machine with the treated side in tension. The modulus of rupture was then computed and the variation in strengths considered as a measure of the completeness of curing. Inasmuch as the same operators made all of the specimens and the consistency and gradation of aggregates was the same in all cases, it was fairly well established that a uniform concrete was present. Due to the magnitude of the investigation, it was necessary to divide the tests into eight series, some of which were check tests and some which differed only in the method of applying the curing materials or placing the specimens. The slight differences in the method of treatment in each series failed to show any great variation in results, and for this reason the eight series are reported as condensed into but three, which are taken up individually.

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Series 1.—This series included 260 specimens of which 100 were poured in holes in the ground having side boards only, 60 in oiled wood forms in the basement of the laboratory, and the remaining 100 in concrete forms. These forms consisted of 1:4:5 concrete with walls 3 in. thick and 8 in. deep. They were allowed to set up for 48 hours before the curing specimens were poured. The object in using these forms was to provide a condition of curing concrete against concrete as similar as possible to a portion of a slab within a pavement.

Care was taken to cover all specimens with a canvas immediately after they were finished to provide protection against hot, drying sunlight. All specimens were finished with a trowel and were given their curing treatments the day following.

The specimens in this series received the following treatments: 2 in. of dirt wet 3 days; 2 in. of dirt wet 7 days; 2 in. of dirt wet 14 days; 1 lb. per sq. yd. of calcium chloride; 1 lb. per sq. yd. of calcium chloride with 1 in. of dirt; 3 lb. per sq. yd. of calcium chloride; 3 lb. per sq. yd. of calcium chloride with 1 in. dirt; $\frac{1}{2}$ to $\frac{2}{3}$ gal. of asphalt per square yard.

Series 2.—The specimens in this series were poured both in oiled wood forms and in concrete forms. The effect of an oiled subgrade, as well as the use of magnesium calcium chloride and sodium silicate as curing agents, was investigated in this series. The length of time over which calcium chloride must remain on the specimens in order to cure them properly was also investigated in this series. This latter test was prompted by the question regarding the necessity of making a second application of calcium chloride if the original was washed from the pavement by a rain.

The specimens in this series were treated as follows: 3 lb. per sq. yd. of calcium chloride after 10 hours; 3 lb. per sq. yd. of calcium chloride after 24 hours; 3 lb. per sq. yd. of calcium chloride after 10 hours, washed off 12 hours later; 3 lb. per sq. yd. of magnesium calcium chloride after 10 hours; 3 lb. per sq. yd. of magnesium calcium chloride after 24 hours; $\frac{1}{5}$ normal solution of sodium silicate after 10 hours; $\frac{1}{5}$ normal solution of sodium silicate after 24 hours; 2 in. of dirt wet 7 days; 2 in. of dirt wet 14 days; 2 in. of dirt wet 7 days (subgrade oiled). These specimens were tested at the age of 14 and 28 days.

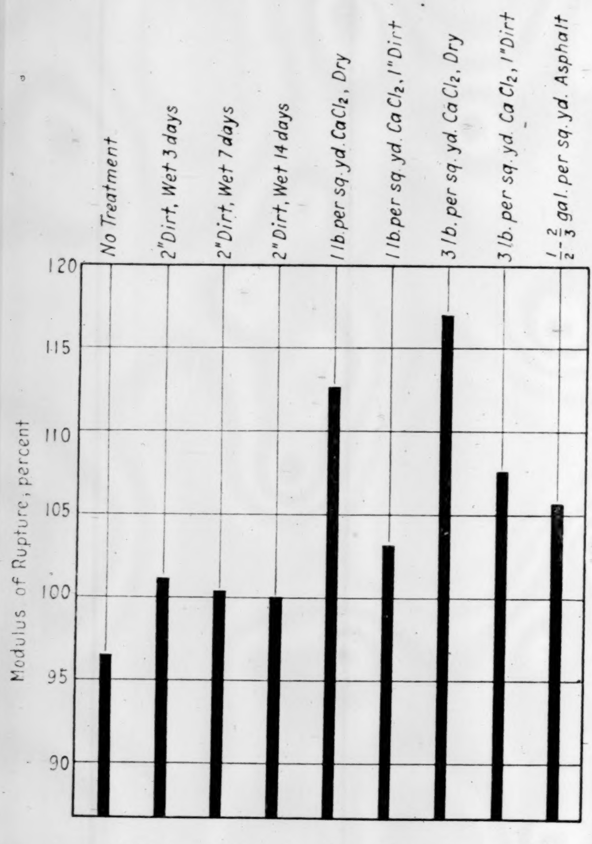

FIG. 1.—Comparisons of External Treatments. Strength of specimens cured with 2 in. dirt, wet 14 days, taken as 100 per cent cured. Each result is the average of 11 specimens. Age 28 days

Series 3.—The series consisted of specimens which were cured with various forms of calcium chloride. It was first desired to compare the results obtained with dry calcium chloride with those obtained with calcium chloride in solution, with the thought in mind that a more even distribution of the material could be made if in solution. The second half of this series was devoted to the comparison of the results obtained by the use of flake calcium chloride with those obtained by using granulated calcium chloride.

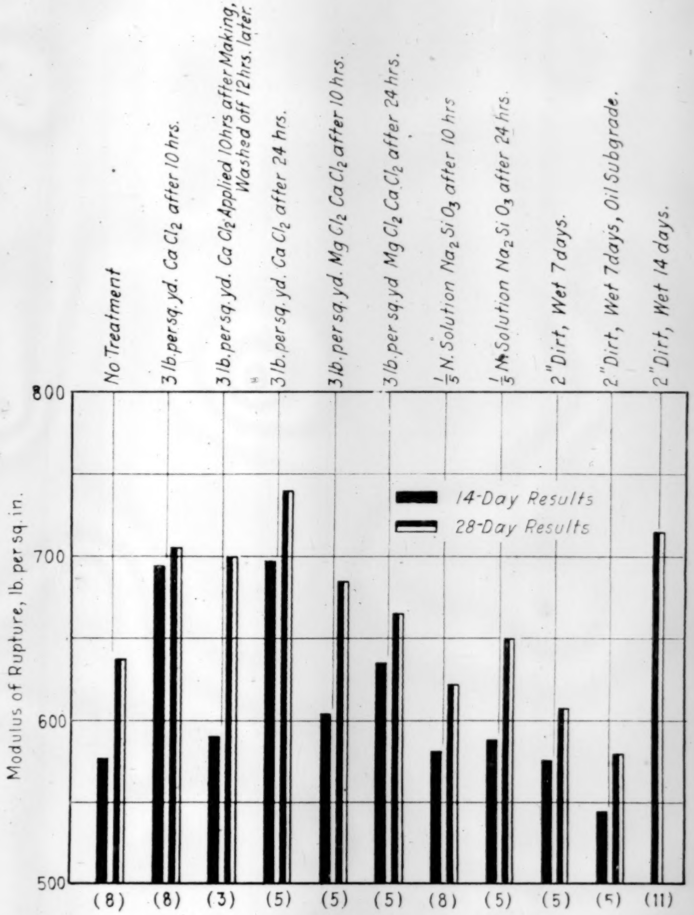

FIG. 2.—Tests Showing Results of Various Curing Treatments. Figures in parentheses indicate number of specimens broken at each age

Results of Various Curing Methods.—Fig. 1 contains a graphical comparison of the results of the various curing methods as indicated by the moduli of rupture of the specimens in Series 1, the results shown being the average of 11 specimens. As the curing of concrete with 2 in. of dirt wet for 14 days has been considered by this state a satisfactory method of curing, the specimens cured under this method have been regarded as 100 per cent cured and the results obtained by other methods taken as a percentage of this. As shown in the figure, only those specimens which received no treatment showed a strength less than 100 per cent, while the specimens cured with 3 lb. per sq. yd. of calcium chloride indicate the maximum of 116.9 per cent curing. Those slabs cured with 1 lb. per sq. yd. of calcium chloride also show a high degree of curing.

Fig. 2 contains a graphical comparison of the results of the various curing methods as indicated by the moduli of rupture of the specimens in Series 2. For the purpose of comparing the results at 28 days of one curing method with the results of another at 14 days, the moduli of rupture of the specimens at both ages are contained in this figure. As in Series 1, the specimens cured with 3 lb. per sq. yd. of calcium chloride indicate by far the best results. It should be noted that the strength of these specimens at the end of 14 days is within 20 lb. per sq. in. of the strength of those specimens cured with wet dirt at the age of 28 days. This early strength obtained with using calcium chloride curing would indicate that a pavement could be opened for traffic at the end of 14 days, whereas under the old method of curing it was felt unsafe to subject a pavement to traffic in any less time than 30 days. The results obtained from the specimens which were treated with calcium chloride that was removed some hours later illustrate that at the end of 28 days, proper curing has taken place. In other words, if the calcium chloride remains on the concrete for a period of 24 hours, its removal will not affect the curing of the concrete. Later tests in this regard illustrate that even less time than 24 hours was required for calcium chloride to properly cure concrete.

A brown surface stain developed on the specimens cured with magnesium calcium chloride. This discoloration of the concrete seems to result from the use of magnesium calcium chloride and for this reason this material as a curing agent is eliminated.

It is evident in the comparison of results on the specimens cured in like manner, but having both a normal and oiled subgrade, that no benefit is derived from the oiled subgrade. In both the 14 and 28-day tests, less strength resulted from the specimens placed on the oiled subgrade than was obtained with the normal condition of subgrade.

Fig. 3 contains a graphical comparison of the results obtained in the use of the dry calcium chloride and the calcium chloride in solution investigated in Series 3. The dry calcium chloride indicated much the better curing at the end of 28 days. The effectiveness of the calcium chloride appeared to be diminished when in solution.

In the comparisons made between the results obtained with flake calcium chloride and those obtained with the granular form of the material, the indications were that the granular gave slightly better results. However, the difference was slight, and in actual practice, the flake or granular forms of calcium chloride are considered equally as good.

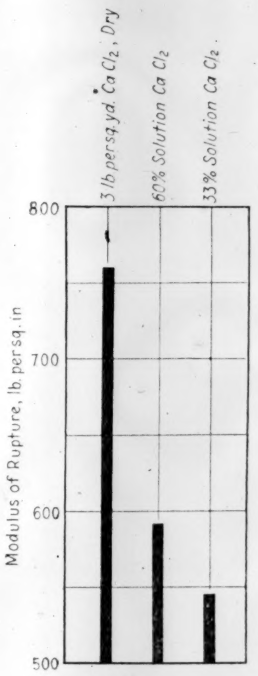

Fig. 3.—Twenty-Eight-Day Results on Specimens Comparing the Use of Dry Calcium Chloride with Solutions of Calcium Chloride. Each Result is the Average of 3 Specimens

To gain an insight into the hygroscopic properties of calcium chloride, a test was made which illustrated very conclusively the power of this material to attract moisture from the air. Mortar specimens of exactly the same weight were made up, some being surface treated with calcium chloride and some receiving no treatment. In order to insure against a base of variable absorbent properties, the specimens were made up on tin which allowed no loss of water through absorption. At regular hourly intervals these specimens were weighed.

The specimens which were untreated lost weight immediately after making. It was noticeable that an increase of weight was secured in the treated specimens up to 45 hours after they were made, after which a gradual loss of weight developed but to a less extent than the untreated specimens. The increase in weight secured by the treated specimens in the first 45-hour period would illustrate that the calcium chloride not only prevented the moisture in the concrete from evaporating but also was responsible for securing additional moisture from the air. A graphical comparison of the results obtained in this test is given in Fig. 4.

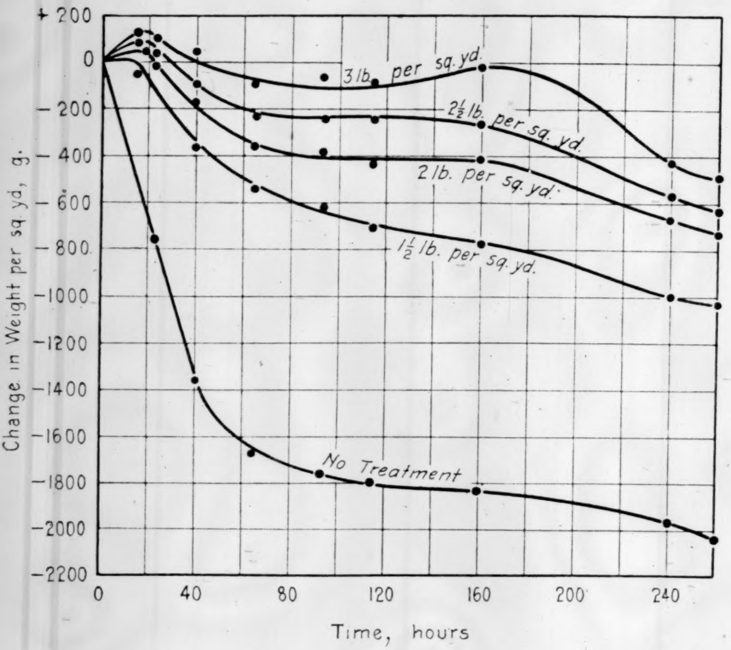

FIG. 4.—Change in Weight of Treated and Untreated Specimens

For the purpose of studying the action of calcium chloride on cement, a number of cement mortar briquettes were made and subjected to various amounts of surface applications. When these briquettes were broken (no consideration being given to the strength), a depth of penetration of calcium chloride was noticeable. Fig. 5 shows a number of broken briquettes which had been treated with various amounts of calcium chloride. The depth of penetration of the chemical is plainly seen in the darkened portion of the briquette.

A study was also made of the effects of concentrated and excessive amounts of calcium chloride on the surface of mortar briquettes. The proper distribution of this material is a constant source of trouble with the Highway Department and it was thought advisable to learn as far as possible in the laboratory the effects of too much calcium chloride on the surface of concrete. Consequently, a number of briquettes were made and were subjected to surface applications of calcium chloride up to what would amount to 10 lb. per sq. yd. of surface. A puffing up of the surface was noticed with the result that a deadened concrete was developed. The puffing up of the briquettes is shown in Fig. 6. The danger of a like condition developing in a pavement surface is seen when, in cases where moisture is present in the calcium chloride drums, the salt has a tendency to lump into pieces of considerable size. When these pieces are larger than the size of an egg, their use should not be allowed until they are broken up, since injurious effects of uneven application of the salt will result.

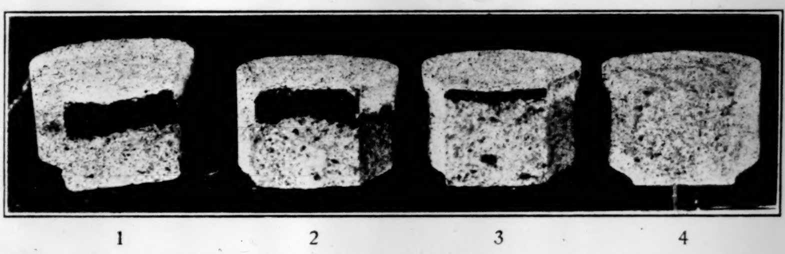

FIG. 5.—Broken Briquettes Showing Depth of Calcium Chloride Penetration

No. 1. 8 lb. per sq. yd. applied immediately
No. 2. 3 lb. per sq. yd. applied 8 hours after making
No. 3. 3 lb. per sq. yd. applied 24 hours after making
No. 4. No treatment

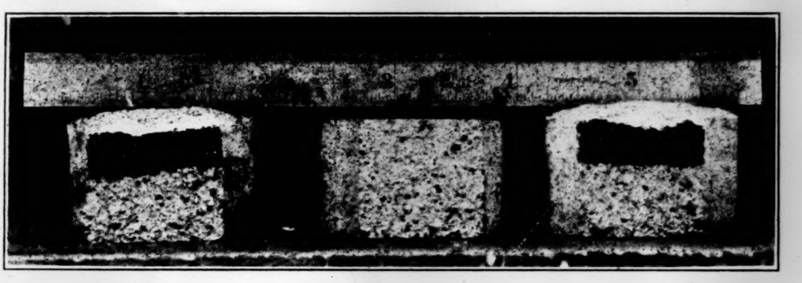

FIG. 6.—Briquette Showing Puffing of Surface Due to Use of Excessive Quantities of Calcium Chloride

Theory of Action of Calcium Chloride on Concrete.—Though the exact action which takes place in the concrete when a surface application of calcium chloride is applied is not known, a theory has been suggested which appears to be the most logical of any yet advanced. The great amount of work done with calcium chloride by the Illinois Highway Department corroborates so far as is known the theory which is as follows:

The hygroscopic properties of calcium chloride are responsible almost entirely for the value of the material as a curing agent. Though the calcium chloride may not obtain sufficient moisture from the air to satisfy this property, its presence on the surface of a concrete specimen which contains more moisture than the air, attracts moisture to aid in the dissolution. This action of the moisture being drawn to the surface of the specimen leaves minute channels, which, after the complete dissolution of the chemical, affords passages for the calcium chloride solution to penetrate the surface, thus giving it access to more cement particles and causing more complete hydration in a shorter time. This quick hydration and decomposition of the lime compounds of the cement particles on the surface causes an impervious layer which prevents the drying out of the concrete. It is also probable that the viscous or oily film of calcium chloride which is formed on the surface offers a protection against the drying effect of the air.

An understanding as to the reason for approximately as great a strength resulting in 14 days from the use of calcium chloride as that resulting in 28 days when curing with wet dirt is apparent when the two methods of curing are considered. In curing with wetted dirt, the increase in strength and hardness of the concrete is due to the ever present moisture which gradually hydrates and decomposes more of the lime compounds. In curing with calcium chloride, the chemical acts as an accelerator in hastening the hydration and inter-reaction of the cement particles on and immediately below the surface. This action due to the nature of calcium chloride is much more rapid than the same effect produced by the wetted dirt, with the result that the specimens cured with calcium chloride usually show a much higher strength at early ages than do those cured with wetted dirt.

Result of Investigation on Curing Pavements in Illinois.—As a direct result of the investigation in curing concrete, the Illinois Highway Department is now permitting the use of calcium chloride in curing state highways. In order to reduce the cost of curing by this method, the amount used has been specified as 2½ lb. per sq. yd. of surface, this amount being considered as effective as the 3 lb. which was employed in the tests. In order to acquaint the engineers and contractors with the use of this material, a circular has been issued which reads as follows regarding the proper application of calcium chloride for curing:

> "Calcium chloride may be used in connection with curing of pavements, taking the place of the usual curing with earth and water, or the curing with water by what is known as the "ponding method." Two and one-half pounds of the flaked or granular material (the flaked is preferred as it is more easily distributed and less easily removed by wind or rain) shall be applied to each square yard of pavement and it shall be distributed uniformly over the surface of the finished pavement by means of a mechanical drilling device or by the use of shovels and long-handled brooms. The material shall not be spread upon the pavement until the latter has thoroughly set—ordinarily from six to eight hours after laying the pavement. Care shall be taken that the material is uniformly spread and if, in the opinion of the engineer, a uniform distribution is not obtained from the shovels, a thorough brooming shall be required. All lumps shall be broken up and uniformly distributed over the surface.
>
> "Calcium chloride shall not be applied during rains and experiments have definitely determined that if a rain follows the placing of the calcium chloride after a period of two or three hours there will have been enough absorption of the calcium chloride by the new pavement preceding the rain so that there will not be required additional applications of the material.
>
> "Although the water line for curing of pavements with water may be omitted where central mixing plants are operated and calcium chloride is used in curing; nevertheless the general specifications with regard to wetting of subgrade and the general specifications with regard to placing of burlap on the freshly deposited concrete and the wetting of burlap shall be strictly enforced. The use of calcium chloride for wetting the subgrade will not be permitted. When calcium chloride is used in curing, the sides of the concrete pavement shall be banked with earth as soon as the forms are removed."

INTERNAL TREATMENTS

The investigations in internal treatment with calcium chloride was mainly in reference to the effect on setting time, the strength in low temperatures, the internal temperatures, and the variable strengths obtained with the same percentage of material in different brands of standard cements. Some work was begun on determining the percentage of hydration caused by the calcium chloride but sufficient data are not at hand for a comprehensive discussion of the experiment at this time, although a short outline of the work will be given. Both physical and chemical tests were included in this investigation. A uniform incorporation of calcium chloride was obtained by dissolving the chemical in the gaging water before the water was placed in the mixer.

Effect on Setting Time.—Three individual experiments were carried on in the study of the effect of calcium chloride on setting time of cement. The first dealt with a comparison of results obtained by the use of various percentages of calcium chloride in neat cement specimens, some of which were stored in an ice box which had a temperature of 38° F. and some of which were stored in a moist closet which had a temperature of 68° F. This test gave an idea of what the calcium chloride might be expected to do in cases of low temperature.

The specimens used in this investigation were of neat cement in the form of small truncated cones having an altitude of 3 in. The apparatus used to determine the set of the cement consisted of an ordinary Vicat apparatus to which was added a horizontal beam suspended on a vertical knife edge. One end of the beam terminated at the top of the needle system, while the opposite end counterbalanced the weight of the needle. By means of a thumb-screw adjustment on the vertical shaft to which an indicator was also attached, the penetration into the cement paste was regulated to any desired depth. In making a reading with this apparatus, the counter weight was first adjusted so that the needle system was in equilibrium. The depth of penetration was regulated by the thumb screw and the specimen in an ordinary Vicat ring was placed under the needle. Weights were then added until the needle penetrated the mass to the depth fixed by the thumb screw. This procedure was repeated at stated

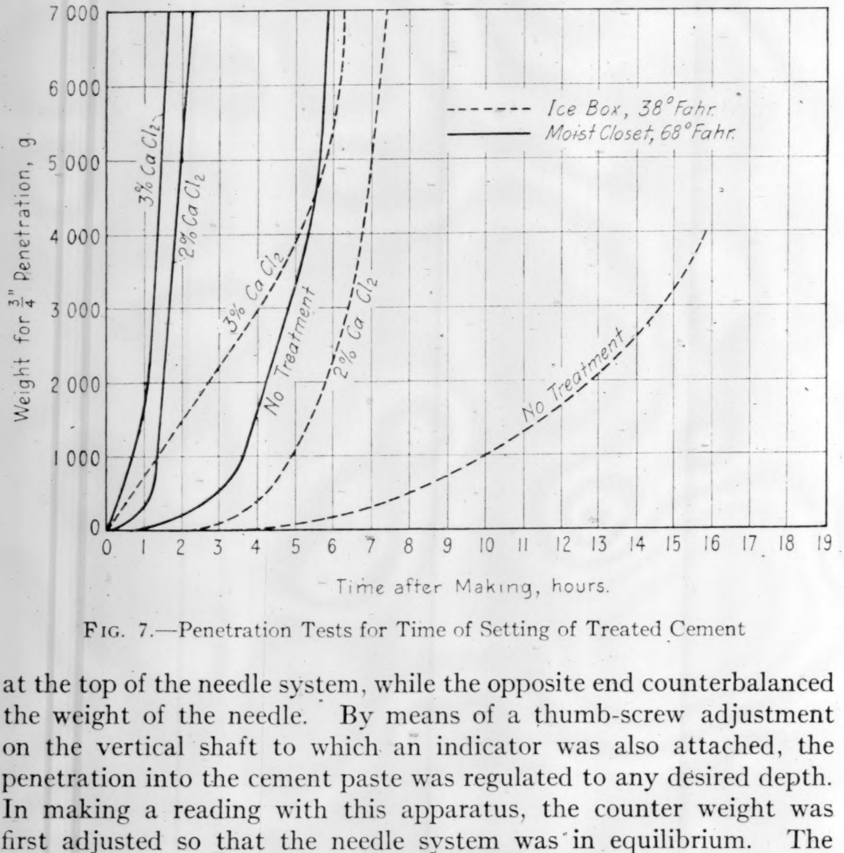

FIG. 7.—Penetration Tests for Time of Setting of Treated Cement

intervals until the final set, as determined by the Gilmore or Vicat needle, was reached or exceeded. In all tests conducted with this apparatus, a constant penetration of $\frac{3}{4}$ in. was used. By plotting the weights causing this constant penetration against the time, a curve was developed which illustrates the action of setting from the time the specimen was made until the final set was reached.

Fig. 7 contains curves typical of the action of the setting of neat cement specimens as determined by the above described apparatus. It is noticeable that the setting time of the specimens at both temperatures is reduced through the addition of calcium chloride. The specimens containing 3 per cent of calcium chloride indicate the best results as far as securing the quickest set is concerned. The percentage of this chemical is based on the weight of the cement.

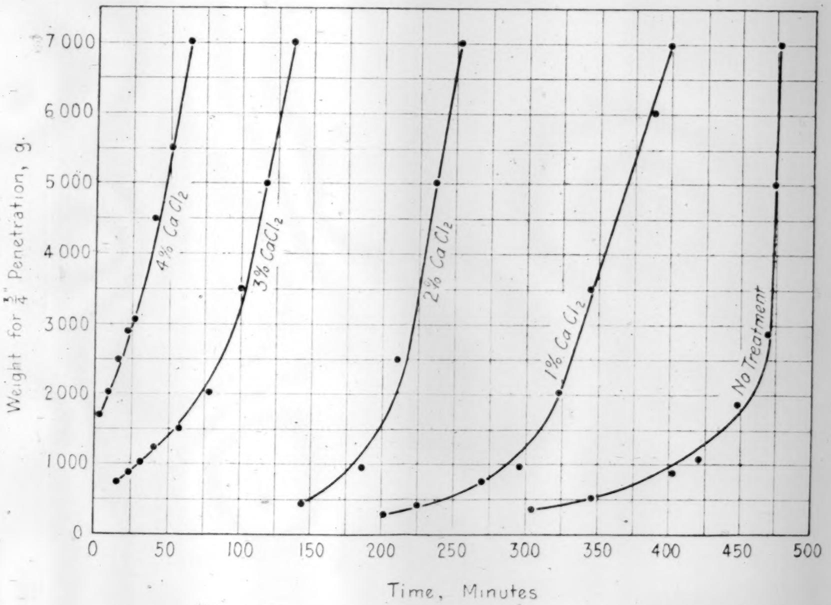

FIG. 8.—Effect of Calcium Chloride of Various Concentrations on the Setting of Cement

Fig. 8 also contains curves which illustrate the setting of cement specimens containing percentages of calcium chloride from 1 to 4 per cent. As in the preceding test, a constant penetration of $\frac{3}{4}$ in. was used and the weight causing this penetration considered as a measure of the set of the specimen. The specimens in this test were stored in a moist closet, the temperature of which averaged 68° F.

Fig. 9 contains a curve which illustrates how variable percentages of calcium chloride incorporated in neat cement specimens affect the initial set of the cement. This curve was also secured through the test with the modified Vicat apparatus. In this particular curve, the carrying capacity of the cement for calcium chloride appears to be about 2.25 per cent in order to comply with the specifications for

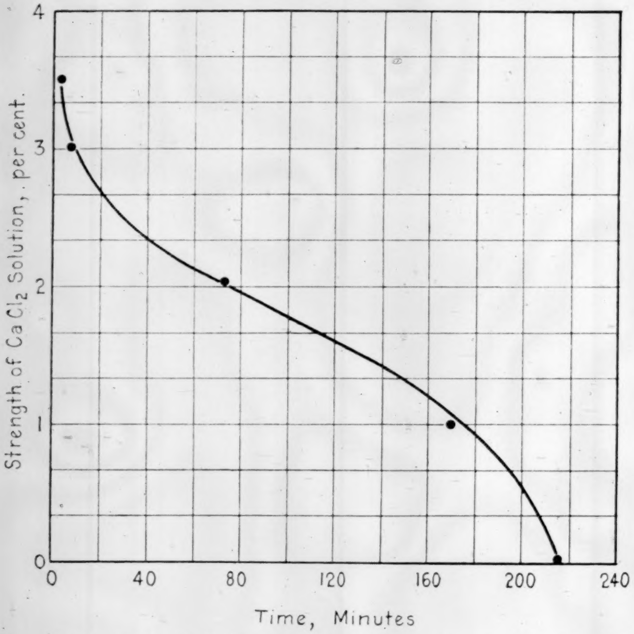

FIG. 9.—Effect of Calcium Chloride of Various Concentrations on the Initial Set of Cement

TABLE I.—TRANSVERSE STRENGTHS OF TEST SPECIMENS CONTAINING VARIOUS QUANTITIES OF CALCIUM CHLORIDE

Treatment	Transverse Strength, lb. per sq. in.		Flow	Temperatures, deg. Fahr.	
	14-Day	28-D		Air	Concrete
No treatment	185	230	185	26 to 28	40
2 per cent $CaCl_2$ (Incorporated)[a]	171	222	190	"	"
4 per cent $CaCl_2$ (Incorporated)[a]	228	249	180	"	"
6 per cent $CaCl_2$ (Incorporated)[a]	167	234	175	"	"
No treatment	87	123	180	19 to 22	55
2 per cent $CaCl_2$ (Incorporated)[b]	156	138	170	"	"
4 per cent $CaCl_2$ (Incorporated)[b]	168	201	185	"	"
6 per cent $CaCl_2$ (Incorporated)[b]	186	255	170	"	"
10 per cent $CaCl_2$ (Incorporated)[b]	176	256	165	"	"

[a] Eight hours after these specimens were made snow fell on them for ten hours.

[b] Warm water and partially heated aggregates were used in these specimens, so as to bring the temperature of the concrete as placed to 55° F.

initial set as stated by the Vicat test. It has been further ascertained that different brands of cement, although standard with respect to general specifications, will show a variation in their carrying capacities of calcium chloride.

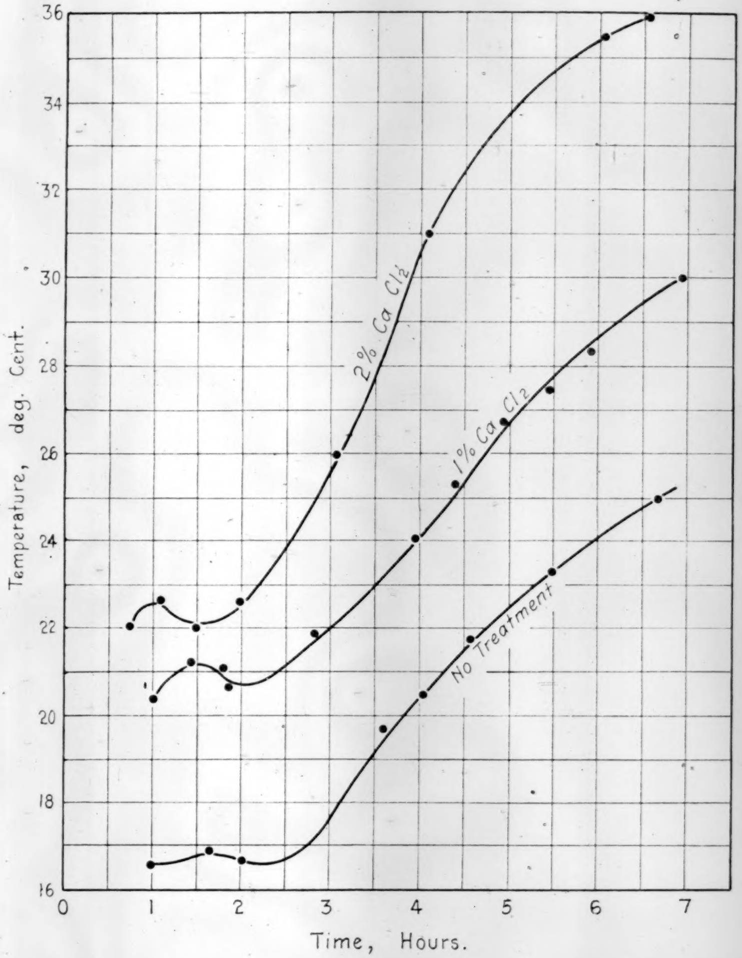

FIG. 10.—Effect of Calcium Chloride on the Internal Temperature of Cement

Strength in Low Temperatures.—The investigations conducted with concrete specimens poured in low temperatures merely illustrated the fact that even the presence of a large percentage of calcium chloride will not allow the placing of concrete in freezing temperatures without some provision being made to protect the concrete from the cold weather. A number of slabs, 8 by 12 by 30 in., were made up for transverse test specimens with various percentages of calcium chloride incorporated. The specimens were poured in a temperature which varied from 22 to 28° F. The results secured from this investigation are contained in Table I. Probably the most striking feature of this experiment was the failure of the specimens to gain strength past the 14-day period. Though a fair strength was secured at the end of two weeks by those specimens containing calcium chloride, no additional strength of appreciable amount was developed at later ages.

Internal Temperatures.—Considerable time was spent on the study of the variation of internal temperatures caused by the incorporation of the different amounts of calcium chloride. Specimens were made up of both neat cement and concrete and the changing temperatures observed through the use of thin brass tubes containing a light oil in which a sensitive thermometer rested. The tubes were placed in the specimens before a set took place. The open ends of these tubes were fitted with corks through which holes were bored to allow the insertion of the thermometers. The top of the tubes were set ½ in. below the surface of the specimen in order that the brass would not be exposed to the air temperatures.

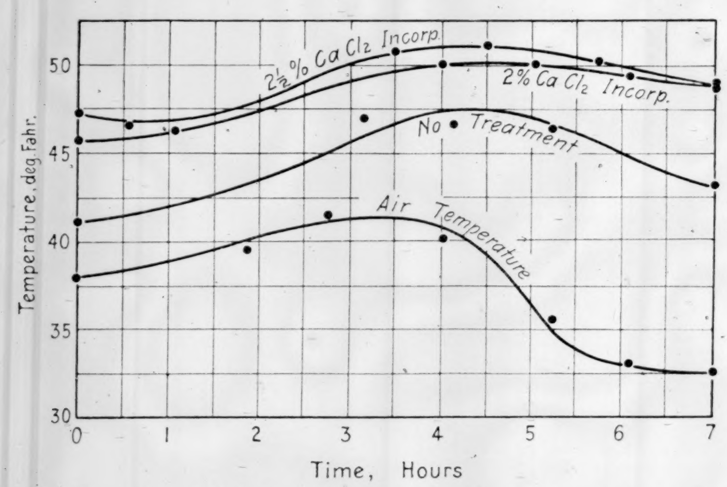

FIG. 11.—Effect of Calcium Chloride on the Temperature of Concrete

Fig. 10 contains curves showing the change in temperatures in the neat cement specimens. The small hump in the curves near the origin is thought to be due to the heat generated by the decomposition of the aluminates at the time of the initial set. The variation in the slope of the curves illustrates that a greater heat is generated by the specimens containing the larger percentage of calcium chloride than in the others. It is also apparent that the initial temperature is greater in the calcium chloride incorporated specimens than in those which received no internal treatment.

TABLE II.—VARIATION IN COMPRESSIVE STRENGTHS OF VARIOUS BRANDS OF CEMENT CONTAINING DIFFERENT CONCENTRATIONS OF CALCIUM CHLORIDE

Brand	Calcium Chloride, per cent	Compressive Strength, lb. per sq. in.				Brand	Calcium Chloride, per cent	Compressive Strength, lb. per sq. in.			
		7-Day	28-Day	60-Day	90-Day			7-Day	28-Day	60-Day	90-Day
A	0	3582	4103	4590	4731	G	0	3620	5527	4711	5757
	1	3212	4053	3524	5056		1	2975	3990	4772	5925
	2	3167	4351	5275	4793		2	3123	5684	5013	6058
	3	3910	4110	5502	4547		3	3562	4732	5655	5257
	4	1997	3571	5070	5352		4	3119	4934	6030	6825
	5	2238	2825	4952	5117		5	3063	3019	4955	5573
	7	2507	3909	4150	5783		7	2873	3770	4750	5635
	10	2094	3637	3140	5282		10	1800	3458	3270	4982
B	0	3112	4317	3985	5270	H	0	2547	4227	4570	5134
	1	3302	4017	4085	4936		1	3108	3962	4405	4827
	2	3257	4471	4296	6086		2	2717	4592	5597	5354
	3	3320	4103	5222	5583		3	3093	4419	4487	5415
	4	2169	4102	5180	6147		4	2191	2898	5435	5761
	5	1996	3183	4493	5744		5	2325	4278	4350	6879
	7	1926	4009	5430	5603		7	2420	3078	3865	5800
	10	1563	2961	3830	4556		10	1517	2460	3730	4479
C	0	2202	3285	3752	4482	I	0	2592	3943	4253	4470
	1	2215	3117	3746	3988		1	2678	3922	4511	4692
	2	1915	3007	3663	4427		2	3055	4131	3942	4958
	3	1851	2726	3709	3998		3	2600	4254	4495	4802
	4	1528	2515	4370	4266		4	2241	3489	4280	5139
	5	1795	2828	4120	4050		5	2679	3266	4130	4601
	7	1370	2532	6347	6035		7	2026	3602	5505	6108
	10	1239	2274	3978	5553		10	1884	3068	4563	4904
D	0	2132	3382	3728	4681	J	0	2592	3827	4733	4254
	1	1908	3387	4012	4280		1	2371	4540	4889	4656
	2	2170	3214	4155	4404		2	3170	5030	5224	5544
	3	2130	3215	4554	5336		3	3222	5414	5544	5130
	4	1513	2988	4650	4727		4	2963	3589	5310	5054
	5	1682	2554	4103	5026		5	2532	3750	3810	5358
	7	1505	2787	4070	5373		7	2347	3929	4590	5804
	10	1188	2341	3140	4743		10	1982	4393	4847	4922
E	0	2699	3977	4514	4911	K	0	1828	2840	3966	4122
	1	2763	4140	3568	4686		1	1608	3235	4364	5036
	2	3117	4277	4895	4445		2	1793	2714	4559	5875
	3	3740	4892	4535	4866		3	2147	3374	4929	5613
	4	2261	3724	5070	5245		4	1967	2653	3950	5158
	5	2376	3444	4650	3950		5	1628	3230	3970	4958
	7	2251	3684	4300	5400		7	1526	3145	5065	5294
	10	1529	3338	3710	5279		10	1142	3087	3860	5232
F	0	3243	4157	4948	5114	L	0	2220	3487	4487	4700
	1	3408	4153	3980	5820		1	2160	4080	4450	4380
	2	3418	4185	4380	5313		2	2140	3245	5185	6532
	3	3345	4600	4559	4766		3	2765	4087	4352	6072
	4	3210	3992	5270	4419		4	2075	3247	4390	3751
	5	2293	3711	4150	6144		5	1876	3567	5210	5932
	7	2419	4111	4817	5178		7	2000	3457	5182	4903
	10	2430	3285	5968	6490		10	2056	3572	5785	4113

Fig. 11 contains curves which show the variation in internal temperatures observed in concrete specimens containing calcium chloride. It is again illustrated in these curves that higher initial temperatures are secured in the incorporated specimens than in those receiving no treatment. Though the specimens containing no calcium chloride show internal temperatures exceeding the air temperatures, they fail to approach the temperatures indicated in the incorporated specimens.

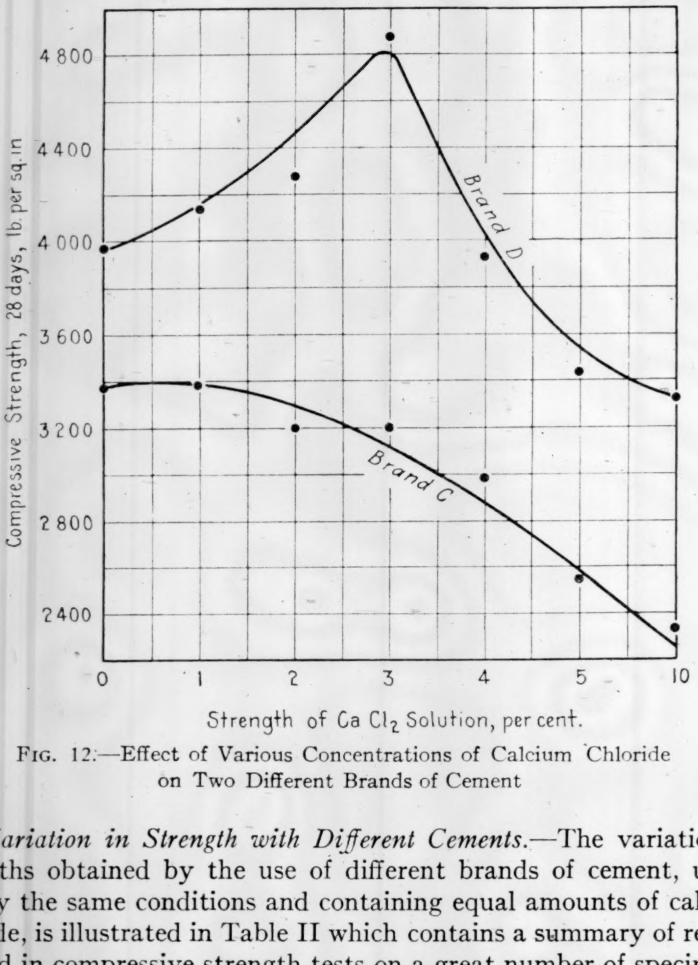

FIG. 12.—Effect of Various Concentrations of Calcium Chloride on Two Different Brands of Cement

Variation in Strength with Different Cements.—The variation in strengths obtained by the use of different brands of cement, under exactly the same conditions and containing equal amounts of calcium chloride, is illustrated in Table II which contains a summary of results secured in compressive strength tests on a great number of specimens. Twelve different brands of cement were used, all of which passed state specifications for both physical and chemical tests. The amounts of calcium chloride used varied from 1 to 10 per cent, specimens of each cement being made for each percentage. As seen in this table, the increased strength at all ages due to the use of calcium chloride is pronounced.

Fig. 12 is included to bring out more strikingly the variation in strengths which is often secured with various brands of cement subjected to the same amount of calcium chloride. It is quite evident from these curves that no common relation exists between the specimens of the different cements. Up to the point of 3-per-cent incorporation, the slopes of the curves are directly opposite. Though the strengths of both sets of specimens decrease with each increase in percentage of calcium chloride exceeding 3 per cent, it cannot be said that the curves indicate a similarity in the properties of the two cements. The failure of these two cements to show more uniform results is thought to be due to a variation in the basicity of the cements which causes the calcium chloride to hydrolize differently.

Study of Hydration.—As was previously stated, the work in the investigation of hydration has not progressed far enough to permit of the formation of any definite statements. However, it has been fairly well established that various brands of standard cements hydrate four to five times as fast as others when incorporated with a constant solution of calcium chloride. The work in connection with this study included the making, breaking, regrinding, and retesting of the specimens. The specimens used were compression test cylinders of both neat cement and mortar. After the first testing, the specimens were reground to a fineness which allowed 80 per cent to pass a 200-mesh sieve. By using various concentrations of calcium chloride up to 10 per cent to gage the neat cement cylinders, it was possible by regrinding and retesting to determine the percentage hydration of the cement containing the chemical as compared to those specimens which were entirely free from calcium chloride. In the remaking of the specimens, distilled water was used. The results obtained to date show that when the solution contains more than 4 per cent of calcium chloride the reground specimens decrease in strength rapidly. The effect of calcium chloride on the original cement was to hasten the setting time, while with the reground specimens which originally contained calcium chloride, a very slow set was observed. Though the reason for this action has not as yet been found it is probable that the calcium chloride hydrated more cement particles than is usually the case where water alone is used. The peculiarity of set is very apparent in the specimens containing a 10-per-cent solution of calcium chloride. The initial set of the original specimen was present in five minutes and the final set was secured at 45 minutes. After this specimen was reground and remade, the initial set took place in seven hours while the final set was not secured until 30 hours had elapsed.

Results of Investigation on Incorporating Calcium Chloride in Pavements in Illinois.—The results secured in these investigations were such that the Illinois Division of Highways was satisfied that calcium chloride could be incorporated in concrete to advantage under certain conditions. Consequently, its usage is allowed in cold weather construction upon the permission of the engineer in charge of the job. To insure the proper supervision, the Department has issued a circular explaining how this material must be used. The circular reads as follows:

"The Department will permit the use of calcium chloride in the concrete mix for pavement construction during cold weather. When used, the solid, granulated, flaked, or powdered materials shall be thoroughly dissolved in water. It is recommended that the proper amount of calcium chloride solution should enter the mixer drum just before the water is added to mix the aggregates, or the solution may be added direct to the stone or sand when in the skip, but in either case the method of handling this solution shall be approved by the Engineer.

"When used in the pavement work, not more than two per cent by weight of the cement shall be used.

"Calcium chloride should not be used in a pavement that crosses an electric railroad, but its use should be discontinued approximately one quarter of a mile from the electric railroad crossing. There is no objection to using the material when the highway parallels an electric line. Calcium chloride shall not be used *in pavements which are reinforced.*

"It is not definitely known at what temperature concrete will freeze when the two per cent of calcium chloride is mixed with the concrete and it will, therefore, be necessary to arrange to cover the pavement with loose straw or earth as soon as it has taken sufficient set to prevent marring of the surface. In other words, calcium chloride hastens the setting of the concrete but is in no sense a protective. It makes possible an earlier application of a straw or earth covering.

"Due to its action on reinforcing steel, calcium chloride shall not be used in bridge or culvert construction; therefore, the usual precautions should be taken when placing concrete in culverts and bridges during temperatures around freezing point.

"All material should be purchased with the guarantee that it contains from 70 to 75 per cent calcium chloride and before using samples should be submitted to the testing laboratory, as in the case of any other materials."

DISCUSSION

Mr. Mattimore

Mr. H. S. Mattimore[1] (*presented in written form*).—The use of calcium chloride as an accelerator in the hardening of concrete, has, to my knowledge, been experimented with and tried in practice on a small scale for at least ten years. Unfortunately many of the published data are widely scattered and some that I have consulted have apparently not been handled in a scientific manner, which makes it very difficult to draw conclusions. Within the past several years, the study of this subject has been intensified, and there is now convincing evidence that the incorporation of this salt in a concrete mix in the proper proportions does accelerate the early hardening.

My interest in the subject has dated back for ten years and upon examining data accumulated during that time, some interesting phases of the action of this salt when incorporated in concrete, may furnish a few interesting points for future research work. During the course of investigation, this problem was attacked from several standpoints; although far from being of an exhaustive nature, the results of these experiments, to my mind at least; give a fairly reliable indication of some advantages secured by the use of calcium chloride in concrete, and of the conditions under which it seems the most efficient.

The first phase is the action of calcium chloride on several different brands of cement. The specimens were 2-in. cubes of neat cement with calcium chloride added as a 4-per-cent solution to the mixing water.

Cement	Condition	Compressive Strength, per cent				
		3 Days	7 Days	14 Days	21 Days	28 Days
Standard Blend	Plain	100	100	100	100	100
" "	With 4 per cent $CaCl_2$	120	125	104	112	113
Commercial Cement	Plain	135	103	87	97	95
" "	With 4 per cent $CaCl_2$	155	117	107	112	118

One of the interesting facts of the above table is the difference in the percentage of increase on the compression of the different brands during the three and seven-day periods. From the tests at longer periods we realize that the action of this salt on different cements is a factor which has to be taken into consideration in using it.

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[1] Engineer of Tests, Pennsylvania State Highway Commission, Harrisburg, Pa.

Mr. Mattimore The effect of calcium chloride under different temperature conditions was tested by making a number of 2-in. mortar cubes of $1 : 1\frac{1}{2}$ mix and subjecting some to the normal indoor temperature of from 60 to 68° F., while others were stored outdoors at temperatures of from 30 to 44° F.

The increase in strength of out-door specimens containing calcium chloride, over the plain specimens under the same storage is an indication of the real efficiency of this product under such conditions. Also Table I, showing the strength at various periods from one to twenty-eight days, demonstrates that the acceleration is manifested at early periods.

TABLE I.—COMPRESSIVE STRENGTH OF MORTAR

Age	Curing	Temperature, deg. Fahr.	Plain Mortar		Mortar with 4 per cent CaCl	
			lb. per sq. in.	Per cent	lb. per sq. in.	Per cent
1 day	Indoor	68	500	100	900	180
	Out-door	42	100	100	200	200
2 days	Indoor	60, 68	1250	100	1720	137
	Out-door	39, 42	180	100	600	332
3 days	Indoor	60, 68	1920	100	2430	126
	Out-door	38. 42	420	100	1190	283
4 days	Indoor	60, 68	2430	100	2850	117
	Out-door	38, 42	740	100	2050	278
5 days	Indoor	60, 68	2750	100	3540	129
	Out-door	38, 44	1640	100	2500	152
6 days	Indoor	60, 68	3400	100	3650	108
	Out-door	38, 44	1950	100	2920	149
7 days	Indoor	60, 68	3520	100	4060	115
	Out-door	36, 44	1250	100	3080	268
8 days	Indoor	60, 68	3780	100	4300	114
	Out-door	36, 44	1670	100	3600	215
9 days	Indoor	60, 68	3780	100	4620	122
	Out-door	36, 44	2320	100	3950	169
10 days	Indoor	60, 68	3910	100	4240	107
	Out-door	36, 44	2280	100	3580	157
11 days	Indoor	60, 58	3950	100	4890	124
	Out-door	36, 44	3120	100	3880	124
12 days	Indoor	60, 68	3960	100	4760	121
	Out-door	36, 44	2920	100	3900	133
13 days	Indoor	60, 68	4100	100	4610	112
	Out-door	34, 44	2580	100	3980	154
1 days	Indoor	56, 68	....	...	4930	...
	Out-door	32, 44	2960	100	4120	139
28 days	Indoor	56, 68	5200	100	6150	118
	Out-door	32, 44	3550	100	4280	121

In order to judge of the effect of calcium chloride in mortar at longer periods, the following series of tests on mortar of $1 : 1\frac{1}{2}$ mix

Mr. Mattimore

was carried out to ages of twelve months. These cubes were cured in a laboratory water storage tank.

	Compressive Strength			
	Plain Concrete		Concrete with 4 per cent $CaCl_2$	
	lb. per sq. in.	Per cent	lb. per sq. in.	Per cent
3 days	3500	100	4050	116
7 "	4200	100	4900	117
28 "	5200	100	6200	119
3 months	6200	100	6850	111
6 "	6450	100	6900	107
12 "	8760	100	8810	101

This table indicates that the efficient use of this product is obtained within early periods. At twelve months, the mortar containing calcium chloride has about the same compressive strength as the plain concrete.

The following table gives the results of tests on plain concrete of 1 : 1½ : 3 mix and concrete of the same mix containing a 4-per-cent solution of calcium chloride:

Concrete	Curing	Temperature, deg. Fahr.	Compressive Strength at 7 Days		Compressive Strength at 10 Days		Compressive Strength at 28 Days	
			lb. per sq. in.	Per cent	lb. per sq. in.	Per cent	lb. per sq. in.	Per cent
Plain	Indoor	49 to 54	3150	100	3750	100	4200	100
With 4 per cent $CaCl_2$	Indoor	49 to 54	3750	119	3900	104	4750	113
Plain	Out-door	14 to 42	2050	100	2600	100	2750	100
With 4 per cent $CaCl_2$	Out-door	14 to 42	2500	122	3150	121	3800	137

The low temperature under which these out-door specimens were stored is especially interesting and indicates probably one of the main uses of this salt when incorporated in a concrete mix.

The practical application of the use of calcium chloride as an accelerator in a concrete mix is illustrated in a concrete roadway placed at several bridge approaches over which traffic was admitted seven days after the roadway was placed. The following series of tests give the compressive strength of concrete cores taken from this roadway when six months old:

Number of Specimens		Compressive Strength, lb. per sq. in. Maximum	Minimum	Average
2	Plain concrete	4851	3476	4163
4	4-per-cent solution $CaCl_2$	3559	3053	3230
4	2-per-cent solution $CaCl_2$	4175	3218	3747

Mr. Mattimore

A study of this table is especially interesting in that the concrete as tested is as near representative of the concrete in place as it is possible to obtain. The following points should be considered in analyzing a table of this character: In the first place the concrete containing calcium chloride shows a lower strength at the six months' period than the plain concrete, but consideration must be given to the fact that the lowest strength on the calcium chloride concrete is in excess of 3000 lb. per sq. in. which we consider a safe value for any structure.

The plain concrete slabs were cured by being kept wet for eight days and were not opened to traffic for twenty-one days, while the calcium chloride concrete was kept wet for only two days, from that time during the remainder of the seven-day period it was not covered, while at seven days it was opened to a normal traffic on one of our main routes.

Attention is also called to the fact that the concrete with a 2-per-cent solution of calcium chloride shows higher compressive strength than does the concrete to which a 4-per-cent solution has been added. Recent data on calcium chloride concrete check this result.

These concrete slabs were reinforced with cold-drawn wire mesh, and an examination of the steel showed no detrimental action of the calcium chloride.

My experience with calcium chloride has been limited to its use when incorporated in the mix. In this condition it has been found to act as an accelerator and to some extent as a curing agent. No attempt has been made to apply it on the surface, since up to the present time our experience has lead us to believe that we should have it uniformly distributed through the concrete section to preclude any difference across the section in curing or acceleration of hardening.

Mr. Clemmer

MR. H. F. CLEMMER.—As a result of obtaining the same strength at the end of 14 days by the use of calcium chloride that we do by curing with wetted earth or ponding over a period of 28 days, on a good many jobs where we have used calcium chloride, we have opened the roads at the end of 14 days and have even hauled heavy loads over them in some cases since it often happens that a contractor wants to use his own road for hauling. In the cases where we have opened the road at the end of 14 days there have been no ill effects whatever.

Mr. Slater

MR. W. A. SLATER.[1]—I want to ask about conditions under which this calcium chloride was applied. Does the calcium chloride remain on the surface, and what treatment is given to it at the same time? Is it applied in that way under all conditions of weather?

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[1] Engineer-Physicist, U. S. Bureau of Standards, Washington, D. C.

Mr. Clemmer

MR. CLEMMER.—Ordinarily it is recommended that calcium chloride be applied in the morning, simply because the air at that time is a little more moist and for this reason it requires a shorter length of time for the calcium chloride to reach a condition in which it may be broken up. The material is applied by simply shoveling on to the pavement and then raking or brooming to obtain an even distribution. It is applied regardless of the condition of the day, whether sunshiny or not.

No water is applied when calcium chloride is used and there is also no necessity for removing the calcium chloride. The material does leave an oily film on the surface which, I imagine, has some effect in preventing evaporation. The calcium chloride, however, is not removed.

Mr. Slater

MR. SLATER.—What does it cost per square foot?

Mr. Clemmer

MR. CLEMMER.—About $4\frac{1}{2}$ cents per square yard and in many cases much less. The contractor, however, is not required to clean the pavement afterward which is a factor to be taken into consideration. I am not certain how much pavement we are curing with calcium chloride but it is at least three-fourths of our entire program which will reach 1000 miles this season.

Mr. Harsch

MR. RAYMOND HARSCH.[1]—An investigation similar to that described by Mr. Clemmer was conducted by the U. S. Bureau of Public Roads the early part of this year. Treatments were applied on the surface of the concrete in all but one case, and included, besides calcium chloride, applications of water-gas tar, coal-gas tar, and one patented hardener. Concrete which was untreated, and some which was cured with a damp earth covering, was made for comparison.

Preparation of the Slabs.—On a uniform subgrade, 10 plain concrete slabs 7 ft. square and 6 in. thick were constructed. Five slabs were laid the first day and the remaining five the following day. As the concrete had the consistency shown by a 4 in. slump no tamping was used but the surface was merely worked smooth and level by means of a steel-edge strike board. The aggregates, consisting of Potomac River sand and $\frac{3}{4}$-in. gravel, were proportioned by volume to give a 1 : 2 : 4 mix, the correct proportion being maintained by measuring the aggregates for each batch. Mixing was accomplished in a one-bag per batch mixer. The consistency was determined at least once for each slab to check the judgment of the operator controlling the water. Iron bolts, measuring $\frac{3}{4}$ in. by 8 in. were driven into the concrete in four lines, spaced 1 ft. apart, and removed the following day

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[1] Junior Assistant Testing Engineer, U. S. Bureau of Public Roads, Washington, D. C.

Mr. Harsch forming holes used in splitting the slabs. The slabs were treated as follows:

Slab No. 1.—No precautions taken in curing the concrete.

Slab No. 2.—Covered with 2 in. of sand and kept soaked with water for ten days. The sand was then removed and the slab exposed to the weather.

Slab No. 3.—This slab was sprinkled with dry calcium chloride at the rate of 3 lb. per sq. yd. three hours after the concrete was poured. It was protected from rain by a canvas cover, allowing the calcium chloride to remain for fifteen days. The surface was then scrubbed with water to remove the remaining material.

Slab No. 4.—Calcium chloride was sprinkled in the same quantity as above twelve hours after the slab was placed. After remaining twenty-four hours it was removed by washing. Slab exposed to weather.

Slab No. 5.—Treated the same as Slab No. 3, except 2 lb. per sq. yd. were used.

Slab No. 6.—Covered twenty-four hours after placing the concrete with a coating of water-gas tar, at the rate of 0.1 to 0.2 gal. per sq. yd., forming a uniform and impervious covering.

Slab No. 7.—Treated the same as Slab No. 4, except that 2 lb. per sq. yd. of calcium chloride were used.

Slab No. 8.—Same as Slab No. 6, using coal-gas tar as a covering.

Slab No. 9.—Sprinkled with 2 lb. commercial patented hardener (essentially a compound containing $CaOCl_2$) mixed in one gallon of water. Allowed to dry in air.

Slab No. 10.—Twenty-seven and one-half pounds of a commercial patented hardener, mixed with the concrete at the rate of one gallon per sack of cement. No other treatment given.

For each slab there were molded at the same time three 6 by 12-in. compression test cylinders and two 6 by 8 by 48-in. beams. The cylinders were not given any treatment except those representing Slab No. 10, which were made of the concrete containing the patented hardener as an ingredient. The beams, however, were treated the same as the slabs. Each slab was split into four beams 12 in. wide and 7 ft. long, leaving a remaining portion of 3 by 7 ft. from which cores were drilled. Six 2 by 4-in. cores were drilled from the corner pieces of these slabs by means of a diamond core drill. Three were drilled from the top and three from the bottom of the slab. All of these cores were capped with a 1 : 1 mortar, using a 2-per-cent solution of calcium chloride for the mixing water. They were brought to a uniform height of 4 in. by means of a capping mortar. From the

Mr. Harsch

remaining 3 by 7-ft. slab four $4\frac{1}{2}$ by 6-in. cores were drilled with the Calyx steel shot drill. These cores were capped similar to the smaller ones except that the height was kept as nearly 6 in. as practicable.

TABLE II.—MODULUS OF RUPTURE OF CONCRETE BEAMS IN CURING TEST

Slab	12 by 6 in. by 7 ft. Beam	Modulus of Rupture, lb. per sq. in.	Average	6 by 8 by 48 in. Beam	Modulus of Rupture, lb. per sq. in.	Average
No. 1	A B	324 377	351	A	427	443
	C D	369 345	357	B	459	
No. 2	A B	600 499	550	A	517	521
	C D	401 453	427	B	525	
No. 3	A B	452 446	449	A	397	357
	C D	437 285	361	B	316	
No. 4	A B	441 411	426	A	369	392
	C D	455 423	439	B	414	
No. 5	A B	405 329	367	A	283	472
	C D	387 ...	387	B	661	
No. 6	A B C	473 353 344	390	A	245	267
	D	368	368	B	288	
No. 7	A B	432 438	435	A	383	368
	C D	426 471	449	B	353	
No. 8	A B	328 281	305	A	289	341
	C D	420 398	409	B	392	
No. 9	A B	420 420	420	A	396	412
	C D	495 422	459	B	428	
No. 10	A B	379 257	318	A	308	349
	C D	347 304	326	B	390	

Beam Tests.—The beams were tested at a center-point loading, a 42-in. span being used for the smaller beams and a 72-in. span for the 7-ft. beams. Two of the 12 by 6-in. by 7-ft. beams were tested

Mr. Harsch as molded and two in an inverted position. All of the small beams were tested as molded. Table II gives the results of the tests of all the beams.

TABLE III.—RESULTS OF COMPRESSION TESTS ON 2 BY 4-IN. CYLINDERS

Slab	Treatment	Compressive Strength, lb. per sq. in.	
		Drilled from Top	Drilled from Bottom
No. 1	No treatment given	3810	3995
		4120	3970
		3540	5110
		Average 3825	4358
No. 2	Covered with 2 in. of sand and kept wet for 10 days	3470	3850
		3365	3680
		3410	3840
		Average 3415	3790
No. 3	3 lb. $CaCl_2$ per sq. yd. for 15 days	3645	3815
		4090	3370
		4110	3770
		Average 3948	3652
No. 4	3 lb. $CaCl_2$ per sq. yd. for 24 hours	3670	3370
		3350	3770
		3930	3815
		Average 3650	3652
No. 5	2 lb. $CaCl_2$ per sq. yd. for 15 days	3450	2780
		3415	3180
		3945	3060
		Average 3603	3007
No. 6	1.5 gal. water-gas tar per sq. yd. 24 hours after pouring slab. Applied cold	2690	3330
		3315	5440
		2490	3850
		Average 2832	4207
No. 7	2 lb. $CaCl_2$ per sq. yd. for 24 hours	2720	....
		2930	3675
		2790	4740
		Average 2817	4208
No. 8	1.5 gal. coal-gas tar per sq. yd. 24 hours after pouring slab. Applied cold	2885	4210
		2925	4005
		2900	3645
		Average 2903	3953
No. 9	Sprinkled with 2 lb. patented hardener ($CaOCl_2$) mixed in 1 gal. of water	2970	4910
		3300	4870
		3780	4880
		Average 3350	4887
No. 10	Patented hardener (essentially a $CaOCl_2$ compound) mixed in concrete at rate of 1 gal. per bag of cement	3350	4360
		3080	4165
		3420	3860
		Average 3283	4128

A and B of the large-sized beams were tested as poured and C and D were tested with the treated surface downward.

Cylinder Test.—All the cylinders were tested for compressive strength. The results are given in Table III.

Mr. Harsch

Results of Curing Tests.

Appearance of Surface.—Slab No. 1, which was untreated, showed a surface resembling dry aged concrete within 48 hours after pouring. There were, however, no shrinkage cracks visible from too rapid drying.

Slab No. 2, which had been covered with 2 in. of sand at the age of twenty hours and this was allowed to remain for ten days, being sprinkled every day, showed a surface resembling that of Slab No. 1 after removing the covering and exposing to the weather.

Calcium chloride treatment when applied at the rate of 2 or 3 lb. per sq. yd. several hours after pouring the concrete and left on for fifteen days had a deleterious effect on the concrete. Instead of the surface becoming hard and acquiring a set, the concrete for a depth of ½ to 1 in. remained mushy and soft. There was a slight swelling of the surface material under a thin crust of a calcium chloride and mortar scale. When the surface was washed at the end of the 15 days much of this scale came off leaving the surface pitted and rough. When cores were drilled with the steel shot drill, the concrete was shown to be weakened to a depth of over 1 in.

The same treatment when removed after twenty-four hours produced no scaling of the surface. The slabs did not show as hard a surface at the age of one day as the untreated slab.

The treatment of the surface with tar did nothing more than form a coating which remained soft and sticky for several weeks, a condition probably aggravated by hot weather. There was no penetration of the tar into the pores of the concrete.

Surface treatment of a patented hardener mixed with water produced a very white surface having the appearance of being painted with a flat white paint. Rain did not remove this coating. The surface became hard sooner than the untreated surfaces poured the same day.

Slab No. 10, which contained the patented hardener as an integral part of the mix, had a slightly darker gray color than the untreated slab. It retained this gray appearance as compared to the whitening of the untreated concrete upon aging in the weather. The effect of this treatment was a rapid set of the concrete. The concrete became hard enough in one hour to make the placing of bolts quite difficult.

Tests of 6 by 12-in. Control Specimens.—Table V, giving the results of compression tests, shows the concrete to be uniform during each day, but considerable variation between the strengths for the two days. The tests of slab core specimens do not agree with this

Mr. Harsch

TABLE IV.—RESULTS OF COMPRESSION TESTS ON $4\frac{1}{2}$ BY 6-IN. CORES IN CURING TEST

Slab		Compressive Strength, lb. per sq. in.	Slab		Compressive Strength, lb. per sq. in.
No. 1	*a*	3164	No. 6	*a*	3272
	b	3580		*b*	3333
	c	3415		*c*	3924
	d	3250		*d*	3798
		Average 3352			Average 3332
No. 2	*a*	3490	No. 7	*a*	2926
	b	3225		*b*	3570
	c	3115		*c*	2379
	d	2895		*d*	2905
		Average 3181			Average 2945
No. 3	*a*	4381	No. 8	*a*	3183
	b	3367		*b*	3150
	c	3570		*c*	3070
	d	3990		*d*	3170
		Average 3577			Average 3143
No. 4	*a*	2700	No. 9	*a*	2880
	b	3089		*b*	3191
	c	3696		*c*	3333
	d	2775		*d*	3812
		Average 3065			Average 3304
No. 5	*a*	3720	No. 10	*a*	2780
	b	2660		*b*	2635
	c	3038		*c*	2955
	d	3975		*d*	3318
		Average 3348			Average 2922

TABLE V.—RESULTS OF COMPRESSION TESTS ON 6 BY 12-IN. CYLINDERS IN CURING TEST

Slab		Compressive Strength, lb. per sq. in.	Slab		Compressive Strength, lb. per sq. in.
No. 1	*a*	2365	No. 6	*a*	1165
	b	2000		*b*	1400
	c	2225		*c*	1210
		Average 2197			Average 1258
No. 2	*a*	2240	No. 7	*a*	1341
	b	2240		*b*	1557
	c	2364		*c*	1247
		Average 2281			Average 1382
No. 3	*a*	2660	No. 8	*a*	1366
	b	2445		*b*	1223
	c	2440		*c*	1085
		Average 2515			Average 1225
No. 4	*a*	2130	No. 9	*a*	1202
	b	2470		*b*	1148
	c	1970		*c*	1775
		Average 2190			Average 1375
No. 5	*a*	2115	No 10	*a*	2270
	b	1990		*b*	2280
	c	2240		*c*	2432
		Average 2115			Average 2327

Mr. Harsch difference in quality of concrete poured on the two days. Specimens representing Slab No. 10 showed much higher strength than the other cylinders made the same day but only equal to those of the first day's molding. As these were made under special supervision the high strength may be attributed to better molding.

The conclusion to be drawn from these results is that the concrete was of uniform quality for each group of slabs.

Tests of 2 by 4-in. Cylinders.—The results given in Table III seem to indicate that there is a decided curing effect from the subgrade itself. Only in the case of the 15-day treatments of calcium chloride were the cylinders drilled from the top of the slab stronger than those from the bottom. The treatment of both the water-gas and coal tar caused the surface cores to be weak. This result was expected because the impervious coating prevented the additional moisture, necessary for complete hydration, to enter.

Tests of $4\frac{1}{2}$ by 6-in. Cores.—The results given in Table IV have not been corrected by a factor for the low ratio of the height to the diameter of the cylinder. There is so little variation in the compressive strength of cores from the different slabs that no general conclusion can be drawn. Attention is called to the fact that the cylinders from Slab No. 10 are the weakest in compression. This is opposite to the result from the 6 by 12-in. control specimens, and is more nearly representative of the concrete making up the slab.

Beam Tests.—The modulus of rupture (Table II) obtained from the two sets of beams agree very well. As in the case of the 2 by 4-in. cores, the tar treatment produced a weak concrete shown by tests of beams from Slabs Nos. 6 and 8. The highest strength was obtained from the slab kept covered with wet sand for ten days. The difference found by testing beams with the treated surface down was not pronounced enough to be an indication of the effect of the surface treatment, at the age of 28 days.

Mr. Clemmer MR. CLEMMER.—In the work of the Bureau of Public Roads which Mr. Harsch describes, Slab No. 2 was treated with a 2-in. layer of sand and kept sprinkled for ten days and on this slab the highest strengths were obtained. This method of curing, however, cannot be used as a comparison with wetted earth for the reason that sand, being composed of coarser grained particles, affords more voids and does not attain that degree of compactness found in earth especially when wetted, rewetted and exposed to the rays of the sun. It can easily be seen that considerably more free moisture would remain on the surface of the slab if covered by sand since, due to the coarseness of the material as well as the nature of the grain itself, very little

Mr. Clemmer effect of capillarity is obtained; whereas in the use of earth, originally a finely divided and compact material and gradually becoming more and more compact on wetting, a surprising degree of vertical capillarity and evaporation by the sun prevails and as a result very little free moisture remains on the surface of the slab.

Mr. Harsch also mentions that 2 by 4-in. cylinders were drilled from both the top and bottom of the slabs and tested for compressive strength. Considering the use of this size of cylinder, the strengths obtained and the conclusions drawn as a result of these tests should be disregarded. Standard practice recommended by this Society definitely specifies that the maximum size of the coarse aggregate for a 6 by 12-in. cylinder should not be over 2 in., or one-third the diameter of the specimen. If this standard practice is to be considered, the maximum size stone in a 2 by 4-in. cylinder should be $\frac{1}{2}$ in. To expect the results on the small cylinders to which Mr. Harsch refers to be indicative of the true strength of the concrete would be equivalent to expecting consistent results from 6 by 12-in. cylinders in which 4-in. stones had been used as aggregate. Considering also the fact that the cores were drilled from different parts of the slab it is impossible that the relative position of the coarse pieces of aggregate in the several cylinders were the same or nearly so and for this second reason the results obtained on these small cylinders are far from being representative of the strength of the concrete from which they were taken. This fact is even more pronounced when gravel is the coarse aggregate in place of crushed stone.

It has been the observation and experience of the Illinois Division of Highways that the cause of scaling, mentioned by Mr. Harsch as occurring on the slab treated with calcium chloride, can be attributed to excess tamping of the surface more than to any other cause or method of curing. It can be said definitely that in our experience with the use of calcium chloride we have not had a case of scaling which could be attributed to this material. In fact, the most serious cases of scaling have occurred on jobs where calcium chloride was not used.

The strengths on control specimens for the two different sets of slabs made, Mr. Harsch states, vary 100 per cent. Such a pronounced discrepancy suggests too wide a variation in qualities of concrete in the slabs themselves to consider the investigation of value regardless of curing.

Mr. Atwood MR. WILLIAM G. ATWOOD.[1]—In connection with quick-hardening cement for road work, attention is called to the French ciment fondu

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[1] Vice-President, Canadian Car and Foundry Co., Ltd., and Canadian Steel Foundries, Ltd., Montreal, Canada.

Mr. Atwood

or ciment electrique, which is a comparatively new product on the European market. It is being used to a large extent for pavement in Paris where they turn traffic on it in three days. It was used a great deal for gun foundations, under railway mounts and for pill boxes during the World War. It was found that heavy guns could be fired from these foundations under military conditions which required it within a day.

The only strength tests that I happen to have with me are reports of tests made at the French Government Laboratory at Bolougne—1 : 3 mortar mixed with sea water shows for a two-year period about 28 kg. per sq. cm. for French portland cement, and about 33 kg. per sq. cm. for ciment fondu in tension; in compression for the same period, portland cement shows 300 and ciment fondu about 604 kg. per sq. cm.

While not directly connected with this discussion on methods of hardening, it is a fact of interest to cement users that there is a cement other than portland on the market which hardens very much more quickly, has higher strength and so far as the tests go, since its invention, seems to avoid the drop in strength which generally occurs in portland cement about the sixth year. The French laboratories have these records for a period of about ten years.

Mr. Pearson

MR. J. C. PEARSON.[1]—Committee C-9 on Concrete and Concrete Aggregates included in its report for 1923 a report of a cooperative series of tests on accelerators.[2] Included in this report are the results of tests carried out by the Bureau of Standards. About six years ago when we first started our calcium chloride investigations, we put a number of 6 by 12-in. cylinders of the different mixes with embedded steel rods outside on the roof of our laboratory. Just the other day I took occasion to break a single set of those cylinders. The rods being 8 in. long were completely embedded in the cylinders and the latter were not intended for compression specimens, but I thought it would be interesting to note that in weather curing the minimum increase in strength of the specimens containing the chloride was 16 per cent and the highest 69 per cent above the plain concrete after six years' exposure to the weather. There is a much larger series in which we had rods partially embedded, and some of these were broken in one year, and again another set recently, at 5 years. As it is difficult to describe the effects of calcium chloride on steel, I have brought down these rods as we recovered them from the concrete specimens, and I am quite sure that anyone who is interested in the subject will be interested in examining the condition of these rods.

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[1] Physicist, U. S. Bureau of Standards, Washington, D. C.

[2] *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 220 (1923).

Mr. Pearson

I think that the majority of people would pass upon them as being entirely satisfactory. In one or two of the mortar mixtures, there is a surface corrosion that is not by any means deep, though it is general over the surface. In the case of the specimens buried in concrete, corrosion is absent over the greater portion of the surfaces of the rods, what rust occurred being localized apparently where voids occurred on the surface of the steel. Comparisons of the one-year and five-year specimens indicate that corrosion is not progressive.

FATIGUE OF MORTAR

By R. B. Crepps[1]

The importance of investigating the effect of repeated loads upon plain concrete is apparent not only from the standpoint of the economical design of road slabs, but also in the interpretation of failure occurring in concrete roads after years of service. It is a well-known fact that metals exhibit the phenomenon of fatigue, and it might naturally be expected that a material like concrete, which possesses only a small degree of elasticity and a large degree of plasticity, would exhibit a similar phenomenon.

The investigation of the fatigue element with respect to cement mortar is being carried on in the Testing Materials Laboratory of Purdue University as a cooperative project of the Engineering Experiment Station and the U. S. Bureau of Public Roads.

Apparatus

Deflections of a concrete slab occur in the form of a wave action as a wheel passes over the surface. The direction and nature of the resulting stress is constantly changing from tension or compression in the upper fibers to tension or compression in the lower fibers. With these facts in mind the idea of investigating the effect of reversed stresses upon concrete was conceived by W. K. Hatt and later incorporated in the design of an apparatus with which to carry on the investigation. The original device provided for the inspection of only one specimen at a time, while the improved machine allows four beams to be inspected simultaneously.

The machine illustrated in Fig. 1 consists mainly of a steel frame supporting test specimens, a motor and the essential working units. The principle of the application of the loads to the test specimen lies in the operation of an eccentric cam, which raises and lowers a load upon the ends of a horizontal straining member, supported in the center by a vertical test specimen. Four rocker arms, pivoted directly above the same number of test specimens, operate through a small angle by means of the cams rotated from a central line shaft. The U-shaped ends of the rocker arms fit around the cams as shown in the illustration. One end of a beam under test is clamped rigidly to the base of the steel frame and the upper end is clamped to the straining member. The operation of the machine subjects the outer fibers of the beam to alternate tension and compression at the rate of ten cycles per minute. A revolution counter attached to the machine indicates the actual number of reversals of stress for the separate beams.

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[1] Assistant Professor of Testing Materials, Purdue University, Lafayette, Ind.

Ten-inch Berry strain gages provided with Ames dials which read to 0.0001 in. are attached to both sides of the specimen, and observations of the total deformation from tension to compression are taken at designated intervals throughout the period of tests.

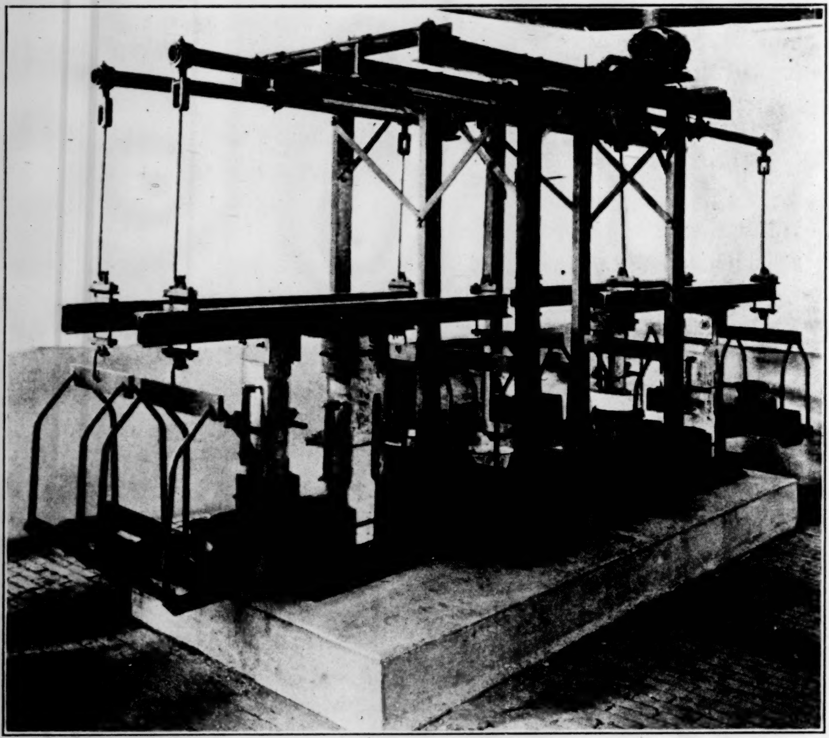

FIG. 1.—Fatigue Machine for Testing Concrete

TEST SPECIMENS

The original specimens were 30 in. long and 4 by 4 in. in cross-section. It was found, however, that beams of uniform cross-section usually failed outside of the strain gage range near the lower support. In order to localize the failure within the strain gage points the cross-section, outside of the 10-in. gage length, was increased to 4 by 6 in.

Mortar specimens of a 1:2 mix have been used in the investigation up to the present time.

STRENGTH OF SPECIMENS

Fig. 2 illustrates the procedure used in determining the strength of the specimens. Duplicate test beams supported as in the fatigue machine are broken by the application of static loads in 10-lb. increments on one end of the straining beam. The strength factor as determined by the static-load breaks is considered as representative of the particular series of beams under question. In spite of all possible precaution observed in the construction of the test specimens, the maximum variation in strength from an average of six beams was about 15 per cent. It is appreciated that this variation creates a source of considerable error in the attempt to arrive at definite conclusions concerning fatigue of mortar.

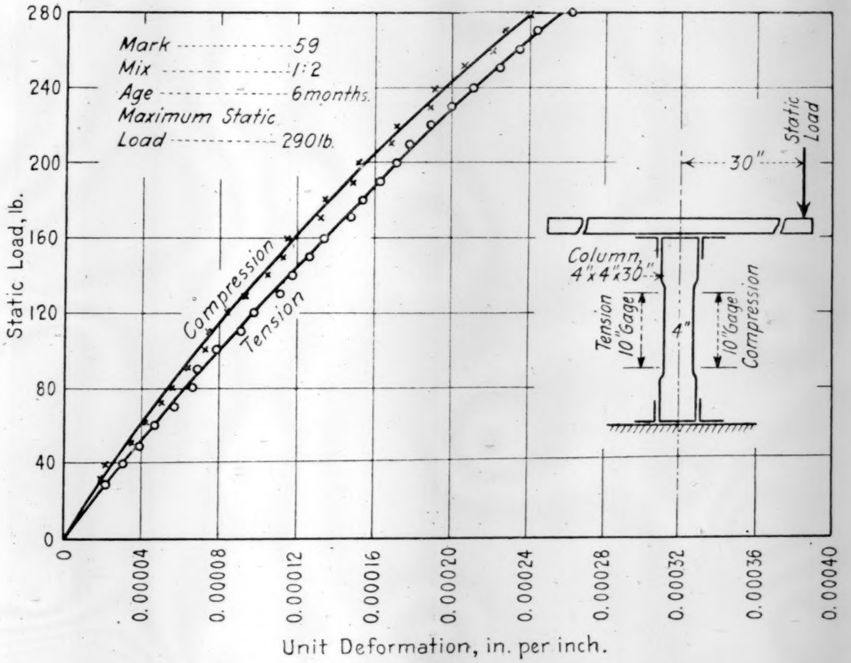

FIG. 2.—Static Loading Curve

Stanton Walker[1] has indicated that a relation exists between the modulus of elasticity and the compressive strength of concrete. The method of load application in the fatigue tests has prompted an

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[1] Stanton Walker, "Modulus of Elasticity of Concrete," *Proceedings*, Am. Soc. Testing Mats., Vol. XIX, Part II, p. 510 (1919).

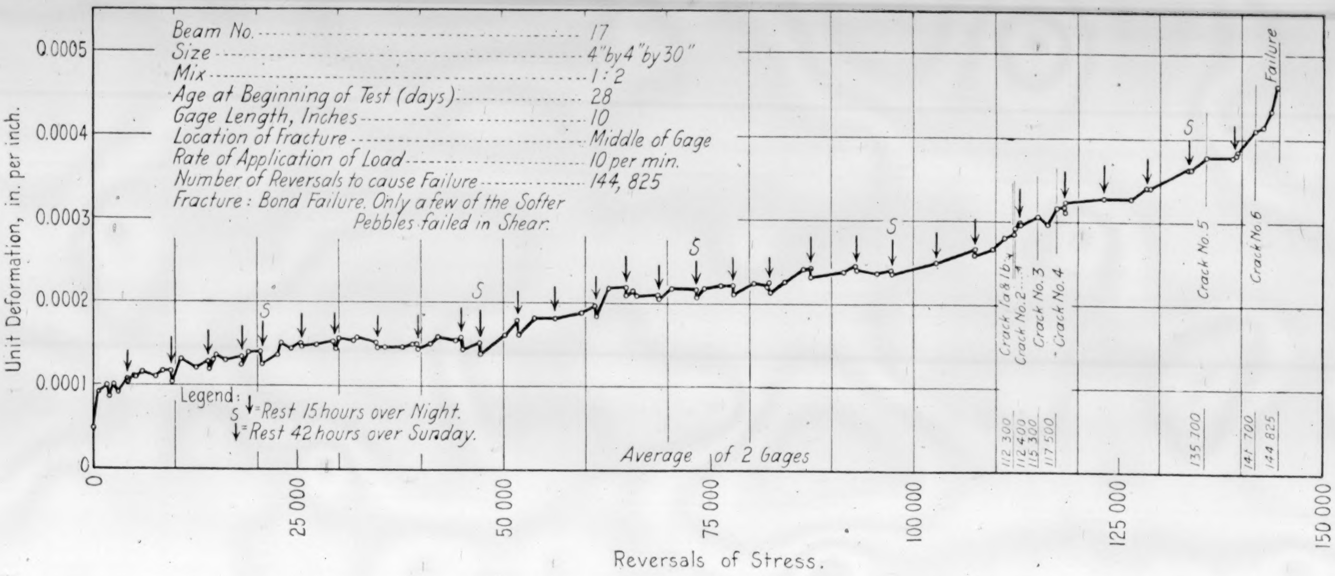

FIG. 3.—Typical Fatigue Curves. Unit Deformation is the Unit Deformation Tension to Compression.

exhaustive study of the modulus of elasticity in tension of the mortar beams. An analysis of the data on hand indicates, however, that the modulus of elasticity bears no consistent relation to the transverse strength which might be used to advantage in more accurately determining the strength of the individual specimens.

DISCUSSION OF TESTS

The curve shown in Fig. 3 illustrates the typical effect of reversed stresses which caused failure of the mortar specimen. It is to be noted that there is a progressive deformation in the extreme fibers until failure of the test specimen takes place. A straight line of constant slope relation is indicated up to 110,000 reversals of stress. At this point, four cracks appeared on the outermost fibers of the specimen, and the slope of the line changed. An additional 35,000 reversals of stress were necessary to produce complete failure. The critical point in the progressive deformation cycle is designated as the premature failure limit.

The curve is representative of the tests completed to date, although in many cases the premature failure limit is not so pronounced and the additional number of reversals necessary for complete failure is less. A parallel cycle of action is indicated, nevertheless, by the general trend of the deformation curves.

It is apparent that rupture or failure of the bond occurs first on the extreme outer fibers where the deformation is a maximum. This action is progressive toward the center of the beam until complete failure is imminent. Continual progressive deformation of the outer fibers is significant of the fatigue element. Progressive deformation may occur in some cases for a certain number of reversals when it becomes evident from the flatness of the deformation curve that the material is showing no ill effects from repetitions of the load. The assumption is made that loads producing constant deformation are below the endurance limit or the safe working stress of the material.

Recovery Phenomenon During a Period of Rest.—An interesting feature of the fatigue tests, shown in Fig. 3, is that of the appreciable recovery during a period of rest of the strength of mortar undergoing a fatiguing action. It has been necessary each night to stop the machine and, as a result, the strength recovery phenomenon is clearly illustrated on the curve. A parallel action is common to many materials, such as, metals, wood, and soils when they are stressed beyond a safe limit. The amount of recovery in the case of cement mortar seems directly proportional to the duration of the period of rest. It is to be noted that no recovery occurs after the premature failure limit has been attained.

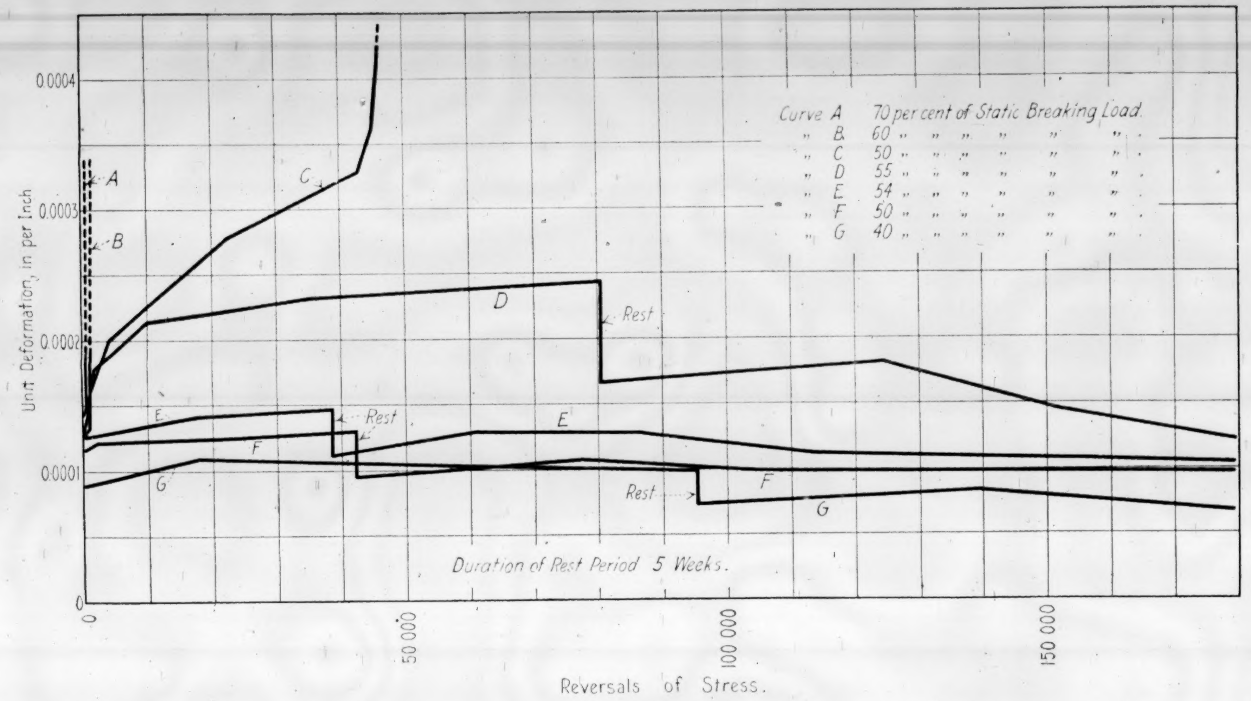

FIG. 4.—Showing Effect of Prolonged Rest Period. Unit Deformation is the Unit Deformation Tension to Compression.

Fig. 4 illustrates some peculiar as well as interesting features of a series of beams under test at the present time. During this series of tests it became necessary to stop the machine for five weeks, and as a result the effects of a prolonged rest period is clearly evident from the trend of the deformation curves. The beneficial or strengthening effect of the rest period is the most pronounced in the case of the specimen represented by curve D. In this instance it is believed that the induced stress approached the critical limit and that failure was imminent. The degree of deformation following the rest period indicates that the beam had fully recovered from the initial overstressings. As these beams were five months old, it is thought that the increase in strength due to age over the rest period exerted a minor influence upon the subsequent resistance of the mortar.

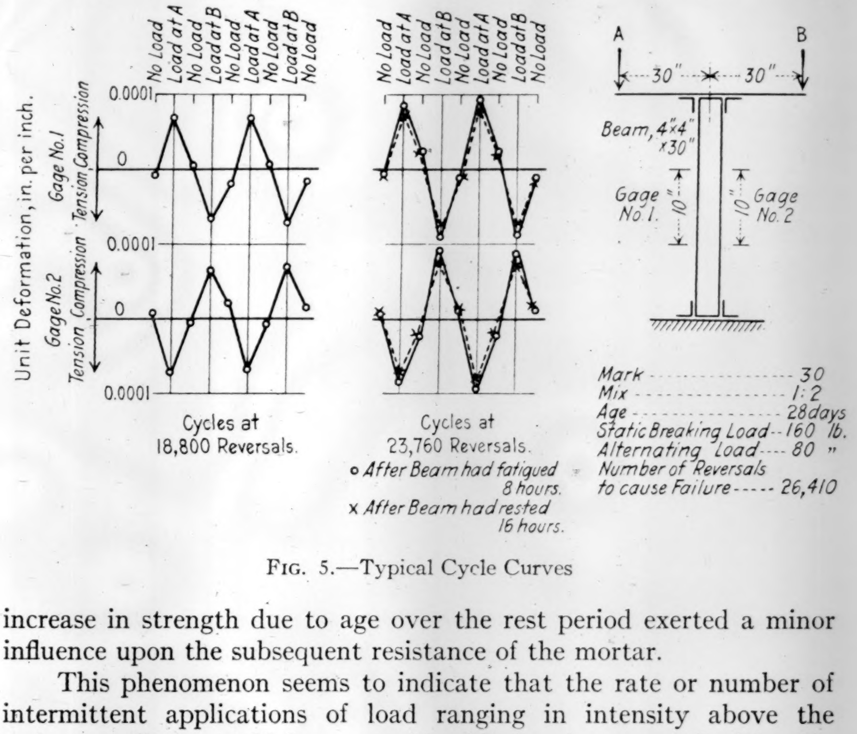

FIG. 5.—Typical Cycle Curves

This phenomenon seems to indicate that the rate or number of intermittent applications of load ranging in intensity above the endurance limit would have considerable bearing upon the life of a concrete structure.

It is to be noted that the effect of the rest period is less pronounced in the case of the specimens represented by curves E, F, and G. In these beams, however, the induced stresses were apparently below the endurance limit and no fatiguing action had taken place. The general trend of the deformation curves after 140,000 reversals seems to be in a downward direction. As previously stated, it is believed that the influence of the increase in strength due to age is negligible, although the prevailing humidity and summer temperatures may have accentuated the apparent increasing resistance of the mortar.

Fig. 5 shows the sequence of deformation and the plastic set or failure of the mortar to return to its original position in both tension and compression. A marked contrast can readily be detected in the magnitude of the plastic set occurring at 18,800 and 23,760 reversals of stress, respectively. The broken line appearing in the cycle curve at 23,760 reversals shows the unit deformation after the beam had rested for a period of 16 hours. These maximum deformations are less than those of the same beam just previous to the rest period, which shows a partial recovery from the fatigue action. This action is typical of all tests in that progressive plastic set simultaneously accompanies fatigue.

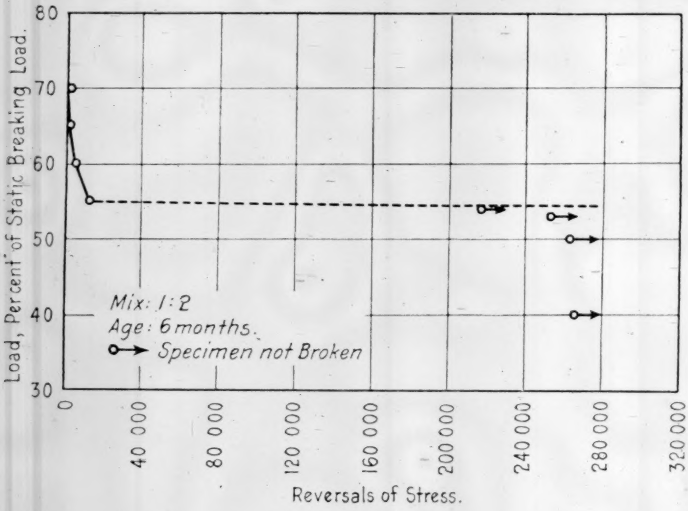

FIG. 6.—Fatigue Endurance Limit of Cement Mortar Beams

INDICATIONS AND SUMMARY OF TESTS

The purpose of this paper has not been to present finished data or to draw definite conclusions concerning the fatigue of mortar and concrete, but more to stimulate interest and discussion of the subject. There are, however, several indications of prominence which are emphasized by this investigation:

1. (*a*) 28 Day Tests, 12 Beams, 1:2 Mix.—Results of this series of tests indicate that no definite endurance limit between 40 and 60

per cent of that load required to break the beam under a single application can be assigned to cement mortar of this age.

(*b*) 4 Month Tests, 8 Beams, 1:2 Mix.—Results of this series of tests shown in Fig. 4 indicate that the endurance limit is approximately 50 to 55 per cent of the static load.

(*c*) 6 Month Tests, 6 Beams, 1:2 Mix.—Results of these tests shown in Fig. 6 indicate that the endurance limit is 54 to 55 per cent of the static breaking load.

2. The number of reversals of stress necessary to cause failure decreases in a proportion to the respective increase of the percentage of stress above the apparent endurance limit.

3. Stresses above the endurance limit cause continual progressive deformation.

4. Stresses below the endurance limit may cause progressive deformation within certain limits.

(In either 3 or 4, progressive deformation is not significant of permanent injury to the product unless actual rupture of the bond occurs on the extreme outermost fibers.)

5. The effect of a rest period indicates that the rate and number of intermittent applications of load ranging in intensity above the endurance limit would have considerable bearing upon the life of a concrete structure.

6. (*a*) The amount of recovery in strength in the case of cement mortar is directly proportional to the duration of the period of rest.

(*b*) Above the premature failure limit no appreciable recovery occurs.

FUTURE INVESTIGATIONS

The effect of rest periods upon the endurance or life of cement products will be carried on at a future date. In several of these series of tests, it is planned to subject beams to a limited number of reversals of critical stress every 24 hours, the stresses being maintained above the endurance limit. In other investigations it is planned to determine the endurance limit when the beams are not given these rest periods. Specimens of concrete as well as mortar will be studied in fatigue.

Authoritative results of the fatigue phenomena in mortar involve a time element of considerable magnitude and although these tests have been conducted during the past two years, this field of investigation is only opened. Certain features of the fatigue phenomena have already been indicated, and it is thought, from this foundation, that valuable information will be secured from future tests to aid in the proper design of concrete.

DISCUSSION

Mr. Johnson

MR. A. N. JOHNSON.[1]—Mr. Crepps is to be congratulated on the painstaking care and thoroughness with which he has conducted this investigation, which is one of a series of three investigations on the fatigue of concrete. Of the other two, one is carried on by the Illinois State Highway Division and the other at the University of Maryland. In the arrangement of the apparatus as described by Mr. Crepps, it is to be noted that the test specimen is submitted to a uniform bending moment throughout its length. In the arrangement of the apparatus used in Illinois, the specimen is a cantilever beam, the maximum moment being at one end; while with the apparatus set up at the University of Maryland the tests are made upon a free beam with the load applied at two points toward the center, over which portion only of the beam the bending moment is uniform.

The most striking feature brought out by Mr. Crepps is the recovery of the beam during a period of rest. It will be noted, however, that in the curve shown, this does not seem to operate to preclude the progressive increase in deformations as the beam proceeds to complete fatigue.

The curve on which the phenomena are noted was produced by a loading which caused complete fatigue. It would seem that we should also study the effect on the beam of a load producing fatigue but which was not progressive in its action, that is, a load which the beam could withstand for an indefinite number of repetitions. The curves that would be produced from data so obtained should be compared with those now presented by Mr. Crepps where the load eventually produced a failure of the beam.

In the work that we have done at the University of Maryland, not enough data have been collected to draw any definite conclusions, although we do seem to note that as a beam has been partially fatigued, it has undergone little if any change in its ability to resist loads. It may be remarked that the beams we have so far tested at the University of Maryland are all over a year old, while the specimens as reported by Mr. Crepps are a few months old.

Mr. Clemmer

MR. H. F. CLEMMER.[2]—The work on fatigue of concrete conducted by the Illinois State Highway Division checks to a great

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[1] Dean, College of Engineering, University of Maryland, College Park, Md.

[2] Engineer of Materials, Illinois Highway Division, Springfield, Ill.

Mr. Clemmer

extent the results obtained at Purdue University by Mr. Crepps. The apparatus used, which produces a direct stress on cantilevered beams rather than a reversal of stresses, was described in a paper presented here last year.[1] Under a repeated load which is applied 40 times per minute beams consisting of 1:2:3½ concrete are stressed to various percentages of their moduli of rupture.

During the past year 45 beams were broken through fatigue. Well-aged beams were used, 11 being over eight months old and 34 being over a year old. The increase in strength with age was not apparent to such a degree as to interfere with the effects of fatigue. Loads were applied which produced stresses from 51 to 82 per cent of the modulus of rupture. The number of applications of load applied before failure took place varied from 246,000 for the lower percentage of stress to 120 for the higher percentage. It was well indicated that stresses from 51 per cent up will produce fatigue and failure.

Considerable time was spent in checking up the effect of loads which cause stresses below 51 per cent of the modulus of rupture. By measuring deflections on a beam under the loads applied it was found that as the number of applications of load were increased, the deflections decreased. If the ability of a beam to resist deflection is a measure of its strength the beams were increasing in strength as the number of applications of the load increased. Actual readings on one beam showed the following:

At 68,000	applications,	0.0230 in.	deflection
688,000	"	0.0210 "	"
2,220,000	"	0.0168 "	"

The set of the concrete after the load is released for a period of time which Mr. Crepps mentioned in his paper is also indicated in the Illinois tests, though not to such a great extent. When the fatigue machine is stopped for any length of time a slight set of the beams is noticed. This set apparently retards the increase of the deflection of the beams if the same load as was originally applied is repeated.

Besides the work in fatigue with repeated loads, Illinois has carried on an investigation in static load fatigue. Cantilever beams of concrete were subjected to static loads which produced stresses of various percentages of their moduli of rupture. The critical percentage of stress which caused fatigue was found to be 71 per cent. A number of beams failed at the end of approximately 105 hours under a load causing a stress of 71 per cent of the breaking stress. With a stress of 84 per cent the beams failed at the end of 14 hours.

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[1] H. F. Clemmer, "Fatigue of Concrete," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 408 (1922).

Mr. Clemmer

The study in set of concrete which had been stressed with a static load revealed the indication that after the load had been removed a set was secured; but this set was more or less temporary, the beam recovering most of its deformation in time. A beam was loaded with 50-lb. increments until the total of 250 lb. was applied. This load was then removed and a set of 0.046 in. was recorded. At the end of a period of 24 hours the dial registered a set of 0.015 in., which indicated that the beam had recovered approximately two-thirds of the original set.

The surface cracking with the gradual increasing depth of crack mentioned by Mr. Crepps has not been noticeable in the Illinois tests. Although an increase in deflection with a decrease in recovery has been noticed when the failure is about to take place, no cracking of the surface of the specimens has been observed.

Mr. Goldbeck

MR. A. T. GOLDBECK.[1]—I might point out the fact that these tests just described are really a part of a large series of investigations now being conducted looking toward the more rational design of concrete roads. The concrete road surface is never at rest. It is acted upon by a lot of different forces. We know very well, for instance, that warping occurs during the day; the sun heats the top surface, expands it and bends the corners down. At night the reverse is true. When loads go by we get high stress under the wheel,—compression on the top surface, tension on the bottom,—and perhaps 2 or 3 ft. away from the wheel there is a point of contraflexure. Concrete roads are continuously subjected to tension or compression varying from one to the other, so that these tests are particularly significant in connection with the correct design of concrete roads and they should lead to the establishment of a safe tensile stress to use in rational design.

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[1] Chief, Division of Tests, U. S. Bureau of Public Roads, Washington, D. C.

SOME RELATIONS BETWEEN THE CHARACTERISTICS OF STRAIGHT DISTILLED TAR RESIDUES

By J. W. Kennedy[1]

Introduction

The necessity for ascertaining the relationship which the physical properties of tars bear to one another is recognized by those who are connected with the tar industry and especially by those who deal with its application in the field of road construction and maintenance. Such relationships are useful to a proper interpretation of test results as well as to the preparation of specifications; they also furnish control data for plant operation in the manufacture of tars.

This paper treats of the inter-relationship between the physical characteristics of tars of different origin as brought out by tests conducted at the Michigan State Highway Laboratory. It is generally conceded that certain tests, such as the ring-and-ball softening point test and the penetration test are applicable chiefly to asphalts. These tests as applied to tars are here investigated, as are also other tests characteristic of tars such as the cube-in-water softening point test and the float test. The data obtained show the general effect of variations in the percentage of free carbon on some of the other properties of tars.

Procedure

Three distinct types of raw tars were obtained: namely, water gas (low carbon), coke oven (medium carbon), and gas house (high carbon). A gradient in variation for one property was established for each type of tar and the resulting effect on the other properties of the tar was determined. Accordingly, the cube-in-water softening point was varied from 95° F. to 125° F. in steps of 5° F. This range covers practically all tars that are used in road work excepting those for surface treating and dust laying, which were too thin to permit of the tests contemplated.

The raw tar as obtained from the manufacturer was placed in an ordinary two-quart tar still in which it was dehydrated and subsequently distilled until the residue reached a cube-in-water softening point (A.S.T.M. Standard Method of Test for Softening Point of Tar Products (Cube-in-Water Method), D 61 – 20)[1] of 95° F. A sample was then drawn and on it were conducted the following tests:

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[1] Chemical Engineer, Michigan State Highway Department.

1. Softening point, ring-and-ball method. (A.S.T.M. Standard Method of Test for Softening Point of Bituminous Materials Other than Tar Products, D 36 – 21.)[1]
2. Float test at 50° C. (A.S.T.M. Standard Methods of Sampling and Analysis of Creosote Oil, D 38 – 18, Sections 17 and 18.)[1]
3. Penetration (A.S.T.M. Standard Method of Test for Penetration of Bituminous Materials, D 5 – 21, Roberts No. 2 Needle.)[1]
4. Free carbon. (Method described in U. S. Department of Agriculture *Bulletin No. 949*, p. 38.)

The above tests were repeated for each 5° F. variation until 125° F. was reached. It should be noted that the float test apparatus did not conform exactly to the dimensions given under the new A.S.T.M. Tentative Method of Float Test for Bituminous Road Materials (D 139 – 22 T),[2] and that the penetration test was conducted at 4° C. under a weight of 200 g. for 5 seconds in order to obtain conditions applicable over the entire range of consistencies investigated.

DISCUSSION

The data obtained have been plotted as shown in Figs. 1 to 8.

Ring-and-Ball Compared with Cube-in-Water Softening Point.—Referring to the curves plotted on Fig. 1, it is to be noted that, as the softening point by one method increases, the same is true of the other method but not in a direct ratio. The gas-house tar which has a high free-carbon content exhibits the smallest difference between maximum and minimum ring-and-ball softening points for the given range of cube-in-water softening points. This might be explained by the high percentage of free carbon, which tends to make the material less reactive to this test. It cannot be said, however, that the higher the percentage of free carbon, the less will be the difference between extremes of ring-and-ball softening point values with a given range of cube-in-water softening points, since coke-oven tar exhibits in this investigation a wider range than does water-gas tar of low-carbon content.

Softening Point Compared with Float Test.—In Fig. 2 are plotted curves showing the relation between cube-in-water softening points and the float test results, while in Fig. 3 is found a comparison of the ring-and-ball softening point with the float test. As the cube-in-water test and ring-and-ball test are only different methods of measuring the same property, one would expect to find a similarity in the two sets of curves, and such a similarity is easily noted. Water-gas tar shows the greatest range in float-test values for the range of softening points covered. At the upper limits, the float test for this type of tar increases at a much greater rate than does that of either coke-oven or gas-house tar when it is plotted against the cube-in-water softening point, as is done in Fig. 2; at the lower range, however, the rate of increase of the float test is about the same for all tars and, at a given cube-in-water softening point, gas-house tar shows the highest float test. In all cases, gas-house tar showed a higher float test than did coke-oven tar of the same softening point, but water-gas tar, at the upper range, showed the highest float test, while at the lower range it gave values intermediate to those of the other types. These curves tend to show why different observers have not agreed very closely on the relation between float and softening point tests. The relation varies as the

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[1] 1921 Book of A.S.T.M. Standards.

[2] *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part 1, p. 809 (1922).

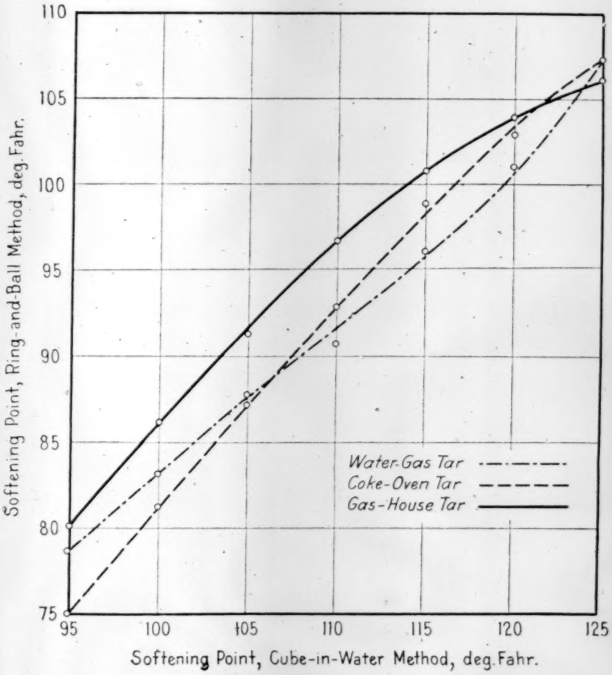

FIG. 1.—Comparison of Ring-and-Ball and Cube-in-Water Softening Points for Different Types of Tars

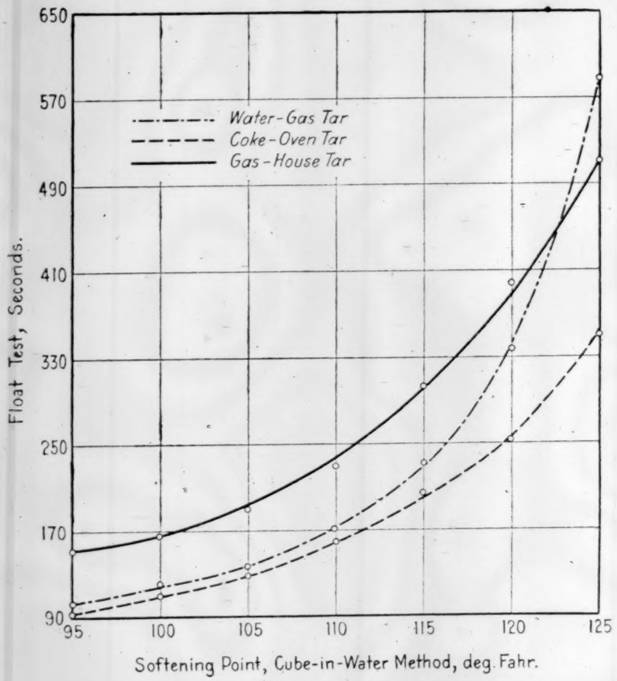

FIG. 2.—Comparison of Cube-in-Water Softening Point and Float Test for Different Types of Tars

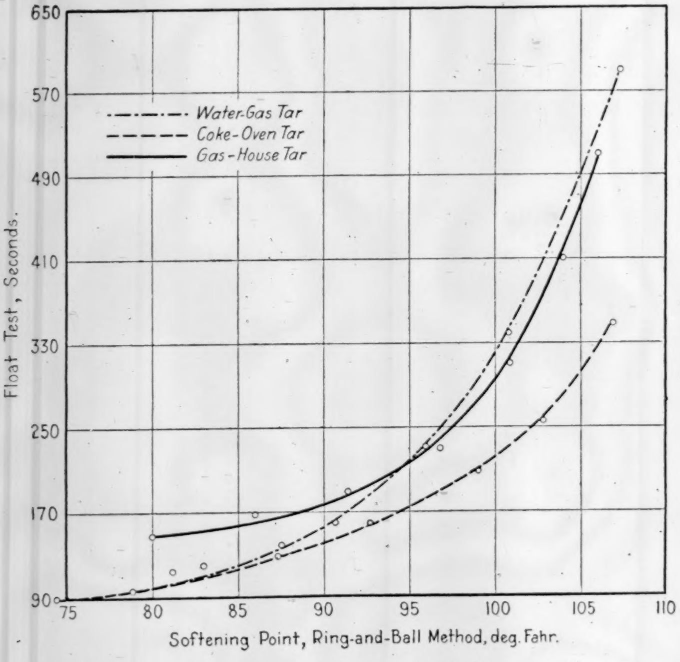

FIG. 3.—Comparison of the Ring-and-ball Softening Point and the Float Test for Different Types of Tars

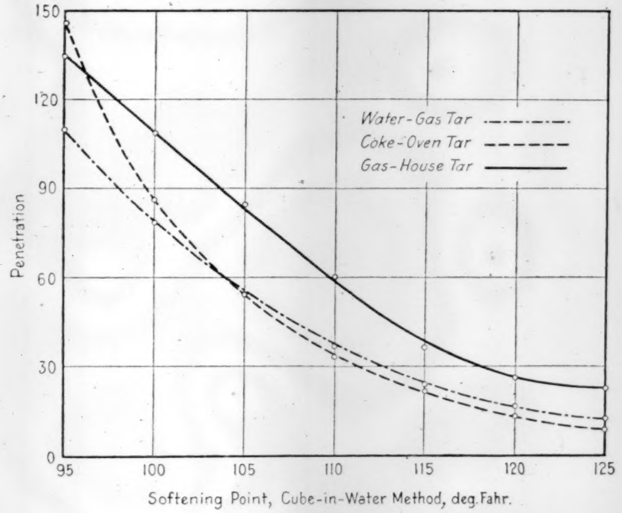

Fig. 4.—Comparison of Cube-in-Water Softening Point and Penetration for Different Types of Tars

Penetration at 200 g., 5 sec., and 4° C.

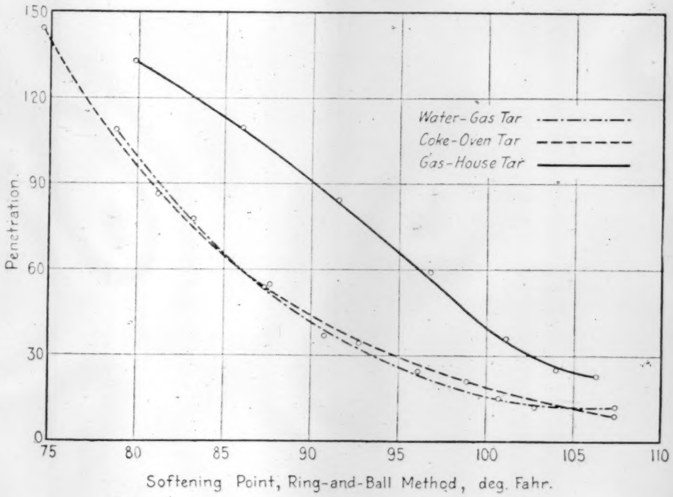

Fig. 5.—Comparison of the Ring-and-Ball Softening Point and Penetration for Different Types of Tars

Penetration at 200 g., 5 sec., and 4° C.

softening point is increased and tars of high free-carbon content show quite a different relation than do those of low free-carbon content. The seemingly strange behavior of water-gas tar can only be explained (in the light of present observations) by the very low percentage of free carbon which tends to make it more sensitive to the temperature changes to which it is subjected in these tests.

Softening Point Compared with Penetration Test.—The penetration and cube-in-water softening point, two of the most common determinations for bituminous materials, although the former is seldom determined for tars, are plotted in Fig. 4. The penetration tests were made with the old parabolic needle and not the new tapered one adopted by the American Society for Testing Materials. The conditions under which these tests were conducted were somewhat unusual and for this reason the results should be considered only from a comparative standpoint. From the curves, it is seen that coke-oven tar shows the greatest range of variation for the given range of softening point figures. With the data available from the present investigation no explanation of this phenomenon is offered.

Fig. 5 shows the relationship between the ring-and-ball softening point and the penetration test for tars. The curves for water-gas and coke-oven tar are practically identical. For a given ring-and-ball softening point, the penetration of gas-house tar is considerably higher than that of either of the other two types. The maximum difference of penetration seems to occur at a ring-and-ball softening point of from 85 to 90° F.

Float Test Compared with Penetration Test.—A qualitative relationship between the penetration test and the float test is shown in Fig. 6. At the lower range of float test values for all types of tars investigated, the penetration shows a rapid decrease for only slight increases of float-test values. At the upper range, the penetration values remain rather constant while the float test increases rapidly. This indicates that a certain stability is reached upon distillation at which the tar shows little or no difference in penetration. This phenomenon is probably due to the nature of the penetration test which is a measure of the resistance of the material to displacement by the needle. Apparently a point is reached where the bituminous material becomes more of a solid than a liquid, so that its resistance to the needle increases very rapidly. Such a point might well represent a physical property of the material which corresponds to the melting point of crystallin substances. For a given float test, gas-house tar of high free-carbon content, contrary to expectations, shows a much higher penetration than does either of the other types.

This might be explained as follows: Increasing the free carbon to the amount present in gas-house tars gives a softening point that is higher than the real softening point of the bitumen. The needle, which might be considered as measuring the ease of penetration of the bitumen itself, pushes out of the way and, in a sense, slides by the free carbon particles and registers a penetration higher than would be expected from the softening point for the given sample. This explanation is offered for what it may be worth as an aid in determining why erratic results are obtained in the penetration test on tars of high free-carbon content.

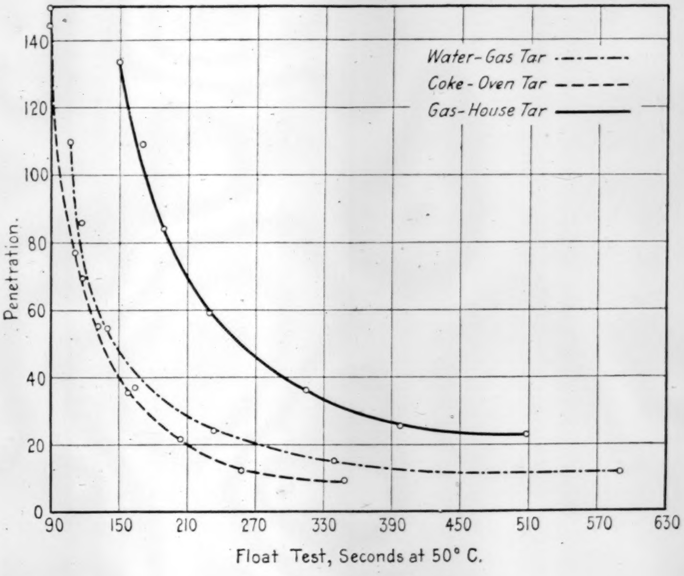

Fig. 6.—Comparison of the Float and Penetration Tests for Different Types of Tars
Penetration at 200 g., 5 sec., and 4° C.

Effects of Free-Carbon Content in Tars.—If curves were drawn for each type of tar showing the relation between free-carbon content and the other properties in a manner similar to that of the foregoing curves, little could be learned. They would show the increase of free-carbon content with concentration of the residue and, also, the cracking of the material which causes an increase in the free carbon beyond the percentage called for by concentration alone. The effect of free-carbon content in tars, in so far as brought out in this investigation, can better be considered in conjunction with Figs. 7 and 8, which show the relation between: first, the float test and free-carbon content for tars having the same cube-in-water softening point, and second, the penetration test and free-carbon content for tars having the same float test.

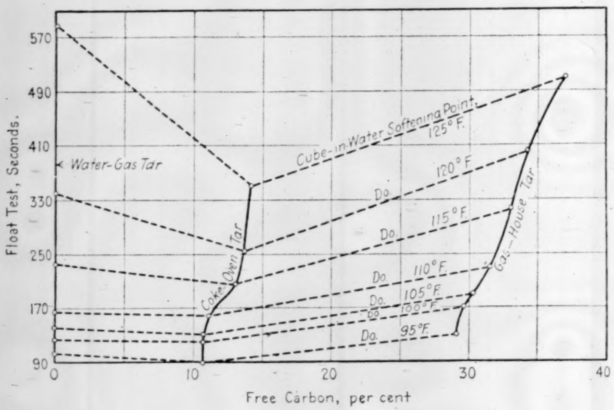

FIG. 7.—Relation Between Float Test and Free-Carbon Content for Tars Having the Same Cube-in-Water Softening Point

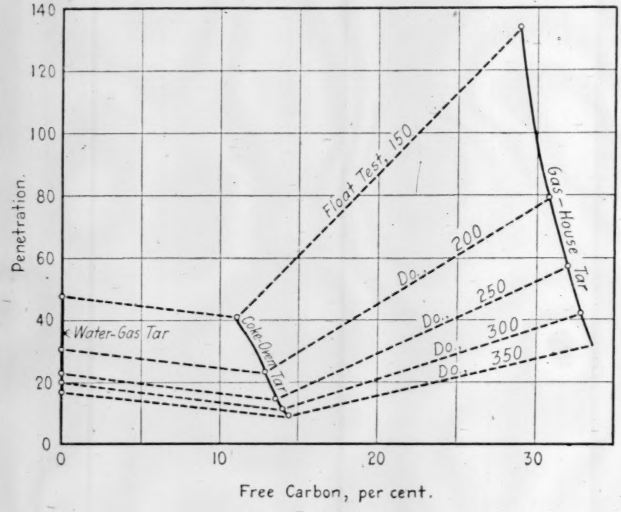

FIG. 8.—Relation Between Penetration Test and Free-Carbon Content for Tars Having the Same Float Test

Fig. 7 shows the relation between the float test and free-carbon content for tars of seven different consistencies as measured by their cube-in-water softening points. As only three tars of different carbon content were available, only three points on each curve were located.

It will be noted that all of the curves are similar and that an interesting break in the relationship is indicated: thus, coke-oven tar containing a medium percentage of free carbon shows in all cases an abnormally low float test. Whether this phenomenon is a function of the free-carbon content or of some other characteristic of the tar due to its origin, this investigation does not determine; nor does it locate the exact point of the break, but merely establishes the existence of the phenomenon. From these curves it would appear that, at a given float test and cube-in-water softening point, there exist two tars of quite different free-carbon content.

Merely to emphasize further the facts brought out above, the curves of Fig. 8 were plotted, showing the relation between penetration and free-carbon content for tars having a given float test. The same general tendency is shown in all. At a given float and penetration test, are found two tars of different free-carbon content. The determination of the exact meaning of this phenomenon will require further investigation. A thorough study of the characteristics brought out by the curves of Figs. 7 and 8 should furnish the real solution of the effect of free-carbon content on the physical properties of tars.

CONCLUSIONS

A few of the more important conclusions to be drawn from this investigation are the following:

1. Different types of tars show the same general changes in their physical properties upon refinement.

2. Upon distillation, a certain point (similar to melting point in crystallin substances) is reached, after which, tars show little or no further changes in penetration.

3. Water-gas tar and coke-oven tar show a closer relationship in physical properties than does gas-house tar with either of the above two.

4. The relation between the cube-in-water softening point and the float test varies as the softening point is raised and tars of higher free-carbon content show quite a different relation than do those of lower free-carbon content.

5. Concerning the relations between the physical constants:
    (*a*) The cube-in-water softening point is raised with the ring-and-ball softening point.
    (*b*) The float test increases with the softening point.
    (*c*) The penetration decreases as the softening point and float test values are raised.

In no cases are the relations between any two given properties sufficiently uniform to be considered as straight line functions.

6. For a given float test, gas-house tar shows a much greater penetration than does either water-gas or coke-oven tar. The high percentage of free carbon present in gas-house tar seems to act as a lubricant on the needle and causes it to register a penetration greater than that for the bitumen alone.

7. In general, the presence of free carbon in the amount found in the gas-house tar imparted a stability which made this type less reactive to those tests which involved the continued application of heat.

8. Variations in the free-carbon content seem to affect the relations between all of the properties. As the free-carbon content is increased, a well defined break occurs in the relationship between the other physical properties.

All of the above conclusions are understood to apply only to the range and materials as covered in this investigation.

USE OF AIR BATH IN DETERMINATION OF SOLID RESIDUE OF ROAD OILS AT 100 PENETRATION

By H. F. Clemmer[1] and H. C. Helmle[2]

During the past few years the increased use of road oils in Illinois has made it necessary that these materials be purchased from a great number of refineries whose processes and products vary to a considerable degree. In making the tests on these oils, difficulty has been experienced in securing uniform results on the test known as "Solid residue of 100 penetration."

Table I.—Percentages of Solid Residue (100 Penetration) at Different Temperature Ranges

Each value is average of four determinations

Laboratory Number	Temperature Ranges					
	230 to 240° C.		250 to 260° C.		270 to 280° C.	
	Solid Residue, per cent	Time of Evaporation, hours	Solid Residue, per cent	Time of Evaporation, hours	Solid Residue, per cent	Time of Evaporation, hours
22-1899	66	9	64	6	61	4
22-2265	63	4	62	2.5	61	2
22-1968	63	4.5	61	3	59	2.5
22-1969	70	4.5	68	3.25	66	3
22-2030	73	33	68	22	64	13
22-2059	70	37	66	27	62	20
22-2096	72	30	65	23	60	16

There have been in general use two methods of evaporating a road oil to a residue of desired penetration, namely, the sand bath and the hot plate. Since 1915, however, the testing laboratory of the Illinois Division of Highways has made use of the ordinary gas hot plate, finding that it affords greater ease of temperature control as well as greater accuracy of test than was possible with the sand bath.

In recent years, many road oils obtained by pressure distillations and cracking processes have been sold in the state. Due to the nature of these materials, considerable trouble has been experienced in obtaining, with existing methods, uniform results in solid residue determinations. The appearance of a coky residue or of coke deposits on the sides of the residue box has led to the belief that the inconsistencies in results are caused by local heating, during the evaporation, of the oil on the sides or bottom of the residue box. Cracked or burned road oils, being originally of a coky nature, contribute more readily to these conditions of irregular heating and it is with these materials that the greatest discrepancies in results occur.

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[1] Engineer of Materials, Illinois Division of Highways, Springfield, Ill.

[2] Assistant Engineer of Materials, Illinois Division of Highways, Springfield, Ill.

In order to provide a method which would eliminate these conditions of local heating and give uniform results on both cracked and skimmed products, the air-bath apparatus was designed and has now replaced both previous methods in the inspection of oils by the Illinois Highway Laboratory. The apparatus shown in Fig. 1 is the same as that used in the previous gas hot plate method with the addition of a metal ring 3 in. in diameter and 1¼ in. in height. The residue box fits snugly through the top of the ring and is supported at a distance of ¼ in. from the surface of the hot plate by a wire shelf constructed inside the ring. The inside diameter of this ring is ½ in. greater than that of the residue box, so that on the side and bottom of the box there is an air space of ¼ in. The air bath may be used in conjunction with either an electric or gas hot plate and in either case provides for an even temperature at all points where the oil would come in contact with the sides and bottom of the residue box during its evaporation to a solid residue. In addition to the fact that the air bath eliminated local heating and coke deposits, it was noted during the tests that very little attention of the operator is required to maintain a constant temperature throughout the period required to evaporate the oil.

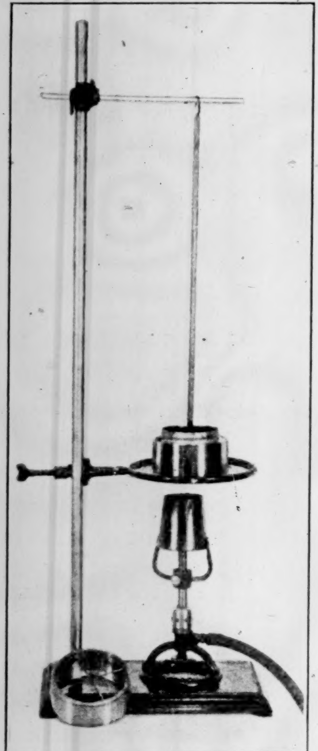

FIG. 1.—Illustration of Air Bath Used in Determination of Solid Residue

In order to determine the effect of temperature control upon the solid residue test, seven grades of oil were evaporated between three separate temperature ranges: 230 to 240° C., 250 to 260° C., and 270 to 280° C. Four samples of each grade of oil were heated at each temperature range, making in all 84 determinations. Table I gives the average of four determinations for each grade of oil at each temperature range and shows the variation in the percentages of solid residue due to the difference in temperature at which the tests were run. The importance of a uniform temperature is demonstrated in the results obtained, being especially illustrated in the case of sample 22–2096 which varied 12 per cent with 40° C. difference in temperature. The number of hours required to make each run was also recorded and shows a proportionate increase in variations of results with the increase in length of time required to evaporate the oils.

TABLE II.—PERCENTAGES OF SOLID RESIDUE (100 PENETRATION) TEMPERATURE 250 TO 260° C.

Laboratory No.	Type of Oil	Solid Residue, per cent		
		Sand Bath	Hot Plate	Air Bath
22–1899	Mid Continent and Mexican Blend "Skimmed"	62 62 62 62	64 64 63 64	64 64 64 64
22–2265	Mexican	60 60 60 61	62 61 62 62	61 61 61 61
22–1968	Pressure Tar Residue	59 59	61 61 61 61	63 62 62 62
22–1969	Pressure Tar Residue	65 64	69 68 68 68	68 68 68 68
22–2030	Mid Continent "Skimmed"	65 66 65 67	67 67 68 68	68 68 67 68
22–2059	Mid Continent "Skimmed"	63 63 64 62	66 66 65 65	65 65 65 65
22–2096	Mid Continent "Skimmed"	61 62 60 60	65 65 65 65	66 66 66 65
22–1921	Mid Continent "Cracked"	59 59 60 ..	49 49 47 ..	62 62 62 61
22–1922	Mid Continent "Cracked"	68 64 67 65	57 58 54 55	73 73 74 74

Table II shows for each grade of road oil the percentages of solid residue obtained by the use of each method of determination. Four samples of each grade were tested by each method, requiring in all 108 separate determinations. These results show clearly the value of the air bath method in obtaining uniform results regardless of the type of oil tested.

From this investigation it has been shown that temperature control, being a vital factor, should be considered in all methods for the determination of solid residues. In the sand bath method it is difficult to control the temperature of the oil and although even temperature is more easily maintained on the hot plate, direct contact of the residue box with the hot plate lends to the formation of a coky residue and causes wide variation in the percentages obtained. The air bath, quickly adjusted to the proper temperature and easily controlled, has been found to be adaptable to the determination of solid residue on all types of road oil.

DISCUSSION

Mr. Milburn

MR. H. M. MILBURN[1] (*presented in written form*).—A requirement for the percentage of residue of a specified penetration is one that is quite commonly included in specifications for road oils and for fluxes for use in the preparation of asphaltic cements and for use in softening the bitumen of some rock asphalts. This requirement undoubtedly would be more generally specified were it not for the fact that some consider that the test as usually made may not give closely concordant results. Therefore, it would seem that the work of the authors of this paper should be of great value, as it appears that by the use of the air bath much better results can be obtained than heretofore. Especially does this seem true in view of the fact that they state a constant temperature can be maintained very easily.

In connection with this point of temperature control, I note that the time of evaporation in Table I is as high as 37 hours. I believe it would be of interest to know if the evaporations were conducted continuously or intermittently and if any attempts were made to ascertain if similar results would be obtained in continuous and intermittent evaporations. That is to say, would an oil show the same percentage of residue of the same penetration when evaporated for five periods of 6 hours each as when evaporated for 30 hours continuously. I believe it would also be of interest to know the exact penetrations which were obtained on the residues.

Mr. Clemmer

MR. H. F. CLEMMER.—We found that the percentage of residue determined by evaporating for four 9-hour periods checked within 1 per cent as compared with continuous evaporation for 37 hours, and that the same penetration was reached in both. All those samples requiring heating periods of less than 18 hours were run continuously, but we did run that check using four 9-hour periods in comparison to one 37-hour period and found that the two residues checked within 1 per cent and were practically of the same penetration.

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[1] Chemist, U. S. Bureau of Public Roads, Washington, D. C.

FURTHER STUDIES OF THE PHYSICAL PROPERTIES OF DRYING-OIL, PAINT AND VARNISH FILMS[1]

BY HARLEY A. NELSON[2] AND GEORGE W. RUNDLE[3]

This paper discusses the fundamental properties of protective paint coatings, the information having been derived from more detailed studies of stripped films of drying oils, paints and varnishes.

The tests here described were conducted on film specimens obtained by applying the coatings to amalgamated surfaces.[4] Of the pigments used in ordinary painting practice, zinc oxide and basic carbonate white lead are chiefly considered because they differ most typically in their known reactions under normal weather exposure conditions.

MODIFIED STRESS-STRAIN TESTING METHODS

In a previously reported investigation,[4] an ordinary dynamic stress-strain method for testing was described. This method is simple, easily applied and has yielded much valuable information. However, a brief consideration of the behavior of typical films under stresses applied by means other than the direct dynamic method has been found worth while.

Application of Fixed Loads (Elongation with Time).—Under fixed loads the first distortion is at a constantly decreasing rate as shown in Fig. 1 (*a*). Later a contrast in that respect may appear, such as exists between No. 3 (basic carbonate white lead, 70 per cent by weight in 30 per cent vehicle, consisting of 90 per cent raw linseed oil, 5 per cent turpentine and 5 per cent dryer), and No. 4 (lead-free zinc oxide, 50 per cent by weight in 50 per cent vehicle, consisting of 90 per cent raw linseed oil, 5 per cent turpentine and 5 per cent dryer). Each of these was stressed by a load representing one-fourth of its tensile strength as measured by the dynamic method. No. 3 apparently reached a state of equilibrium and no break was recorded.

Curves Nos. 1 and 2 are for the same film as curve No. 3 taken at loads of three-fourths and one-half of the tensile strength, respecttively. Curve No. 5 is of unpigmented linseed oil at a load of one-fourth of its tensile strength (880 g. per sq. cm.). Judging by curves Nos. 2 and 3, a critical load for this film exists slightly above one-fourth its ultimate load (7800 g. per sq. cm.)

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[1] A contribution from the Research Laboratory of the New Jersey Zinc Co.

[2] Chemist in charge of Paint Section, Research Division, New Jersey Zinc Co.

[3] Assistant Investigator, Research Division, New Jersey Zinc Co.

[4] H. A. Nelson, "Stress-Strain Measurements on Films of Drying Oils, Paints and Varnishes," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1111 (1921).

A most interesting contrast is in the relation between time required for distortion, and the elongation at which rupture takes place. This is best illustrated by the data in Table I.

TABLE I.—TIME AS A FACTOR IN DISTENSIBILITY OF FILMS STRESSED BY FIXED LOADS

		Results of Dynamic Test			Results of Applying Fixed Loads		
	Pigment[1]	Tensile Strength, g per sq. cm.	Elongation, Per-cent	Time, Minutes	Load, Percentage of Tensile Strength	Time to Break, minutes	Elongation to Break
Film No. 262	Zinc Oxide..	62,500	12	4	50	77	17
					33	904	32
Film No. 263	Zinc Oxide..	48,500	24	3	50	52	33
					25	770	56.5
Film No. 270	Basic Carbonate White Lead.....	31,000	29	2	75	2¾	24
					50	130	20
					25	2796	8.3[a] (No break)
Film No. 250	Linseed Oil.	3,200	46	..	25	1000	29

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[1] Vehicle: raw linseed oil (a. n. 4.5), 90 per cent; turpentine, 5 per cent; liquid dryer, 5 per cent.

[a] Apparently come to rest. Load increased to one-half of tensile strength, broke after 3000 minutes at 15 per cent elongation.

TABLE II.—EFFECT OF RATE OF ELONGATION ON ULTIMATE DISTENSIBILITY

Film		Relative Humidity	Rate of Separation of Jaws, mm. per minute	Elongation at Break per cent
Film No. 1	Linseed Oil....................	60 per cent	49.00	48.5
Film No. 1	Linseed Oil....................	60 "	0.33	36.0
Film No. 2	Paint (Zinc Oxide)[1]............	60 "	150.00	12.0
Film No. 2	Paint (Zinc Oxide)[1]............	60 "	0.08	27.5[a]
Film No. 3	Paint (Basic Carbonate White Lead)[1].....................	60 "	108.00	26.0
Film No. 3	Paint (Basic Carbonate White Lead)[1].....................	60 "	0.38	23.0

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[1] Vehicle: raw linseed oil, 90 per cent; turpentine, 5 per cent; dryer, 5 per cent.

[a] Disintegration of the film structure is visible at this point. Specimen finally pulled apart at 35 per cent elongation.

Effect of Stretching Film Specimens at Fixed Rates.—The differences illustrated in Table I may be brought out in another way: If film specimens are clamped between jaws which are separated at determined rates, the distensibility varies with the time. The variation may be in either direction, depending on the type of film. This is illustrated by the results in Table II.

The differences brought out in Tables I and II are all the more interesting in view of known differences in the general characteristics of the paint films in question. Possibly a detailed study of films by this method will throw light on the fundamental causes for certain types of "checking" and "cracking."

A logarithmic plot of the curves of Fig. 1 (*a*) is presented in Fig. 1 (*b*). Those at greater loads were found to be of the common parabolic form

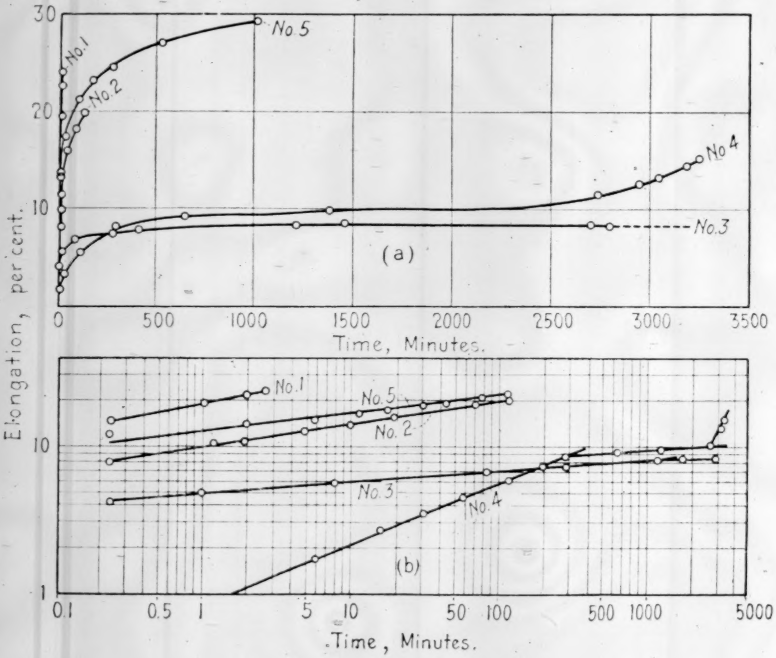

FIG. 1

(*a*) Time-elongation curves for film specimens under stress by fixed loads; (*b*) Logarithmic plot of curves in (*a*)

$$y = a x^n$$

Where y = elongation;

x = time;

a = a constant;

n = an exponent always less than 1.

At smaller loads decided inflections are introduced, of which curve No. 4 is a most notable example. As the load increases, fewer inflections are observed. It is most interesting that these inflections are so sharply defined and maintain the same general mathematical form for the equation. Undoubtedly each inflection represents the yielding of film structure of a different degree of hardness. With No. 4, the first break in the curve is at 275 minutes, and the second at about 2000. The last inflection, from 2000 minutes to the end, has not been observed at the greater loads.

Curve No. 3 shows its first inflection at about 1200 minutes and then becomes entirely parallel to the time axis. Whether this resistance would continue indefinitely without final breakage was not determined.

Although the elongation-time curve appears to retain the logarithmic form for all loads (at least for loads above one-fourth the tensile strength by the dynamic test) the slope of the curve seems to vary somewhat with each variation in load. That is, in the equation $y = a\,x^n$, the constant a and the exponent n change more or less with each load applied.

It should be noted that because of a reduction in cross-sectional area upon elongation, the loads do not remain constant. If it could be assumed that this reduction is proportional to the elongation, a correction might be made by the following formula, which is sometimes advocated by rubber technologists:

$$\text{Corrected Load} = \text{Observed Load} \times \frac{\text{Original Length} + \text{Elongation}}{\text{Original Length.}}$$

Observations failed to show definitely that the reduction in cross-sectional area is proportional to the load, although it apparently approximates this. This correction has been generally ignored by rubber technologists as being unnecessary in the routine testing of rubber specimens.

SOME FACTORS THAT INFLUENCE THE DISTENSIBLE PROPERTIES OF OXIDIZING FILMS

Moisture.—Some, or, perhaps, all of the film constituents are more or less hygroscopic. The increased distensibility due to absorbed moisture has already been briefly mentioned.[1] The important rôle that it may play is further graphically illustrated in Fig. 2. The curves shown are for three single pigment paints (raw linseed oil, turpentine and dryer) aged 50 days at 35° C. previous to testing by the dynamic test method. The specimens were exposed for 48 hours to atmospheres maintained at the desired relative humidities.

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[1] H. A. Nelson, "Stress-Strain Measurements on Films of Drying Oils, Paints and Varnishes." *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1111 (1921).

Reasons for the tendency to crack of straight zinc oxide-linseed oil paint in very dry exposures, and the very poor resistance to wear exhibited by basic carbonate white lead paints in humid localities, are evident from the relative positions of the curves.

There apparently is a connection between the so-called moisture resistance of films and the effect of moisture on the relative positions of the stress-strain curves.[1] Further significance of this shifting of the curves caused by absorbed moisture will be mentioned later.

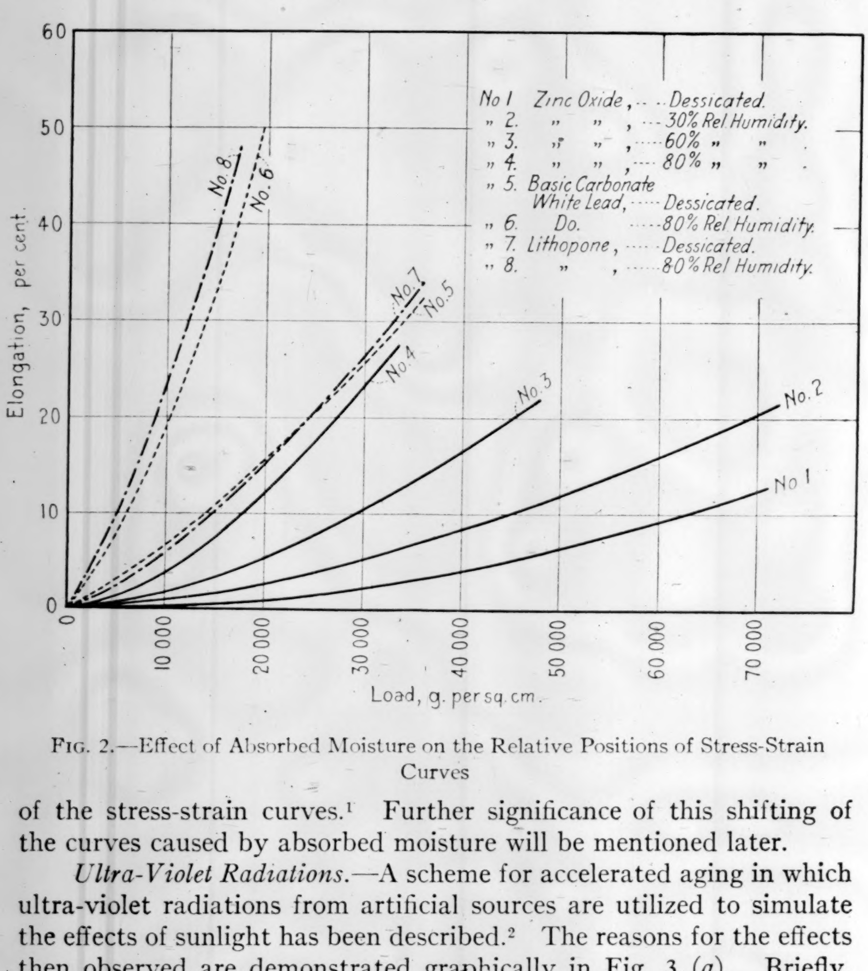

FIG. 2.—Effect of Absorbed Moisture on the Relative Positions of Stress-Strain Curves

Ultra-Violet Radiations.—A scheme for accelerated aging in which ultra-violet radiations from artificial sources are utilized to simulate the effects of sunlight has been described.[2] The reasons for the effects then observed are demonstrated graphically in Fig. 3 (*a*). Briefly,

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[1] See Appendix to the report of Sub-Committee IX on Varnish of Committee D-1 on Preservative Coatings for Structural Materials, entitled "Physical Properties of Varnish Films Indicated by Stress-Strain Measurements," by H. A. Nelson, *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 290 (1923).

[2] H. A. Nelson, "Accelerated Weathering of Paints on Wood and Metal Surfaces," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 485 (1922).

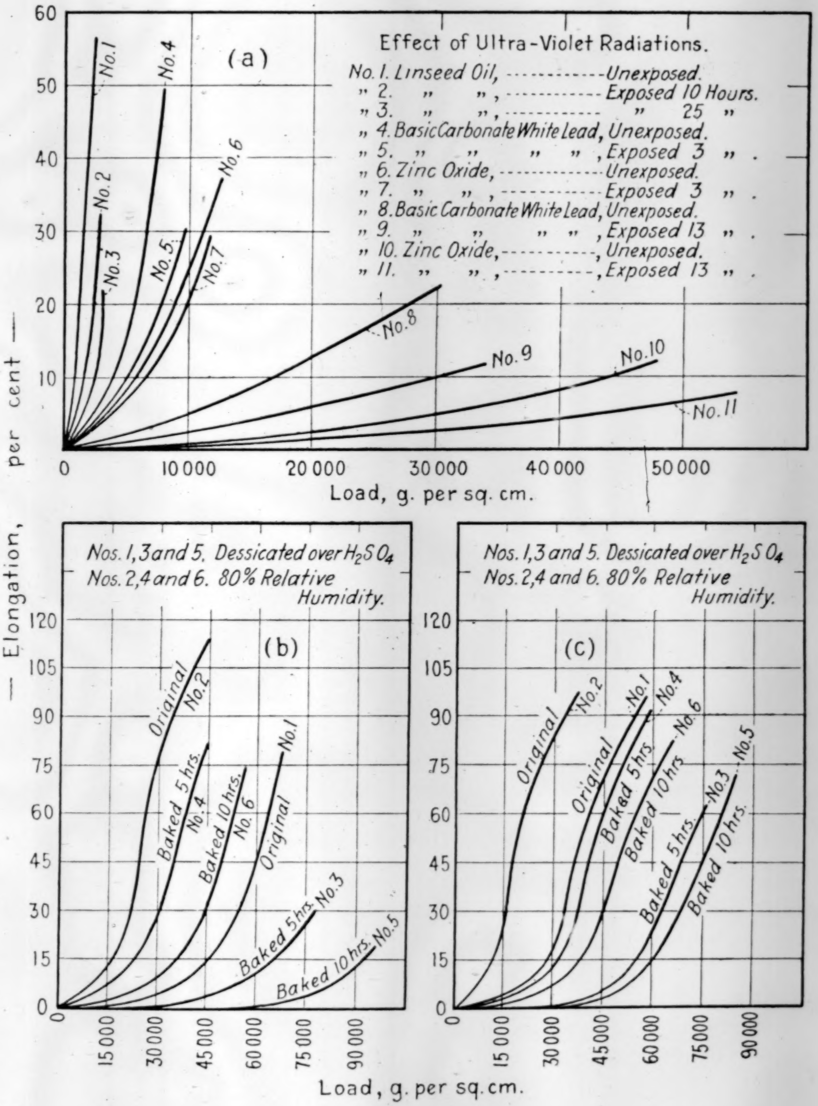

FIG. 3.—Typical Effects of Weathering Agents on the Properties of Films

(*a*) Effects of exposure to ultra-violet radiations; (*b*) and (*c*) Effects of exposure to heat we may emphasize (1) the very great susceptibility of linseed oil alone; (2) the relative protection offered by an opaque (to ultra-violet radiations) pigment such as zinc oxide as against the other extreme, basic carbonate white lead, which is known to be more transparent to ultra-violet radiations.

The hardening, and subsequent disintegration of the surface linseed oil binder through reactions induced by ultra-violet radiations must be recognized as a fundamental cause for the failure of paints by chalking. Visible evidence of chalking may be hastened by temperature expansion and contraction effects, but chalking may be reproduced on pigments such as basic carbonate white lead by light exposure alone without introducing any temperature or moisture variations.[1]

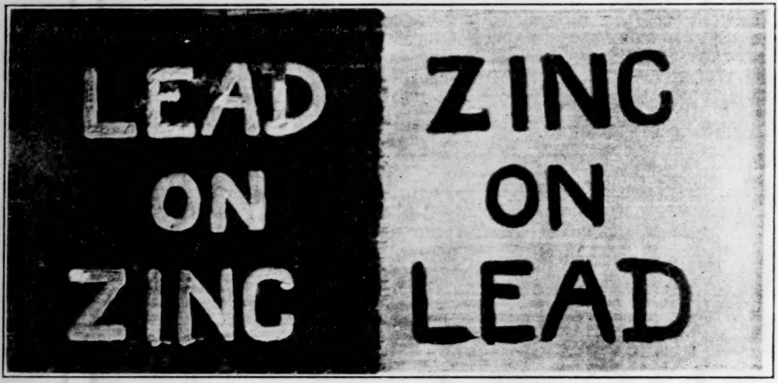

FIG. 4.—Photograph Taken by Reflected Light Radiations from the near Ultra-Violet, Illustrating Differences in Transmission by Two Pigments

That the two pigments in question do differ in the degree to which they transmit ultra-violet radiations is indicated in an interesting way in Fig. 4.[2] To the eye, the lettering and background are white and quite indistinguishable. Photographed[3] at an angle of 90 deg. by reflected light from the near ultra-violet region of the spectrum (range about λ = 3800 Å. U. to approximately λ = 3100 Å. U.) the zinc oxide reflected no radiations to activate the plate. Hence, the black photograph instead of white. The basic carbonate white lead reflected sufficient radiations to activate the plate and so appears an approximately normal white.

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[1] H. A. Nelson, "Stress-Strain Measurements on Films of Drying Oils, Paints and Varnishes," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1111 (1921).

[2] The authors wish to acknowledge the cooperation of P. R. Croll and A. H. Pfund in obtaining the photograph reproduced in Fig. 4.

[3] By light from iron arc through Corning Glass G 586 A.W. Camera with crown-flint glass lenses.

Reflected light from these surfaces would normally be possible only by transmission of the radiations from crystal to crystal and refraction during the course of this transmission until a certain portion finally emerges from the surface as reflected light. Obviously, then, one pigment has transmitted the radiations while the other has not.

Heat.—The immediate effect of increased temperature is to soften the film structure.[1] No doubt this is due to a general increase in the mobility of the film constituents or perhaps, an increase of the liquid phase brought about by the melting of some otherwise solid material.

Naturally, the reverse effect takes place at lower temperatures. The true extent of the loss of distensibility at low temperatures has not been accurately determined.

The well-known effects of continued application of heat are to hasten oxidation and polymerization. The most informing examples of how these may vary are those observed for varnishes. Fig. 3 (*b*) represents a china wood oil - ester gum varnish with 30 gal. of oil to each 100 lb. of gum. The varnish used in the tests recorded in Fig. 3 (*c*) has 40 gal. of china wood oil to each 100 lb. of ester gum. Of the curves shown, Nos. 1 and 2 are for the original films 35 days old; Nos. 3 and 4 after baking 5 hours at 95° C.; and Nos. 5 and 6 are for films after baking 10 hours at 95° C. Specimens Nos. 1, 3 and 5 have been desiccated over $CaCl_2$ and Nos. 2, 4 and 6 maintained at 80 per cent relative humidity for at least 48 hours just previous to testing. In this case, the decreasing effects of heat and moisture with increases in the oil content are evident.

Reaction Products (Soaps).—It is known that the characteristics of paint films are modified by soaps formed with acid vehicles. Fig. 5 (*a*) illustrates the effect of increased zinc soaps on the zinc oxide paint film. It can be readily seen that even the differences between a vehicle of acid number 3 and one of acid number 6 would have a marked effect on the distensibility of the film when dry. This argues for vehicles of low acid number for use in zinc oxide paints.

It is to be expected that the plastic lead soaps would have the opposite effect, as indicated in Fig. 5 (*b*). It also follows that very highly acid vehicles should be detrimental to lead paints used in regions of high relative humidity, since they tend to make the film excessively soft.

Addition of Hygroscopic Agents.—The effect of absorbed moisture, shown in Fig. 2, suggests the addition of materials more hygroscopic than the normal constituents of the film. For example, glycerol, which is quite hygroscopic, should normally be present in small amounts. If the amount present is increased by additions to the vehicle, decided softening results, as well as an increased effect of moisture. This last is illustrated by the wider spread of the curves at desiccated and 80 per cent relative humidity shown in Fig. 5 (*c*).

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[1] H. A. Nelson, "Stress-Strain Measurements on Films of Drying Oils, Paints and Varnishes," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1111 (1921).

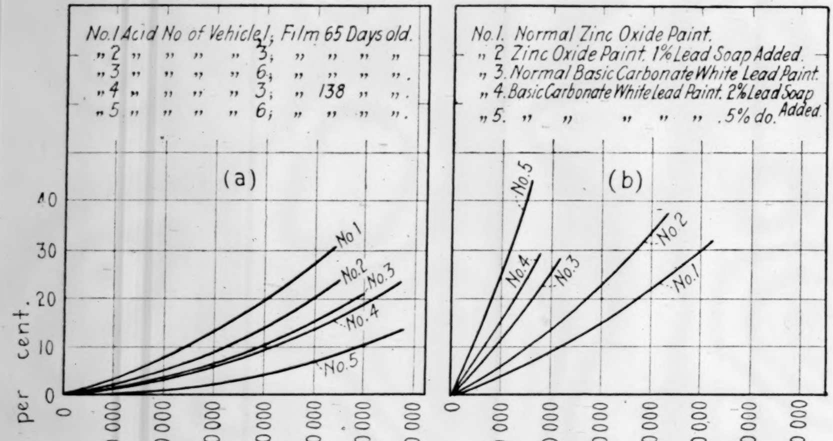

FIG. 5

(*a*) and (*b*) Effects of the normal reaction products (soaps) formed in the film; (*c*) and (*d*) Effects of adding materials of varying degrees of hygroscopicity

The effect of moisture can be reversed by adding some material which is slightly hygroscopic, such as castor oil (Fig. 5 (*d*)), although the total effect is to soften the film at lower humidities due to an increase in the amount of non-oxidizing liquid phase present.

Generally, the distensibility of a film is determined by the mobile properties of the film constituents and the surface relationships between the several phases. The hygroscopicity of the film constituents will then be a determining factor in so far as the absorbed moisture affects the mobile properties or induces changes in the existing surface relationships.

TABLE III.—COMPOSITIONS OF PAINTS USED FOR HUMIDITY EQUILIBRIA CURVES (FIG. 6)

		PIGMENT, PER CENT BY WEIGHT					
FILM	PIGMENT-VEHICLE[1] RATIO (BY WEIGHT)	ZINC OXIDE	BASIC CARBONATE WHITE LEAD	SULFATE WHITE LEAD	TITANIUM PIGMENT	NEW PROCESS LITHOPONE	INERT
a.....	50 to 50	100	..	..	..	..	......
b.....	70 to 30	40	40	..	..	..	10 Barytes 10 Asbestine
c.....	55 to 45	50	50	..	..	..	......
d.....	70 to 30	..	..	100	..	..	......
e.....	65 to 35	..	..	..	100	..	......
f.....	55 to 45	30	..	..	70	..	......
g.....	55 to 45	40	..	..	..	40	20 Whiting
h.....	55 to 45	..	..	..	..	100	......
i.....	65 to 35	..	100	..	..	..	......

[1] Vehicle composition: raw linseed oil, 90 per cent; turpentine, 5 per cent; liquid dryer (Pratt & Lambert), 5 per cent.

HUMIDITY EQUILIBRIA OF PAINT FILMS

The effect of moisture on the distensibility of films suggests that appreciable amounts of moisture are absorbed. Reference to Curves Nos. 1, 2, 3 and 4, Fig. 2, will show that the effect is not directly proportional to the relative humidity. This predicts that the humidity equilibria curves will be more flat at low humidities. The results shown in Fig. 6 were obtained by passing nitrogen gas over sections of films, all of which were of the same age (22 days at 35° C. and 30 per cent relative humidity). The compositions of the paints were as given in Table III.

The dry weight (gas desiccated over P_2O_5) was taken as the basic weight. The relative humidity of the gas was then increased step by step by varying the concentrations of the H_2SO_4 solutions used.[1] Periodic weighings were made until a flat portion of the weight increase curve was attained. The time required to come to equilibrium varied from 24 hours to 100 hours, but for some reason, except in one case

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[1] Wilson, *Journal Industrial and Engineering Chemistry*, Vol. XIII, No. 4.

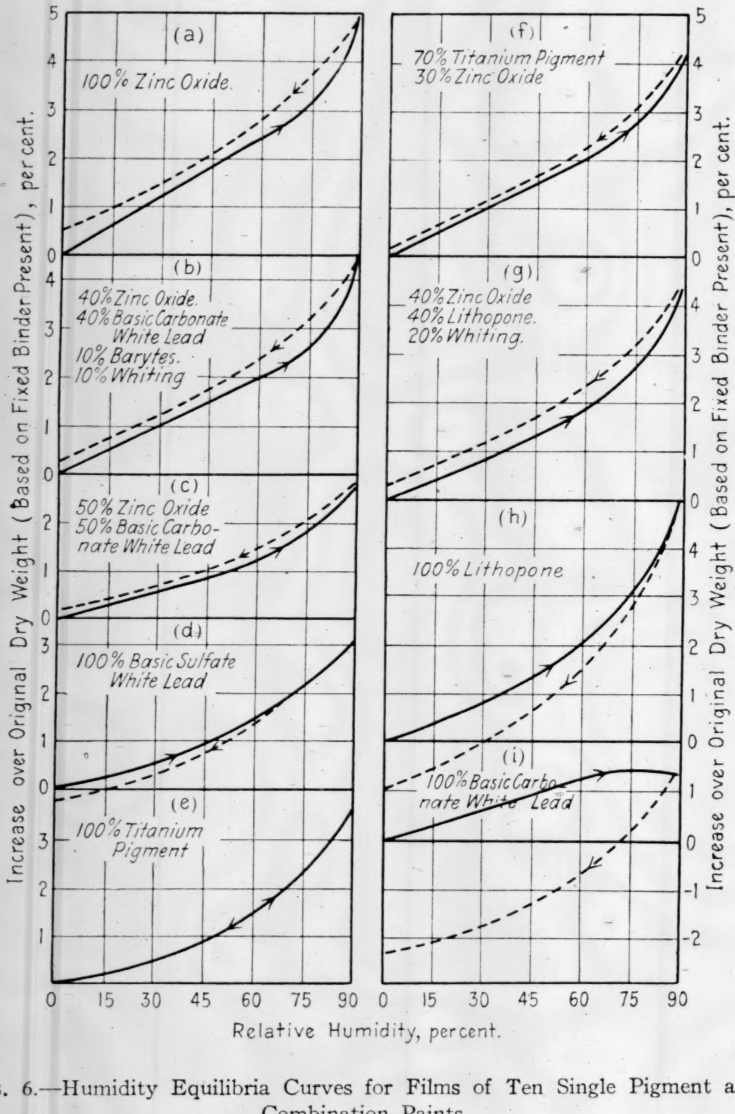

FIG. 6.—Humidity Equilibria Curves for Films of Ten Single Pigment and Combination Paints

no direct relation was established between the time to come to equilibrium and the characteristics of the curves. The exception was the titanium pigment (*e*), which generally attained equilibrium at a more rapid rate.

A return run was made from saturation at 90 per cent relative humidity to desiccation with P_2O_5 in order to observe hysterisis effects. These are represented by broken line curves in Fig. 6.

The times required to reach equilibrium in each case prolonged the experiment somewhat. It was hoped that in the presence of the inert nitrogen gas no further reactions would occur. This evidently was not realized in the case of basic carbonate white lead, Fig. 6 (*i*). Here either one, or both of two things must have taken place: (1) a reaction resulting in loss of weight, or (2) the formation of less hygroscopic film structure (linoxyn). It is probable that both occurred, but that the first is mainly responsible for the relatively great decrease in weight. New tests have been inaugurated in which the time element will be eliminated in so far as possible.

It might be inferred that the spread of the curves in Fig. 2 would indicate the amount of moisture absorbed. While at first this seemed to be the case, some recent tests on the varnishes referred to under Figs. 3 (*b*) and (*c*) and several others of the china wood oil - ester gum type indicated that the effects on the stress-strain curves may vary greatly and yet the apparent amount of moisture absorbed (as measured by gain in weight) will remain practically the same in each case. This might be explained since slight additions to the liquid phase in a short oil varnish, in which the liquid phase is already comparatively low, could be expected to show greater differential effects than the same amount of moisture added to the liquid phase of the long oil varnish, where a high proportion of liquid phase is already present.

The hysterisis curves are interesting. Where the return curve is above (or below) the original, it might indicate (1) formation of more (or less) hygroscopic reaction products or (2) that, although weighings were made until the flat part of the weight increase (or decrease) curve was reached, true equilibrium had not yet been attained. In this connection it should be noted that in the case of a comparatively inert pigment, such as the titanium pigment, where, possibly, no appreciable amount of reaction product (soap) had been formed, no hysterisis effect was observed. As previously stated, this film also showed a tendency to reach equilibrium weight more quickly than other films.

The general form of the humidity equilibria curve is as predicted from the observed effects on the stress-strain diagrams. However, at the present stage of this work few conclusions can be arrived at as to the meanings of the relative amounts of moisture absorbed.

Careful comparison will reveal some differences in the forms of the various curves, aside from the peculiarities exhibited by *i*, basic carbonate white lead.

It is interesting that in the case of the mixed pigment paints, the form and position of the curve is influenced by the individual pigments present. Film *c*, 50 per cent zinc oxide, 50 per cent basic carbonate white lead, is an example.

CONCLUSION

The data so far accumulated clearly shows the value of this method of attack on paint and varnish problems. Many important conclusions are already indicated, but we shall await further accumulation of evidence before coordinating too widely. Investigations following the lines indicated are being continued.

Valuable comment and suggestions by fellow members of our Research Organization are gratefully acknowledged.

SOME OPTICAL PROPERTIES OF WHITE PAINT PIGMENTS IN THE ULTRA-VIOLET SPECTRUM

By A. H. Pfund[1]

Introduction

The degree to which any science approaches perfection may be adjudged by its ability to predict future events. While, in general, the possibility of prediction is rather remote where complex chemical reactions extending over long intervals of time are concerned, the prospects are considerably brighter where physical properties alone are considered. For example, in the case of white paint pigments we are already able to announce in advance, after having determined refractive index and particle size, whether or not the resulting paint will possess adequate brightness. We should like very much to make similar predictions in regard to the life of the paint. Undeniably, one of the principal causes of the deterioration of paints is ultra-violet light.[2] Other things being equal it seems reasonable to suppose that the greater the opacity of the paint pigment to ultra-violet light, the less will be the deterioration due to this cause. With a view of making possible a correlation of deterioration and opacity to ultra-violet light and later, possibly, a prediction as to the behavior of new pigments, the following investigation was undertaken.

Reflection from Dry Pigments

Determinations of the coefficients of diffuse reflection from dry pigment surfaces (sometimes known as "albedo") are intimately associated with the transparency of the material in question. When light falls upon a thick layer of fine particles, it penetrates a large number of particles before being finally returned. The color assumed in reflected light is, therefore, essentially the same as in transmitted light. This is well illustrated by potassium bichromate. The pulverized material has the same orange tint, though less saturated, than is shown by the massive crystals viewed in transmitted light.

The arrangement of apparatus used in the determination of reflection coefficients is shown in Fig. 1, where L_1 is a small quartz-mercury lamp which illuminates the pigment surface, *P*, at an angle of incidence[1] of 50 deg. A small metallic cap was filled with the pigment which was then surfaced, under pressure, with a ground glass plate. The diffusely reflected light leaving *P* normally was allowed to enter the 1-mm. wide slit *S* of a small concave-grating spectrograph, the grating being mounted at *G* and the photographic plate at *K*. So as to minimize the labors of calculation of results, the photographic plate was chosen as large as possible (4 by 5 in.) so as to accommodate as many exposures (40 or more) on the same plate. In order to obtain numerical results, it was necessary to remove the pigment sample *P* and to swing the arc into position L_2 so that the direct light from the arc entered the spectrograph. Due to the great intensity of this light a narrow diaphragm, *D*, was found necessary to bring about the proper reduction. The light was further reduced by means of a rotating sector, *R. S.*, whose relative angular openings were adjusted, successively, to 100, 50, 25, 12.5 and 6.25. All exposures were of 2 minutes duration and all spectra were recorded on the same plate. Since the photographic range extends into the visible spectrum where absolute measurements of reflection coefficients are available, it is possible, by adequate calculation, to reduce the results, obtained photograph-

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[1] Associate Professor of Physics, Johns Hopkins University.

[2] H. A. Nelson, "Accelerated Weathering of Paints on Wood and Metal Surfaces," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 485 (1922).

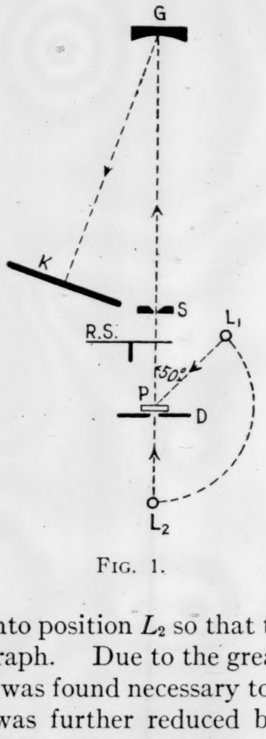

FIG. 1.

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[1] It has been shown by A. H. Taylor (unpublished data) that a rough surface, illuminated as here indicated, yields the same reflection coefficients as when the illumination is uniform in all directions.

ically, to absolute values. For the sake of convenience, all results are referred to the reflection coefficient of MgO (fumed heavily over a surface of $MgCO_3$), taken arbitrarily at 98 per cent at $\lambda = 5460$ Å. U.

A detailed discussion of the well-known mode of procedure followed in reducing the results obtained photographically into reflection coefficients is too involved to be presented within the scope of this paper. It might be mentioned, however, that the photographic densities of the various spectral lines were measured with a new microphotometer, developed by the author. A detailed description will be published elsewhere.

The numerical data are plotted in Fig. 2. These curves are essentially self-explanatory. While all pigments, inclusive of the inerts, possess great brightness in the visible spectrum (wave-lengths greater than 4000 Å. U.) the disparity in the ultra-violet becomes large. It is to be borne in mind that we are here dealing with an "infinitely" thick layer of pigment, and that, consequently, the penetration of light into the pigment will be slight when the reflection coefficient is small and will be large where the reflection coefficient is large. Since these curves partake of the nature of transmission curves it is possible to draw roughly qualitative conclusions as to the opacities of the various pigments. Probably the most striking result is that shown by zinc oxide which becomes practically opaque near

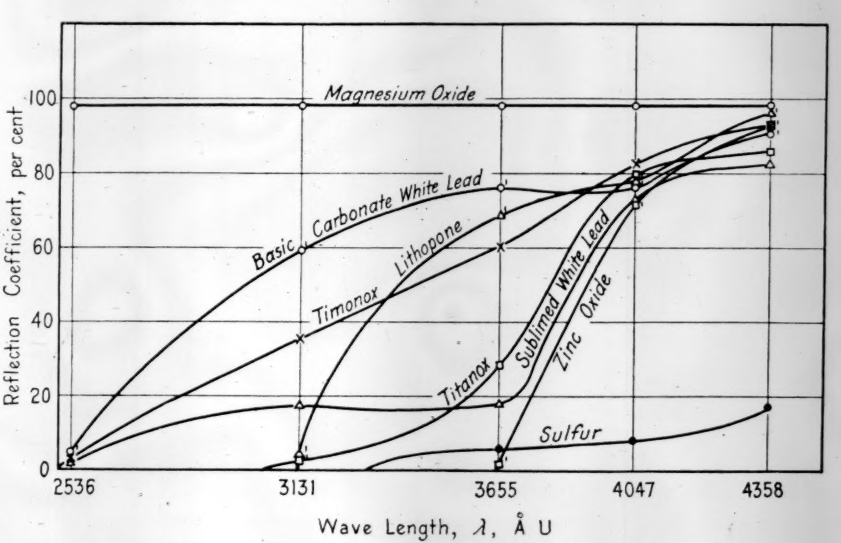

FIG. 2.—Reflection Coefficient Curves of Pigments, MgO Taken as 98 per cent

$\lambda = 3600$ Å. U. Basic carbonate white lead is by far the most transparent, while the other pigments fall in between these limits. The inerts, as a class, are much less opaque, particularly in the short wave-length regions. In addition, the very great opacity of powdered sulfur is demonstrated. Incidentally it is shown that fumed MgO has an almost constant reflecting power from the visible spectrum out to 2500 Å. U. and may, hence, serve admirably as a reflection standard in the ultra-violet as well as in the visible spectrum.

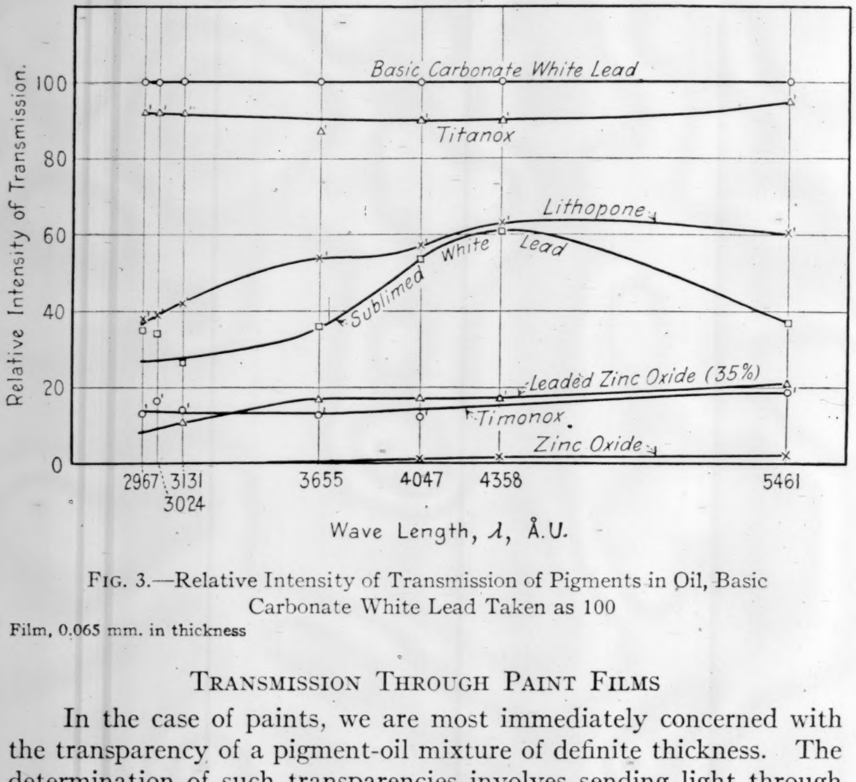

FIG. 3.—Relative Intensity of Transmission of Pigments in Oil, Basic Carbonate White Lead Taken as 100

Film, 0.065 mm. in thickness

TRANSMISSION THROUGH PAINT FILMS

In the case of paints, we are most immediately concerned with the transparency of a pigment-oil mixture of definite thickness. The determination of such transparencies involves sending light through the paint, which, unlike the clear oil film, scatters the light much like ground glass. Hence, without resorting to a more complicated experimental arrangement, it is not possible to obtain exact values upon simply measuring the intensity of a small part of the light diffusely transmitted. While an investigation yielding true transparencies is to be undertaken shortly, it was deemed sufficiently worth while to carry out a number of measurements along the simpler lines above mentioned. Much the same apparatus as shown in Fig. 1 was employed, the only difference being that the light from the mercury arc in position L_2 was allowed to pass through a paint film, between quartz plates, inserted at *P*. So as to make the results comparable, masses of the various pigments proportional to their specific gravities were rubbed down with the same volume of pale linseed oil which had been allowed to bleach in sunlight for several days. The volume ratio of pigment to vehicle is 1 : 60 for the curves shown in Figs. 3 and 4.

Since the thickness of the usual paint film is of the order of 0.15 mm., the present films were made much thinner. A ring of tinfoil 0.065 mm. thick separated the two quartz plates and thus limited the thickness of all mixtures to that amount.

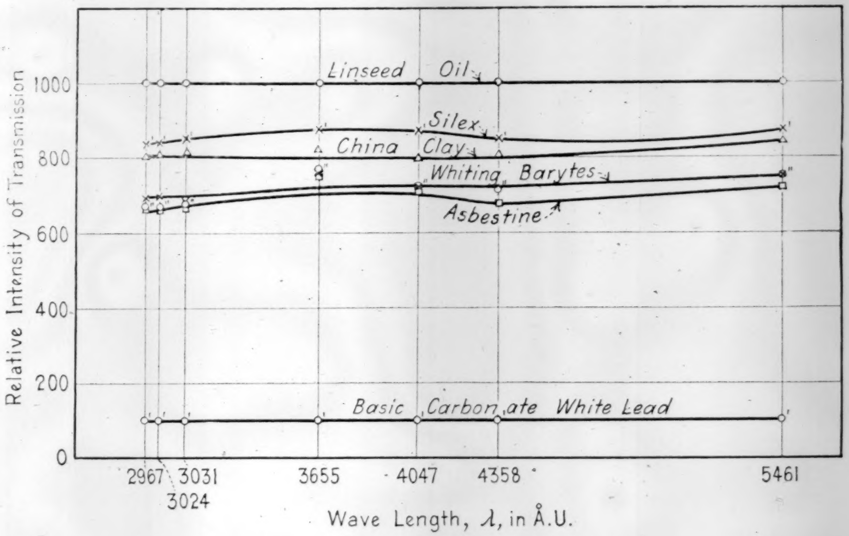

FIG. 4.—Relative Intensity of Transmission of Inerts in Oil, Basic Carbonate White Lead Taken as 100

Film, 0.065 mm. in thickness

It became apparent at once that linseed oil became opaque to wave-lengths less than 2900 Å. U. Since pigments are being studied, the decreased transmission due to the pigment was determined; hence, while Fig. 4 apparently indicates a constant transmission for linseed oil, we merely wish to show the nature of the change in transmission occasioned by the pigment.

Before discussing the results in detail, it is to be emphasized that these results are of the roughest, qualitative character. First of all, no attention has been paid to the loss of light occasioned by reflection from the paint film and, what is even more important, no corrections were made for the varying degrees of diffusion in the different films. The total quantity of light transmitted by a piece of polished glass is but slightly greater than that transmitted when the surfaces are rough-ground. Yet, when held before the eye, the difference between the two samples is very great. These difficulties can be overcome only by mounting the diffusely-transmitting surface over openings and between two diffusely-reflecting hollow spheres in order to determine by methods free from objections the separate intensities of the incident, reflected and transmitted light. Lack of time precluded the presentation of such results in the present paper. This work, however, will be completed in the near future.

Transmission curves for representative pigments are shown in Fig. 3. All results are relative to corroded white lead whose transmission is taken arbitrarily at 100 per cent. This simply means that the transmission of the small amount of pigment present in the mixture studied is essentially non-selective, relative to the transmission of linseed oil. Little need be said in regard to these curves except, possibly, to point out the great percentage increase in transmission of leaded zinc oxide as compared with straight zinc oxide. This is undeniably due to the presence of sulfate white lead whose transmission is shown to be quite large.

The results obtained for inerts are shown in Fig. 4. Due to the great transparency of these substances, it was found necessary to increase the pigment-vehicle ratio (by volume) to 1 : 6. By suitable calculation, the results were reduced so as to apply to the former ratio 1 : 60. For sake of comparison the curve for basic carbonate white lead is reproduced from Fig. 3. All that need be said about inerts is that their transmission is not only very high but is also nearly non-selective. In general, it is not possible to make a strict comparison of these results with those shown in Fig. 2 because we are not dealing here with layers of "infinite" thickness.

While it is not the purpose of this paper to take up the larger subject of the deterioration of paint films due to light, it may not be out of place to call attention once more to the fact that the susceptibility of the oil film to decomposition is one of the principal causes of failure. Paint pigments as a class are but little affected by light. Those radiations which are absorbed are transformed into heat without bringing about decomposition of the pigment. In the case of the oil, however, the absorbed radiations effect, in part, chemical decomposition. It is therefore the function of the pigment to absorb harmful radiations before they have penetrated the paint films to any appreciable extent. This is accomplished most effectively by sulfur and zinc oxide and least effectively by the inerts. There are, of course, a large number of other elements such as elastic properties, continued reaction between pigment and oil, etc., which play a highly important rôle in deterioration of paints; yet, the above considerations are not without significance.

LIGHT SENSITIVENESS OF LITHOPONE

While it has long been known that ultra-violet light of short wave length is particularly active in producing a blackening of lithopone, no quantitative data on this subject are available. The advent of modern, light-resistant lithopones has made the general subject of "sun darkening" relatively unimportant from a practical standpoint; nevertheless, the subject possesses great scientific interest. While the time is not yet ripe for the presentation of a theory of darkening, some experimental data bearing on this subject may be of interest.

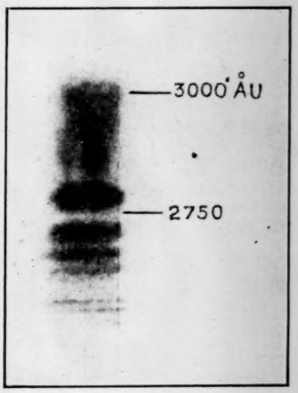

FIG. 5.—Darkening on a Sample of Lithopone, Showing Effect of Various Wave Lengths of Light

In order to form some qualitative idea as to the effectiveness of the different spectral regions in producing sun darkening, a sample of light-sensitive lithopone was rubbed down in water and covered with a quartz plate. This specimen was then mounted in the focal plane of a quartz spectrograph so that the entire spectrum from an iron arc was projected upon the lithopone surface. In other words, the lithopone surface functioned as a photographic plate and indicated the regions of greatest light sensitiveness by darkening in these particular spectral regions. The darkened sample was then quickly photographed and thus a permanent record was obtained. The results are shown in Fig. 5. It is to be noted that wave lengths greater than 3200 Å. U. show very little darkening.

In order to establish a relation between darkening and the wave length of the incident light, it is necessary to eliminate the variations in energy carried by the light of different wave lengths. This is accomplished by means of a quartz monochromator, through whose ocular slit a beam of nearly monochromatic light is allowed to fall upon the blackened junction of a vacuum thermopile connected to a sensitive galvanometer. Since the galvanometer deflections are proportional to the incident energy only and independent of wave length, a standard energy of incident light is realized throughout the investigation. This is the so-called method of "equal energies." Having established this standard energy for a given wave length, a sample of lithopone, rubbed down in water and covered by a quartz plate, is substituted for the thermopile behind the ocular slit and the time necessary for the appearance of the first visible darkening is recorded. This procedure is repeated for the various wave lengths. The reciprocals of the number of seconds required to produce noticeable darkening, when plotted against wave length, yield the desired "sensitivity"

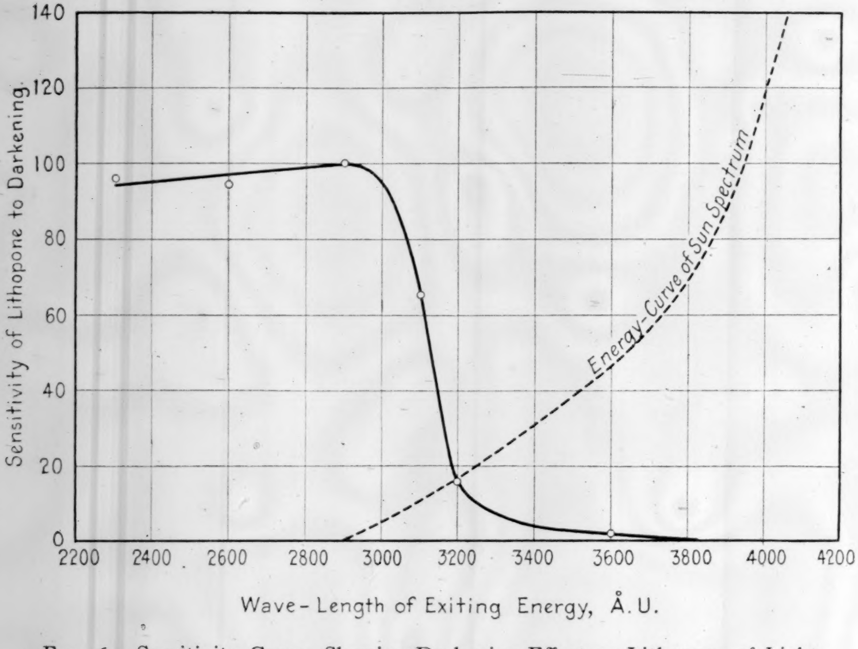

FIG. 6.—Sensitivity Curve, Showing Darkening Effect on Lithopone of Light of Various Wave Lengths

curve. Such a curve, obtained for one of the samples of old or light-sensitive lithopones, is shown in Fig. 6.

This curve shows that, while the sensitivity is essentially zero in the visible, it rises to a high value in the ultra-violet spectrum near 2900 Å. U. and thereafter remains essentially constant. For the sake of ease of discussion, the energy curve for the sun's spectrum is recorded on the same sheet. A comparison of these two curves reveals the very fortunate state of affairs that the visible solar spectrum stops very nearly at the same wave length at which lithopone is most light sensitive. Since ordinary window glass is practically opaque at wave lengths less than 3200 Å. U. it is easy to see why lithopone, exposed to sunlight passing through a window, shows so very little blackening.

While this sensitivity curve applies to one of the most light sensitive of lithopones, it may be said that a curve of the same general type probably applies to light-stable lithopone also. Experiments carried out with unresolved sunlight but filtered through various screens of known transmission limits have shown that, barring minor variations, all lithopones reveal the same general type of sensitivity curve.

The recent improvements in light stability, however, are quite remarkable. A standardized test apparatus for determining the light stability of lithopones has been devised by H. Eastlack, who employs as a source of light the type of iron arc[1] developed by the author for spectroscopic research. Light stability may be measured in terms of the time of exposure necessary for the first darkening. The following table presents a series of typical results obtained about two years ago.

	LIGHT STABILITY, SECONDS EXPOSURE	REMARKS
Lithopone *A*	4	Old process lithopone
Lithopone *B*	35	New process lithopone
Lithopone *C*	135	New process lithopone

The one salutary effect of light on paints is its bleaching action on yellowed surfaces. While no quantitative data have been gathered on the bleaching effects of the different spectral regions, some qualitative results have been obtained by exposing a badly yellowed paint film (linseed oil) to sunlight. Adjacent patches of the film were covered with the screens described below. The second column indicates the shortest wave length reaching the film.

	WAVE LENGTH	REMARKS
(*a*) No screen	2900 Å. U.	Vigorous bleaching
(*b*) Quartz	2900 Å. U.	Vigorous bleaching, same as (*a*)
(*c*) Glass plate	3200 Å. U.	Vigorous bleaching, same as (*a*)
(*d*) Crookes' glass	3600 Å. U.	Vigorous bleaching, slightly less than (*a*)
(*e*) Noviol (yellow) glass	4900 Å. U.	Bleaching, pronouncedly less than (*d*)
(*e*) Ruby glass	6000 Å. U.	No bleaching

From this table it is seen that, whereas light of wave length 3200 Å. U. or less is required to produce pronounced deterioration of paints and darkening of lithopones, the beneficial bleaching action extends to much greater wave lengths—in fact, up into the visible spectrum.

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[1] *Astrophysical Journal*, Vol. 27, p. 296 (1908).

Quite recently the writer has found that if two similar zinc plates be coated with lithopone and be immersed in a weak electrolyte, an electromotive force is set up when one of the lithopone surfaces is illuminated. The response of a galvanometer, connected in series with the above cell, is quite definite, even in diffuse daylight. A more extensive report of this work will be presented later.

In conclusion, the writer desires to express his indebtedness to Mr. George F. A. Stutz for a large amount of careful work, both on the apparatus and on the measurements.

THE EFFECT OF CERTAIN PAINT FILMS ON ULTRA-VIOLET LIGHT[1]

BY R. L. HALLETT[2]

It has long been known that the optical effect of substances of various kinds on ultra-violet light may be quite different from the optical effect of the same substances on visible light. Mr. R. W. Wood of Johns Hopkins University did some very interesting work along this line,[3] demonstrating the marked differences in reflection properties of different substances, more particularly different kinds of paint, when illuminated by visible light and by ultra-violet light.

This field is rather new and is a very interesting one for both theoretical and practical reasons. For the past year some experimental work has been carried on at the Research Laboratories of the National Lead Co. to study the optical effect of paint films of different kinds on ultra-violet light. Because the field is so new, it was necessary to develop special apparatus of the photometric type, but because the light used was invisible, it was necessary to record light intensities photographically instead of by visible photometry.

THE PHOTOMETER

The apparatus which was finally designed and found very satisfactory is really a photometer in which light intensities, at the various points where intensity must be measured, are recorded by means of sensitive photographic films. The general arrangement of the apparatus is illustrated in the diagram shown in Fig. 1. The source of illumination, *A*, is an ordinary 250-volt Cooper-Hewitt lamp with a 4 by $\frac{1}{2}$-in. mercury arc. The light from the Cooper-Hewitt lamp passes through a special Corning glass filter, *B*, made by the Corning Glass Works, and described in U. S. Bureau of Standards *Technologic Paper No. 148*. This special glass is the one designated "G586J" in Fig. 21 on page 23 of Paper No. 148. It will be seen from that figure that the Corning glass is an excellent filter for visible light, permitting only a very small amount of visible light of the shorter wave lengths to pass through, but permitting a considerable amount of ultra-violet light to be transmitted.

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[1] Contributed by the Research Laboratories of the National Lead Co.

[2] Chemist, National Lead Co., Brooklyn, N. Y.

[3] Royal Institution of Great Britain, May 19, 1911.

After having the visible light waves removed, the light then passes through a sheet of quartz glass ground on the under side, shown at *C*. The ground glass is used to diffuse the light which passes through it. The diffused light from the under side of the ground quartz glass passes down into the apparatus, which is entirely enclosed by a wooden cabinet painted black on the inside and lined with sheet lead on the outside. At *D* is placed a quartz concave-convex lens, concave side up, which is evenly illuminated by the diffused light from the ground glass. The light striking the lens is collected by the lens and is transmitted into the lower part of the apparatus. The lens is fitted on the upper side with an iris diaphragm, which is graduated to give any desired area of opening. Varying the size of opening of the iris diaphragm varies the relative intensity of the light transmitted into the lower part of the apparatus, as the intensity of the transmitted light is directly proportional to the size of the diaphragm opening. Below the quartz lens the light passes into a second part of the apparatus through a square hole, *E*, in a wooden frame lined with sheet lead. All light entering the hole must first pass through the lens, as

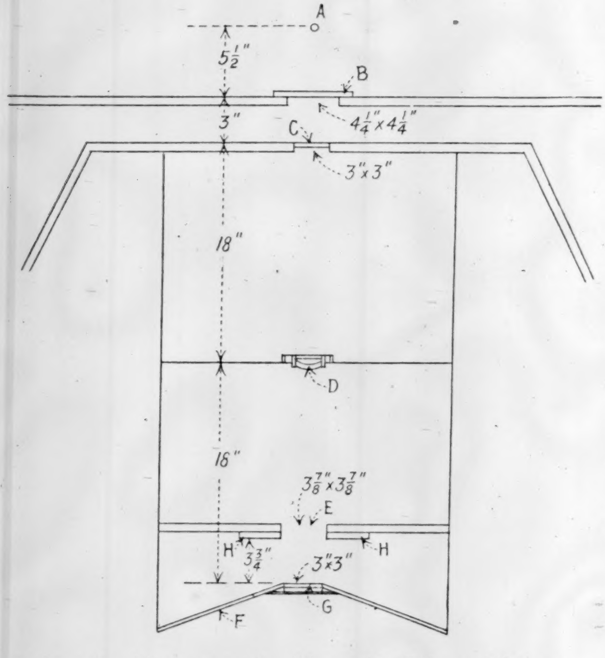

FIG. 1.—Photometer for Measuring Effect of Paint Films on Ultra-Violet Light.

light from *C* which passes outside of the lens cannot enter the hole. At a short distance below the hole is placed a shield, *F*, made of sheet lead with a square opening, *G*, in the center. The opening in the shield is somewhat smaller than the hole *E*. At the positions *H*, on each side of the opening *E*, are placed inverted plate holders which may hold either plates or pieces of sensitive photographic paper. The opening *G* in the shield is fitted with a sheet of clear quartz glass which is coated with various paint films when a study of the effect of the paint films on ultra-violet light is to be made.

The optical effect of the paint films on ultra-violet light was studied by determining the percentage of incident ultra-violet light transmitted by each paint film, and the percentage reflected by each paint film, the assumption being made that the remainder was absorbed.

In measuring photographically the intensity of ultra-violet light, Artura Iris, Double Weight, D, Rough Matte, photographic paper (made by the Eastman Kodak Co.) was used. This paper was selected because when exposed to light and developed it gives shades of gray and black, and because it has a matte surface and medium response to light exposure.

Leakage of light was tested by exposing sensitive photographic paper at the positions *H* with the hole in the shield open, and with the Cooper-Hewitt lamp burning. No appreciable effect on the sensitive paper was found by these blank tests.

CALIBRATION

The apparatus was calibrated for transmission by exposing at the shield position, *G*, pieces of the photographic paper placed on the under side of the clear quartz glass, and in contact with the glass, with the sensitive side of the paper up, but with no paint film on the glass. In this position pieces of paper were exposed for one minute with varying size of opening of the iris diaphragm above the quartz lens. The exposed pieces of paper were developed in a carefully standardized manner. A piece of paper which had not been exposed and a piece of paper which had been exposed for a long time, sufficient to be considered completely exposed, were also developed in the same manner. After development the degree of black which resulted was measured with the Howland Color Photometer.[1]

The readings taken with the Howland instrument were corrected to give the percentage of black in terms of absolute black and white.

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[1] Arthur Howland, *Painters' Magazine*, February, 1918; Arthur Howland, *Science Conspectus*, Mass. Inst. Tech., Vol. VI, No. 2, 1916.

The percentage of absolute black is, of course, the reciprocal of the percentage of light reflection. Considering the percentage of absolute black shown by the unexposed paper as zero black, and the percentage of black shown by the completely exposed paper as 100 per cent black, all of the other exposures were calculated to the same series, which is designated the "Artura Scale." These various exposures

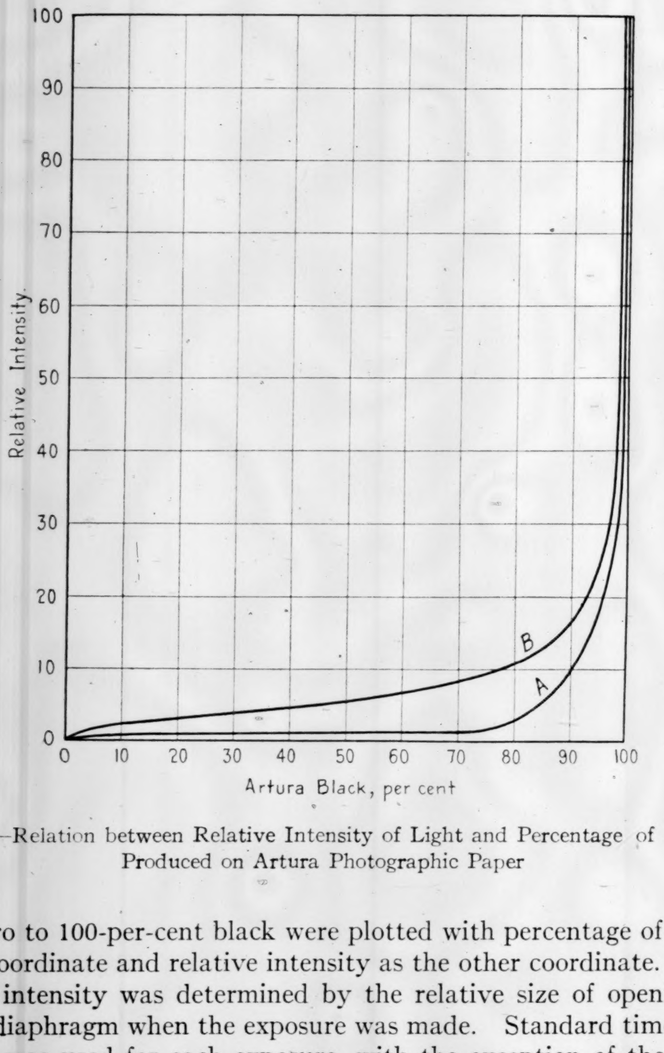

FIG. 2.—Relation between Relative Intensity of Light and Percentage of Black Produced on Artura Photographic Paper

from zero to 100-per-cent black were plotted with percentage of black as one coordinate and relative intensity as the other coordinate. The relative intensity was determined by the relative size of opening of the iris diaphragm when the exposure was made. Standard time (one minute) was used for each exposure, with the exception of the complete exposure for which practically infinite time was used because infinite intensity was not possible, and complete exposure would result with either infinite time or infinite intensity. The resulting curve is shown as *A* in Fig. 2. This curve, when plotted on semi-logarithmic paper is a straight line throughout most of the range of the curve with curved portions at each end, as shown in Fig. 3. This is in accordance with similar work reported by other investigators.[1]

It will be seen that *A* in Fig. 2 is a curve from which the relative strength of any unknown intensity of ultra-violet light may be determined by exposing a piece of Artura paper to the light of unknown intensity for standard time, developing it, and then comparing with curve *A* the Artura scale percentage of black which results.

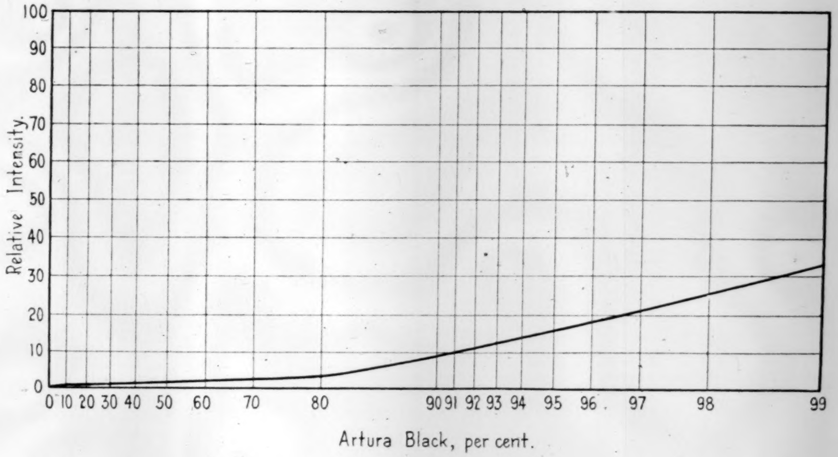

FIG. 3—Relation between Relative Intensity of Light and Percentage of Black Produced on Artura Photographic Paper Replotted Semi-Logarithmically

The percentage of ultra-violet light transmitted by a paint film was determined by coating the quartz glass at *G* with the paint, using a paint coat of standard thickness, and exposing a piece of Artura paper for standard length of time and with maximum diaphragm opening, with the paper placed on the under side of the quartz glass on which the paint film was spread, with the sensitive side of the paper up. The relative intensity indicated by the Artura scale degree of black, obtained when the paper was developed, compared to the relative intensity shown on the curve for the same size diaphragm opening, gives the percentage of ultra-violet light transmitted.

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[1] L. A. Jones, P. G. Nutting and C. E. K. Mees, *Photographic Journal*, December, 1914, p. 342.

REFLECTION FACTOR

With the paint film in place and pieces of Artura paper at the reflection positions *H*, light reflected from the paint film strikes the pieces of paper and exposes them. It was necessary to work out a factor to determine the intensity of reflected light at the positions *H* as compared to the intensity of the total reflected light leaving the paint film at the position *G*. To accomplish this, a 1000-watt nitrogen-filled tungsten lamp was used in place of the Cooper-Hewitt lamp, and a sheet of clear flint (lead) glass, $\frac{5}{8}$ in. thick, was used in place of the Corning filter. It has been shown that the light emitted by a nitrogen-filled tungsten lamp contains only about 0.3 per cent of ultra-violet light.[1] Using a nitrogen-filled tungsten lamp with the clear lead glass filter, it was possible to produce visible light, practically free from ultra-violet light. With the visible light illumination and with Artura paper placed on the under side of the clear quartz glass at *G*, but with no paint film on the glass, exposures were made, using standard time (two minutes) and various diaphragm openings. The exposed pieces of paper were developed and the Artura scale percentages of black were determined. A curve was plotted using percentage of Artura black and relative light intensity in the same way as with the ultra-violet illumination. This curve is shown at *B*, Fig. 2.

With the visible light illumination and a white lead paint film of sufficient thickness to prevent practically all transmission of visible light, placed at the position *G*, pieces of Artura paper were exposed at the positions *H*, using standard diaphragm opening and standard time of exposure (two minutes). The total percentage of visible light reflected by the paint film was determined by the Howland Color Photometer, checked by careful measurement by the Electrical Testing Laboratories. After the exposed pieces of paper were developed, they were measured with the Howland instrument and corrected to the Artura scale of black and white already described, in which unexposed and developed paper is considered as zero black and infinitely exposed and developed paper as 100 per cent black. The Artura scale percentage of black of the paper exposed to visible light at *H*, when referred to curve *B*, Fig. 2, gave the relative intensity of the reflected light reaching that point. The relative intensity of the incident light at *G* for the same diaphragm opening, as determined from curve *B*, Fig. 2, multiplied by the percentage reflection of the paint film, gave the relative intensity of reflected light at the position *G*; which, divided by the relative intensity of light at *H*, gave the factor which all relative light intensities, determined by Artura scale percentages of black resulting from exposures at *H*, should be multiplied by to determine relative reflection intensity at the position *G*. This factor is called *f*, and worked out in this way proved to be 20. It is assumed that ultra-violet light and visible light free from ultra-violet light, such as was used in determining *f*, are reflected in the same manner from any given surface, although they may not be reflected to the same degree. This seems to be a fair assumption, and is in accordance with a statement by Luckiesh[1] that ultra-violet light follows the inverse square law in the same way as visible light.

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[1] U. S. Bureau of Standards *Scientific Paper No. 378*, April 9, 1920, p. 245.

To determine percentage of reflection of ultra-violet light for any paint film, it is, therefore, only necessary to place the paint film at the position *G* and expose paper at positions *H* for standard length of time with standard diaphragm opening and standard ultra-violet illumination. After exposure the relative intensity, indicated by the Artura scale percentage of black shown by the paper exposed at *H*, multiplied by the factor *f*, gives the relative intensity of reflected light at the position *G*.

EXPERIMENTAL RESULTS

With the apparatus calibrated for transmission and with the reflection correction factor *f* worked out, different paint films were examined to determine their optical effect on ultra-violet light. Eight different paints, each made with a different pigment, were tested, the different pigments being the five white pigments, basic carbonate white lead, zinc oxide, basic lead sulfate, lithopone and titanox; two red pigments, red lead and iron oxide (Indian red); and one black pigment, lampblack. The paints were all made according to the same volume formula[2] as follows:

	PERCENTAGE, BY VOLUME
Pigment	26.00
Raw Linseed Oil	35.00
Petroleum Spirits	38.00
Liquid Drier	1.00

All of these paints dried with a flat finish which highly diffused the reflected light.

In making the examination each of the paints was spread on the clear quartz glass, using the same volume spreading rate for all of the paints, which gave the same volume of paint and the same volume of pigment on unit area of glass for each paint. A coat of each paint was applied to the quartz glass, the thickness being 0.04 mm. which was equivalent to 600 sq. cm. of surface to each cubic centimeter of pigment.

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[1] "Ultra-Violet Radiation," M. Luckiesh, 1922, p. 180.

[2] The paint made with some of the pigments in accordance with the formula was not of good brushing consistency because no attempt was made to examine paints of commercial brushing consistency, as the object in view was to make all of the paints in accordance with the same volume formula so as to eliminate the question of the effect of varying amounts of linseed oil. By the use of a special brush, the paints made according to the standard formula were applied as stated.

Using the maximum diaphragm opening and one minute time exposure for each determination, the results for the different paints were obtained as given in Table I.

The low light-reflection results were obtained with a ten-minute exposure instead of a one-minute exposure. Luckiesh states[1] that, in the exposure of photographic emulsions, the influence of intensity of illumination and time may be expressed as follows:

$$I\,t^p = \text{constant},$$

in which I is the intensity, t is the time and p is an exponent whose value lies between 0.7 and 1.0 for various emulsions. The effect on Artura paper was tested by exposing pieces of paper with different intensities of ultra-violet illumination and different times of exposure, but with the relation between them maintained as follows:

TABLE I.—ULTRA-VIOLET LIGHT OPTICAL RESULTS

KIND OF PAINT	REFLECTION, PER CENT	TRANSMISSION, PER CENT	ABSORPTION, PER CENT
Basic Carbonate White Lead	42	4	54
Zinc Oxide	0	0	100
Basic Lead Sulfate	3	0	97
Titanox	16	0	84
Lithopone	36	1	63
Red Lead	1	0	99
Indian Red (Iron Oxide)	1	0	99
Lampblack	2	0	98

$$I\,t = \text{constant}.$$

The photographic effect, as determined by the degree of black produced after developing, proved to be constant, which indicates that the exponent p is equal to 1.0 for Artura paper. This relationship was used in working with the increased time of exposure.

Only one thickness of paint film was used in the work, the thickness being selected to give some transmission of ultra-violet light with the more transparent paints. The transmission of ultra-violet light was found to be very small. The thickness of paint film used in these tests was less than the thickness of most average practical paint films, and still the paint films were of sufficient thickness to prevent the transmission of practically all the ultra-violet light.

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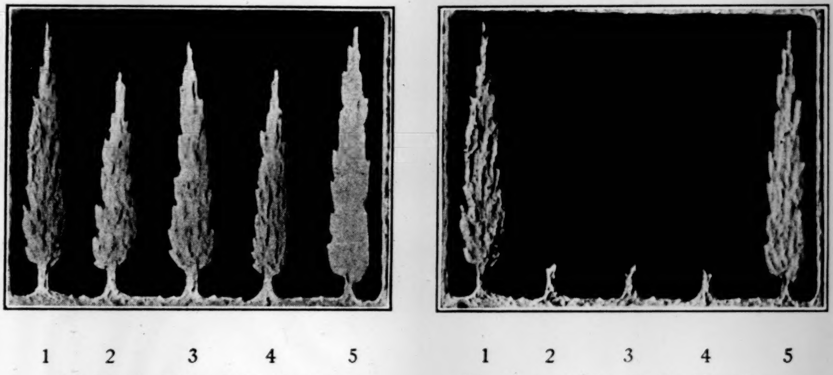
[1] *Loc. cit.*, p. 179.

In order to visualize the light reflection properties of the various white pigments, a picture was painted in the form of a landscape with five trees. The border, groundline and stumps of the trees were painted with white lead paint, the background was painted with lampblack paint, and each of the five trees was painted with a different kind of white paint, using the five different white pigments included in the experiments described in this paper. The picture was photographed with white light illumination and with ultra-violet light illumination; the white light photograph is shown in Fig. 4 (*a*) and the ultra-violet light photograph in Fig. 4 (*b*). The relative light reflection properties of paints made with the various white pigments are well illustrated by these photographs. It will be seen that these photographs are in agreement with the results of the experiments reported in this paper.



(*a*) White Light Illumination (*b*) Ultra-Violet Light Illumination

FIG. 4.—Photograph of Picture Painted with Five Kinds of White Paint Photographed under White Light and under Ultra-Violet Light

1. White Lead
2. Titanox
3. Zinc Oxide
4. Basic Lead Sulfate
5. Lithopone

DISCUSSION

This paper does not present any evidence in connection with the effect of ultra-violet light on the durability of paint films, but reports the results of a fundamental study of the optical effect of paint films of various kinds on ultra-violet light.

If ultra-violet light has any material influence on the durability of a paint film, it seems evident that only that portion of the ultra-violet light which is absorbed can have any influence, as the ultra-violet light which is reflected or transmitted passes out of and away from the paint film and can have no further effect on it. This is in accordance with previous statements of various investigators.[1]

From the results shown in Table I it is evident that nearly all paint films of thickness corresponding to practical painting work transmit a negligible proportion of the ultra-violet light which strikes them. The results also show that some paint films reflect a very large proportion of the ultra-violet light, and others a very small proportion, the result being that some paint films absorb nearly all of the ultra-violet light which strikes them and other paint films reflect a large part of the ultra-violet light, transmit a small amount, and absorb only the remainder.

When light passes into a substance and is absorbed, it may be converted into heat energy or at least partially so. It is the generally accepted theory that light consists of waves in the ether having certain frequencies or wave lengths corresponding to their respective positions in the spectrum. Perhaps the most reasonable theory which has been advanced to explain the absorption of light is that the natural frequency of the electrons in the absorbing medium corresponds to the frequency of the ether light waves.[2] It is known that light produces chemical effects on many substances, but apparently the chemical effect is not due entirely to the heating effect of the absorbed light, because an equivalent rise of temperature will not cause the same chemical reactions, and these reactions seem to depend more on the vibratory character of the light waves than on the amount of energy which they possess.[3]

Bunsen and Roscoe have found that in photochemical reactions the absorption of light by the reacting system is greater than the sum of the individual absorptions of the reacting substances.[4] The absorption of light by the reacting system, over and above the ordinary or optical absorption of the reacting substances, they call "photochemical extinction." While this phenomenon is regarded by many physical chemists as being of doubtful significance, nevertheless the distinction between purely optical absorption on the one hand and chemical absorption on the other is quite generally accepted. Bancroft[5] has held that all light which is absorbed by a substance tends to "eliminate" that substance, and that it is merely a question of chemistry whether any reaction occurs and what the products of the reaction will be.

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[1] "Outlines of Theoretical Chemistry," F. H. Getman, 1918, p. 515.

[2] "Light," R. C. Maclaurin, 1909, p. 54.

[3] "Textbook of Physics" edited by A. W. Duff, Chapter on "Light" by E. P. Lewis, 1920, p. 648.

[4] Getman, *loc. cit.*, p. 515.

[5] Getman, *loc. cit.*, p. 513.

Perhaps the most important chemical reaction in which light plays an important part and one which has been conclusively demonstrated is the action which light exerts in the growth of plant life.[1] In this reaction atmospheric carbon dioxide is taken up by plants under the influence of solar radiation, and while the reaction is exceedingly complex, it is well understood that starch and oxygen result from the reaction of carbon dioxide and water under the influence of light. This reaction is of particular interest in the study of paint films, because it is obviously not an oxidation reaction and is, if anything, a reducing reaction. The reduction of silver compounds[2] and many other reactions might be cited to show the chemical effect of light.

It would be unsafe to draw any definite conclusions from the experimental work reported in this paper, even in connection with the work done by other investigators as referred to. The extent to which the absorption of ultra-violet light affects the durability of paint films and tends to destroy them remains to be determined, but if such absorption has any destructive effect it is only one of the many factors influencing paint decay. For this reason too much reliance should not be placed on any assumption that ultra-violet light is a determining factor in the destruction of paint films.

I wish to express my appreciation of the advice given me by Mr. G. W. Thompson in connection with this work, and the assistance of Mr. M. R. Paul and Mr. W. C. Weber in conducting it.

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[1] Getman, *loc. cit.*, p. 524; Lewis, *loc. cit.*, p. 648; "Physics, Experimental and Applied," Translation from Ganot's "Elements de Physique" by E. Atkinson, 1910, p. 647.

[2] Lewis, *loc. cit.*, p. 648; Getman, *loc. cit.*, pp. 521–527; Ganot, *loc. cit.*, p. 646.

DISCUSSION

Mr. Davis

Mr. J. B. Davis.[1]—Our company has used sources of ultra-violet energy as a means of accelerated aging test for some three or four years, possibly without the knowledge as to the theory involved, but knowing that it is a dependable and accurate means of foretelling the life of any paint vehicle in combination with a pigment. We have found through the use of the Cooper-Hewitt arc lamp, properly set up and ventilated to avoid the effect of heat on the deterioration of the sample film and by keeping away from the bulb itself currents of air which are liable to cause the presence of ozone, which in itself is a deteriorating agent, that we can foretell the life of a zinc-oxide film, a lithopone film or a titanox film, in the ratio of one hour to six weeks. By that means we are able to tell beforehand whether a film is going to stand up under actual usage six, twelve or eighteen months.

In order to control the two factors of heat and ozone, we have attached a ventilator at the top of the Cooper-Hewitt lamp by which a draft of air is drawn continuously across the face of the sample. The cabinet is so arranged that this draft of air is kept entirely away from the quartz bulb, and in that way the formation of ozone is avoided.

I think Mr. Hallett has done a remarkable piece of work in measuring the absorption of ultra-violet light by various pigments. We can substantiate his statements that the absorption of ultra-violet light through the pigment used in the paint films is directly in proportion to the ultimate expected life.

A peculiar feature we have noted is that when films of zinc oxide are exposed under ultra-violet light there is a marked bleaching, whereas when lithopone films are exposed in the same manner in the same vehicle, under the same humidity and other conditions, there is a marked yellowing. That in itself seems to indicate some chemical change due to the absorption of ultra-violet light.

Mr. Busby

Mr. H. S. Busby.[2]—I think Mr. Hallett said that ultra-violet radiation may be a part of the problem but the whole question of exposure to weather is certainly the whole of the problem, and that he is not able to apply weights to those two factors and to tell what

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[1] Technical Engineer, Standard Textile Products Co., New York City.

[2] Consulting Colorist, New York City.

Mr. Busby proportions are due to the ultra-violet and what of the remainder is due to the rest of the weather condition. In that connection Mr. Davis' statement might be modified somewhat and certainly precautionary ideas be injected. Thus, we have found in dyes and paints which were exposed to different regions of the spectrum that whereas a more rapid deterioration due to the ultra-violet spectrum may occur, there is not a proportionate selective determination. What is true of colors must in a certain sense be true of any substance of similar nature, even though the distribution of that spectrum was not the same.

Mr. Breyer MR. F. G. BREYER.[1]—I think this matter is too important to leave in the state of having one paper say one thing and another paper say just the opposite, which is about what Mr. Pfund's[2] and Mr. Hallett's papers have amounted to. It seems to me that there are a few facts that are absolutely established with respect to light, and one of them is that the south side of the house which is exposed to the sunlight deteriorates in anywhere from one to two years earlier than the other side of the house. Secondly, that wherever pigments of known ultra-violet absorptive capacity, such as blacks, are added, the life of the paint is increased. Any attempt to minimize the effect of ultra-violet light will be of no avail because we have got to come back to an explanation of that very thing. The next thing is, those pigments which transmit ultra-violet light or transmit that portion of the ultra-violet light which is present in the solar spectrum are all chalking pigments and those which do not are non-chalking pigments. We mix paint by adding a material which is opaque to a material which is not opaque in order to retard chalking. We add zinc oxide to carbonate of lead to stop it from chalking. That has nothing to do with whether it is going to crack or peel. It is purely and simply from the standpoint of chalking, which is caused primarily by the destruction of the linseed oil binder which alone, without a protective pigment of any sort, is broken down in a very short time by ultra-violet light or ordinary sunlight. We put the pigments in to protect the oil.

Mr. Busby MR. BUSBY.—Mr. Breyer has neglected one point. We are not trying to minimize the effect of ultra-violet light; we are simply trying to put it in the proper place. The question that I raised is not so much a question of the total effect of ultra-violet light but the proportional effect of ultra-violet light in the whole matter. It

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[1] Chief, Research Division, New Jersey Zinc Co., Palmerton, Pa.

[2] A. H. Pfund, "Some Optical Properties of White Paint Pigments in the Ultra-Violet Spectrum." See p. 369.—ED.

Mr. Busby is true, as Mr. Breyer says, that one side of the house chalks and the other side does not, but has he any data to show that if it were not a question of a whole spectrum or part of a spectrum, or a proportional part of a spectrum, what the effect would be on two sides of the house? There are no data published along that line. We know that in other forms of coloring material, which are also organic substances, the different ranges of the spectrum have entirely different effects, and I do not see why the whole problem is assigned as being an effect caused purely by ultra-violet light. Neither can I see why chalking is the entire issue. I do not think either of these two papers mentioned chalking primarily.

Mr. Calbeck MR. J. H. CALBECK.[1]—There is one question that I should like to bring up. We have listened to two papers, the observations in both of which check each other very closely. The difference seems to be in the interpretation of those results. According to one paper the ultra-violet light being reflected from the paint film is removed completely from the film and is, therefore, of no more effect. In the other paper the exact opposite is held, that the ultra-violet light being absorbed by the pigment prevents the destruction of the paint film. Now it seems to me that the answer depends on whether ultra-violet light in passing through a relatively transparent linseed-oil film causes its destruction by the mere transmission through that film, or whether it must be absorbed by the film. We have evidences before this Society of the destructive effect of ultra-violet light on paint films extending back some years. Observations have been made in the dye, textile and paint industries and all have agreed that there is a destructive effect; but does any one know whether destruction is caused by transmission through the material or whether it first must be absorbed by the material before the chemical reaction, or whatever it is, may take place? A biologist will readily answer that and say that it must be absorbed first before any photo-chemical action can take place. I wonder if there have been any observations along that line made with reference to linseed-oil and paint films.

Mr. Pfund MR. A. H. PFUND.[2]—Deterioration of a paint film under the influence of ultra-violet light is occasioned by those radiations which are absorbed by the oil. It is well known that linseed oil is opaque to radiations of wave length less than 3000 Å. U. The Corning glass filter used by Mr. Hallett in conjunction with a mercury arc isolates the mercury line of wave length 3640 Å. U.; therefore all of his results apply to a region of the spectrum in which linseed oil is essentially

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[1] Director of Research, Eagle-Picher Lead Co., Joplin, Mo.

[2] Associate Professor of Physics, Johns Hopkins University, Baltimore, Md.

Mr. Pfund

transparent and not measurably decomposed. Had his experiments been carried out in the region 2900–3100 Å. U. the results would have had a bearing on the subject of deterioration.

Since a white paint pigment is not decomposed by ultra-violet light, I can only repeat my contention that a pigment such as zinc oxide, which is even more absorbent of the harmful rays than the linseed oil itself, will protect the oil film while a pigment such as white lead, which is transparent to these rays, forces the oil to do all of the absorbing. Actual experiments by Mr. H. A. Nelson on the deterioration of paint films under the influence of ultra-violet light are quite in accord with the above argument.

Concerning the virtue of a high reflection coefficient, it may be stated that, if the paint film were covered with a highly reflecting metallic film, a high degree of protection would be realized. In the case of paints, the reflection is due to a large number of pigment particles lying well down within the mass of the film. Even though as large a fraction as 50 per cent of the incident ultra-violet light be reflected, the 50 per cent absorbed is absorbed by the linseed oil alone and the surface layer of oil receives, so to speak, a "double dose" since it is traversed by the incident as well as the reflected light. The contention is, therefore, that a highly reflecting pigment affords less protection to a paint film than does a highly opaque pigment.

Mr. Hallett

MR. R. L. HALLETT (*closure by letter*).—Mr. Breyer's statement that the paint on the south side of a building usually deteriorates more rapidly than the paint on the north side is probably true, but is unimportant in connection with the effect of ultra-violet light, as the south side of a building is subjected to so many conditions which are different from the north side. Perhaps the sun's heat has the greatest influence in bringing about different painting results on different sides of a building.

In discussing the durability of paint films, we are not discussing any one defect, and we must consider a number of defects which, separately or collectively, affect the durability of paint films.

Scaling is perhaps the most objectionable defect which is ever encountered, and is caused by the paint film losing its elasticity and becoming hard and brittle as it ages. The hardening of paint films is probably due to chemical reactions between the pigments and vehicles which result in the formation of hard and brittle compounds.

We have comparatively little evidence to show what takes place when ultra-violet light is absorbed by a paint film, but it seems probable that the ultra-violet light which is absorbed is converted into other forms of energy which may tend to accelerate any chemical reactions which might naturally take place within the paint films.

Mr. Hallett While it is possible that some of the absorbed ultra-violet light may tend to increase chalking, it seems more probable that the absorbed ultra-violet light may tend to accelerate the chemical reactions which cause scaling. For this reason it seems logical to suppose that the tendency of some paint films to develop scaling may to some extent be due to the fact that they absorb all or nearly all of the ultra-violet light which strikes them, and the tendency of other paint films to remain relatively elastic may be partly due to the fact that they reflect such a large proportion of the ultra-violet light.

Mr. Pfund made the statement that the results obtained in our work refer to the region of the spectrum in which linseed oil is essentially transparent, but our work has proved the contrary to be the case, as, working with linseed oil films alone, we found them to be fairly opaque in our apparatus, working with the portion of the spectrum described in the paper.

Furthermore, we would say that our work and the results reported are almost entirely optical, and there is no reason to believe that the light waves of shorter length than those included in the region of the spectrum which we used would be differently affected by the paint films.

While it is true that the region of the spectrum which we used showed a maximum at about 3640 Å. U., it included some wave lengths as short as 3280 Å. U. As sunlight terminates at about 3000 Å. U., it will readily be seen that the region of the spectrum which we used contained most of the short light waves which are present in sunlight.

Mr. Pfund contends that the large amount of ultra-violet light absorbed by some paint films is less harmful than the smaller amount of ultra-violet light absorbed by others, but we cannot see the logic of such a line of reasoning. We know that some paint films absorb nearly all of the ultra-violet light and other paint films only part of it, and it is evident that the ultra-violet light which has been reflected out of and away from a paint film can have no further effect on it. It seems obvious that as only the absorbed ultra-violet light can have any effect on a paint film, a paint film which absorbs all or nearly all of the ultra-violet light would be injured to a much greater extent than a paint film which absorbs only part of the ultra-violet light and reflects most of the remainder. Even though some of the ultra-violet light which is reflected away from a paint film may have perhaps penetrated to a certain distance within the film before being reflected, it, nevertheless, seems to be evident that, whether the light rays have passed through portions of the paint films once or twice, when finally reflected, they certainly can have no further effect on the paint.

Mr. Hallett

While it seems probable that if ultra-violet light has any harmful effect on paint films and is an influence in their destruction, the harmful effect must be in proportion to the ultra-violet light absorbed, we would, nevertheless, like to reiterate our common sense caution that ultra-violet light probably does not have an appreciable effect in determining the life of paint films and should not be given more consideration than it deserves.

COLOR

By G. W. Thompson[1]

Color and Form are the two attributes of the external world which become manifest to us through the optical sense.

As defined by the American Society for Testing Materials, color is:

> "A generic term referring inclusively to all of the colors of the spectrum, white and black, and all tints, shades and hues which may be produced by their admixture."
>
> "Color involves a definite effect produced by the action of light upon the retina of the eye dependent upon the optical composition of the light. This term is also used in reference to material substances such as pigments, stains, dyes, etc., but in specifications it should be recognized that color is primarily a physiological sensation."

As defined by the committee reporting to the Optical Society of America, color is:

> "The general name for all sensations arising from the activity of the retina of the eye and its attached nervous mechanisms, this activity being, in nearly every case in the normal individual, a specific response to radiant energy of certain wave-lengths and intensities."

These two definitions approach the subject of color from somewhat different standpoints and yet are in substantial agreement. While the A.S.T.M. definition, particularly the note that follows it, recognizes that color is primarily a physiological sensation, the definition itself is intended to refer to the colors which objects may have; that is to say, the colors which these objects, acting as stimuli, produce in the way of sensation.

We say that two objects have different colors. This simply means that the eye is stimulated by these two objects differently and to an extent sufficient for the observer to say that the colors are distinguishable one from the other. Strictly speaking, two colors are identical when they are not thus distinguishable one from the other.

In order to approach our subject with discretion, we must carefully scrutinize our understanding of what takes place when color is perceived. Let us assume that a ray of light falls upon a certain object. A portion of this light is reflected to the eye of the observer and the observer says that the object is red. Suppose, on the other hand, you have a different object and a portion of the light falling upon it is reflected to the eye of an observer and he says that the object is white. If the light falling upon these objects in both cases has been noon-day sunlight, and the eye of the observer is an average eye, then we say that the sensations produced are normal sensations and that the two objects have the colors red and white, respectively. You will note that we start out with an assumption: namely, that sunlight is a normal source of illumination. The object which has a white color has that color because it was illuminated we say by white light. There seems to be good reason to believe that there is something real in white light—real, in the sense that it is independent of the fact that the light from the sun is white. The theories of color sensation that have a bearing on this are given below, and we will also quote the following from Parson's "An Introduction to the Study of Colour Vision:"

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[1] Chief Chemist, National Lead Co., Brooklyn, N. Y.

> "It is commonly said that the white light is 'split up' into its component parts, which are colored. Lord Rayleigh has given sound reasons for the view that white light is not thus analyzed into component parts, but that the periodicities characteristic of the several rays are in reality imposed by the prism and are not antecedently present in the white light."

It may be, however, that the relation that exists between the sensation of white when stimulated by the light of the sun falling upon a white object is the result of many centuries of evolution. Whatever our belief on this subject may be, however, we accept the statement that the light from the sun is white and that objects have a white color whenever they produce the same sensation that the direct rays of the sun produce upon the eye, when properly diffused. We make the assumption, therefore, that the object which has a white color reflects all of the rays of the sun that are visible, in equal proportions. In the case of the object that has a red color, we would say that some of the rays from the sun have been absorbed and only those, as far as they are visible, that produce the sensation of the particular red seen are reflected. In the study of colored objects, therefore, we have in the first instance the incident light, in the second instance the reflected light, and in the third instance the sensation experienced.

This statement of what takes place when we perceive color leads us to this distinction: we may consider color either as a stimulus or as a sensation. To study color as a stimulus, we make use of the spectroscope in some form or other. We say that light passing through a prism is dispersed and we conclude that white light contains a great many constituents. White light passed through a prism gives us a spectrum, a portion of which only is visible to the eye;

therefore, we say that white light contains visible rays and invisible rays, the visible rays being infinite in number and differing from each other in wave length. We look at the spectrum and we try to count out the number of different color sensations that the spectrum produces and we find ourselves confronted with a serious physiological difficulty. Suppose we have a spectrum stretched out to great length and, as it were, we proceed to step along the spectrum from one end to the other and our steps are measured in length by just discernible differences. On each step we cross a threshold. This word "threshold" means the difference in stimulus which is large enough to produce a just noticeable difference in sensation. In this particular case it is known as "differential threshold" and we find that due to this differential threshold the greatest number of steps that we can take from one end of the visible spectrum to the other is 128. From this we judge that there are 128 spectral colors, although as a matter of fact the wave lengths vary imperceptibly and present a continuous series from one end of the visible spectrum to the other. (At this point it might be well to explain that a certain minimum intensity of light stimulus is necessary to produce any color sensation. This minimum intensity of stimulus is called "the liminal threshold.")

Let us take another case: suppose we have a surface that has a white color illuminated with two lights, one of which has a slightly greater intensity than the other. Obviously, we have two stimuli differing slightly in intensity. Now it is found that here, too, the difference in intensity of stimuli must amount to a certain minimum in order that the eye may be able to perceive the difference. This is due to the differential threshold which must be stepped over.

Returning to the study of the spectrum we observe that the wave lengths capable of producing the sensation of color, range from 400 to 760 millimicrons, constituting a continuous series. Every wave length differs from its neighbors in the series infinitesimally. The expression "single wave length" has, strictly speaking, no meaning. Conventionally, it means a section of the series that is just distinguishable in terms of sensation from adjacent sections. The number of just distinguishable single wave lengths of the series, as indicated above, is about 128. These single wave lengths are the constituents of all color considered as a stimulus, and every colored object can be examined spectro-photometrically and the relative intensities of each of these stimulus constituents determined.

As we look at the spectrum and see the constituents of color and note the resemblance between spectral colors and objects similarly colored, we are apt to conclude that in some cases these objects reflect only one constituent, say blue or red. As a matter of fact it is probable that if the incidental light is white no object ever reflects only one constituent of stimulus color. In most cases, it reflects all of the constituents of stimulus color in which, however, those constituents which are similar in the sensation they produce to the sensation produced by the object are in excess.

You will note that we have used the expression "stimulus color." We will also use the expression "sensation color." We use these expressions simply for clearness, realizing their crudity.

Sensation color is the combined or resulting effect of all of the constituents of the stimulus. We may consider that sensation color has constituents; usually they are thought of as three in number, namely, the three physiological primary red, green and blue, which can be combined to produce practically every sensation color.

It is interesting to note that the spectrophotometric analysis of color relates to the stimulus, while tri-chromatic color analysis relates to the sensation. In order to get the most recent thought on how stimulus color produces sensation, we are inserting here an excerpt from Watson's "Psychology from the Standpoint of a Behaviorist:"

"*Color Theories.*—Several general theories of vision have been advanced which have for their purpose the presentation of a possible physiological scheme of the kinds of processes that go on in the eye when light falls upon it. All of the theories assume the presence of some kind of substance that is sensitive to light. One of the theories, that of Mrs. Christine Ladd Franklin, has several good points. In order to develop this theory, we must mention the fact that only rods are found on the periphery of the retina and that in the periphery all monochromatic lights are reacted to as to white light. In the middle regions of the retina there are both rods and cones. This region is sensitive only to yellow and blue rays and to white light. Finally, in the central regions we have sensitivity to all wave lengths and to non-homogeneous light as well. In order to account for these functional peculiarities, Mrs. Franklin assumes (1) a primitive photochemical molecule which breaks down under any form of light stimulation. This is found only in the rods. (2) A partially differentiated molecule which is sensitive to yellow and blue rays. This molecule is found in the relatively poorly developed cones of the middle zone of the retina. (3) A triply differentiated molecule which breaks down when any one of three monochromatic lights—red rays, green rays and blue rays—impinge upon it. This molecule is found only in the cones in the central regions.[1]

"Hering assumes three pairs of photochemical substances: one pair sensitive to red and green rays, one pair sensitive to yellow and blue rays and one pair sensitive to non-homogeneous rays. Two antagonistic and simultaneous processes, one katabolic, the other anabolic, are constantly taking place in these three pairs of substances. When the red rays strike the eye, the katabolic phase prevails in the red-green substance, and the subject reacts to the red rays. When the green rays strike the eye, the anabolic phase prevails, and the subject reacts to the green rays. Similarly, when the yellow rays strike the eye, the katabolic phase prevails and the subject reacts to the yellow rays; when the blue rays strike the eye, the anabolic phase prevails and the subject reacts to the blue rays. Non-homogeneous light as well as monochromatic light at all intensities induce the katabolic phase in the white-black substance. These photochemical substances in all color theories are unstable. For example, on the Hering assumption, if the katabolic phase is induced by an appropriate ray, say, 558 millimicrons (yellow) striking the eye for a short time, the subject reacts to the katabolic phase as long as the ray impinges upon the eye. As soon as the ray is removed, the anabolic phase begins (there is a constant tendency to establish equilibrium in the photochemical substances) and the subject reacts as though a ray from the blue region of the spectrum actually impinged upon the eye. This accounts for the *origin of the stimulus* in the negative after-image. We saw on page 93 that when certain rays of light are superimposed upon a single surface, the subject reacts not to monochromatic light but to white light. Hering assumes that two rays, the one from the yellow region, the other from the blue region, leave the yellow-blue substance in equilibrium, that is, they balance or cancel each other. But both of these monochromatic lights affect the white-black substance, hence the subject has every reason for reacting to white light, since the appropriate physiological stimulus is at hand." [1]

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[1] Mrs. Franklin developed this theory very early in 1892. Schenck in 1906 advanced almost exactly the same view but with certain modifications. Luciani in his Physiology gives Schenck the credit for advancing the theory. The germ of the theory is, of course, much older.

"Most of the phenomena described on page 93 can be explained by one theory as well as another. The Hering theory is criticised for assuming that red rays and green rays cancel each other.[2] The balancing factors for the red rays are found not in the green, but in the blue-green region of the spectrum. It is criticised also for its inability to explain certain types of red-green blindness. According to some investigators at least, the ability to react to green rays is sometimes present in cases where the subject cannot react (on the basis of wave length) to red rays. There are possibly many differences in responses between the deuteranope and the protanope. If sensitivity to red and green rays depends upon reverse effects in a single photochemical substance, loss of response to one should entail loss of response to the other."

What I have said so far indicates that, strictly speaking, color is simply an optical sensation, and that any specific color is any distinct optical sensation, produced in a normal way, by some external stimulus. By the words "distinct optical sensation" we mean an optical sensation that is distinguishable from other similar sensations.

The sensation of color stimulated by a non-luminous object depends upon its illumination and the reflecting properties of the object. It does not appear probable that we shall ever find a single non-luminous object that will reflect only homogeneous light: that is, light of a single wave length. Generally speaking, all objects reflect light that contains all the rays that stimulate color sensation. These rays are the constituents of stimulus color. They are about 128 in number, and each can be considered as of a single wave length. If every colored object reflects all of these constituents of stimulus color, the difference in sensation produced must be due to variation in the intensity of these constituents, and to get a difference in sensation between two colors the difference in the intensity of the stimulus constituents must be sufficient to produce a difference in sensation.

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[1] Attention is called to the fact that the sudden withdrawal of all light from an object serves as a positive stimulus "black." The easiest way to offer this stimulus is to have the subject proceed suddenly from a sunny room to a good dark room.

[2] As a matter of fact when red rays of a certain length and green rays of a certain length are superimposed, the subject reacts as though stimulated by a single monochromatic band of yellow rays.

COLORIMETRY REQUISITES

Although color is, primarily, a sensation, we are mostly interested in the study of objects that differ in the color sensation they produce. All colorimetry has to do with the reflecting power of objects. To this extent colorimetry is a study of stimulus color.

It is, of course, requisite that the student of color should have average or normal color vision. If he has not normal vision the results of his study are almost useless if an attempt is made to compare his results with those obtained by another. It is true that the spectrophotometer does not require normal vision, but in most other work normal vision is necessary.

We have already pointed out that if an object when uniformly illuminated produces a uniform sensation, a difference in sensation can be produced by illuminating one portion of the object more or less than the other portion. It is important, therefore, in colorimetry, that colored objects when studied comparatively should have a standard or normal source of illumination.

It is also obvious that all influences that interfere with normal illumination and normal vision should be eliminated as far as possible.

There is this also to be considered: The human eye most accurately differentiates between colors when the intensity of the colors is neither too great nor too low. This indicates that medium illumination is preferable in color comparisons.

HOW COLORS MAY BE CLASSIFIED

The classification that we have in mind here is simple in character, and yet quite fundamental. It is obvious that some colors possess and others do not possess hue. The committee of the American Optical Society calls colors that possess hue "chromatic colors" and those that do not possess hue "achromatic colors." These descriptive terms are probably as acceptable as any that can be devised. By this classification, all true, or neutral, whites, grays and blacks are achromatic because they are without hue. If we examine spectrophotometrically the light reflected from any object that appears white or gray we find that the constituents of the stimulus color in each case are in the same proportional intensity. Thus, if a gray having a stimulus intensity of 20 is examined in this way each of its constituents will have an intensity of 20, the comparison of course being made with white light having an intensity of 100.

It would appear when the sensation white or gray is produced that the constituents of the stimulus balance each other in some way so as to produce the single sensation of white or gray. The constituents seem to neutralize each other, and we often speak of whites and grays as being neutral. How this neutralization takes place we do not know. The more general belief is that the neutralization takes place in the eye, but strong arguments have been advanced by Lord Rayleigh which would indicate that the neutralization is not in the eye but in the stimulus. White light is often spoken of as heterogeneous in contradiction to homogeneous light, supposed to be that containing a single wave length. We do not propose to question the propriety of this custom. We would simply point out that whether white light is heterogeneous or not the sensation it produces is homogeneous.

In a case of chromatic color, that is, color possessing hue, we may have a single wave length as may be obtained from the spectrum. As already indicated, however, colored objects, generally speaking, do not reflect a single wave length—they reflect all wave lengths with varying intensity, so that what goes to the eye is really partially balanced and partially unbalanced stimuli. To the extent that a stimulus is unbalanced the sensation of hue results. Colored objects, therefore, produce a composite sensation representing the effect of balanced and unbalanced stimulation. The unbalanced stimulation results in some way in the sensation of a single hue. This will be explained more fully further on, but for the purpose of making what we have in mind clear, I will quote here a sentence from Luckiesch:

> "In other words, all colors, excepting purples, can be matched by diluting spectral light of a definite wave length with white light."

We may have occasion to refer to the exception Luckiesch makes of purples later on. This is in accordance with the Howland, Munsell and Ostwald systems which make all color sensations to be composed of a single hue and white light.

There is one point here which I am sure is apt to produce confusion unless it is cleared up. We have seen that when the constituents of stimulus color are in balance the sensation of white is produced; also, that to the extent that these constituents are in balance, the sensation produced has as one of its components, white light. How is it, then, that the unbalanced constituents, or portions of constituents, produce the sensation of a single hue? The answer is this: that just as balanced constituents, becoming combined, produce the sensation of white light, so the unbalanced constituents or portions thereof, becoming combined, produce the sensation of a single hue. We will attempt to explain somewhat, later, how this takes place.

HOW ACHROMATIC COLORS ARE RELATED

Noon-day sunlight is considered the best standard for white light. As, however, such a source of illumination cannot readily be made use of, attempts have been made to produce artificially a light that approximates sunlight in whiteness. Assuming that this has been reasonably well accomplished and that we have an accepted standard for artificially produced white light, the next question that confronts us is: What can we adopt as a standard material having the highest reflecting power for all of the constituents of white light? It is generally accepted that chemically pure magnesium carbonate most nearly meets this need. In most colorimetric work what is wanted is diffused reflection which is obtained when magnesium carbonate is in the form of a block, the surface of which is cut smooth. The standard reflecting power for white light of magnesium carbonate can be taken as 100, although its true reflecting power is nearer 98.

Achromatic colors include all neutral whites, grays and blacks. The differences that exist between them are differences of intensity or degree only; whites having relatively high intensity, grays medium intensity and blacks low intensity. They may all be considered as whites having intensities running from 100 down to zero.

For the purpose of studying achromatic colors of varying intensity, the use of the Howland machine is most simple and serviceable. Suppose we start with a white disk, and consider it to have a reflecting power of 100. Then, by means of sectors of various areas revolving against the black background we can get any intensity of whiteness we desire. Thus, if we have two sectors each of 90 deg., together they will reflect 50 per cent of the light reflected by the central disk. By a system of descending sectors, one can have any series of whites of varying intensity that may be desired. The rotation of such a series of descending sectors varying in 10-per-cent differences between each gives a series of intensities of 100, 90, 80, 70, etc.

The striking feature of this series of whites is that there seems to be a greater difference between the whites of low intensity than exists between the whites of high intensity. This indicates that the sensations produced by whites of varying intensity are not arithmetically proportional. If, however, we construct our series so that they bear a logarithmic relation to each other we find that the sensation differences produced are apparently equal, step by step, as may be illustrated by spinning such a series of sectors. Priest has called this series the "natural series."

The significance of the distinction is here noted between stimulus colors and sensation colors: An arithmetical series of stimulus colors does not produce equi-distant steps in sensation; a logarithmic series of stimulus colors produces equi-distant steps in sensation. Therefore, to produce equi-distant steps in sensation we must vary our stimulus logarithmically. A series of figures in which sensation varies in 10-unit steps from 100 down, and showing the relative stimulus variations which are necessary to produce these steps is given in Table I under the heading Natural (Logarithmic).

There is a profound physiological, or, perhaps, psychological significance in the logarithmic relation of sensation to stimulus. Suppose we have a series of logarithmic steps, which we call A, B, C, D, E, etc. In such a series it is found that A:B = B:C = C:D, etc. The rule seems to be pretty general that if we have three sensations, of whatever kind, and attempt to compare them, they bear this logarithmic relation to each other. Thus in the case of music we step from one C to the next C higher, and from that C to the next C higher. If the rate of vibration of the lowest C is 1, the rate of vibration of the next C above it is 2, and the next C above it is 4, and so on. In other words, the stimulus necessary to produce equi-distant steps in sensation must vary logarithmically. It is probable, however, that this general law does not apply when the stimulus is either too intense or when it is too low.

TABLE I.—MUNSELL AND NATURAL SCALES FOR EQUAL STEPS IN SENSATION COLOR

MUNSELL		NATURAL (LOGARITHMIC)	
10	*100*	100	
9	*81*	90	*63*
8	*64*	80	*39.8*
7	*49*	70	*25*
6	*36*	60	*15.8*
5	*25*	50	*10*
4	*16*	40	*6.3*
3	*9*	30	*3.9*
2	*4*	20	*2.5*
1	*1*	10	*1.58*
0		0	

In the Munsell system a different series of steps is used. In this system, assuming that ten steps are desired, that is, from 10 to 1, the intensity of the stimulus is obtained by squaring each of the sensation steps so that we get the Munsell series given in Table I.

We often speak of a medium or "median" gray, which is presumed to be located somewhere half-way between a black and a white. According to our logarithmic series there is, strictly speaking, no such medium gray because we never reach a zero stimulus in such a series. Practically speaking, in such a series, however, when we get the intensity low enough the gray produced is indistinguishable from a perfect black; that is, we cross the liminal threshold. To get a medium gray we must assume for practical purposes a certain minimum intensity of stimulus below which all grays are black. If we take this limit as 5 in each logarithmic series we find that our medium gray is somewhere around 20 to 25. According to the Munsell system, they place it at 25, which is medium if we eliminate the 100 from our series. Therefore, a medium gray has an intensity of whiteness of about 25 per cent. We understand that Munsell arrived at his series by having a large number of grays ranging from a pure white to a black, and had unbiased observers pick out the eleven steps which they thought were equi-distant as to the sensations they produced.

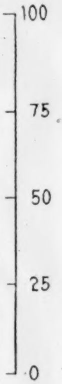

FIG. 1.—Representation of Stimulus Intensity

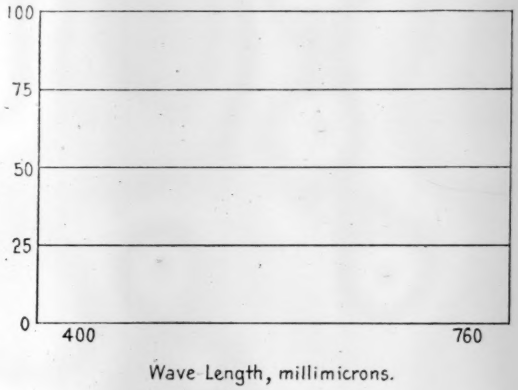

FIG. 2.—Form for Recording Spectrophotometric Analysis

The graphic representation of achromatic colors is shown in Figs. 1 to 3, inclusive.

Fig. 1 gives a simple representation of stimulus intensity.

Fig. 2 shows a form on which to represent spectrophotometric analysis. If pure reflected white light having an intensity of 75 is analysed it will be found that each constituent has approximately an intensity of 75 compared with the intensity of the constituents of reflected white light having an intensity of 100. This rectangular representation is obtained by taking the vertical scale (Fig. 1) and moving it sidewise to cover an area sufficient to include as many groupings of adjacent single wave lengths as desired.

In Table I are shown scales giving equal steps in sensation and the corresponding stimulus (in italics) according to the Munsell and natural scales.

Fig. 3 gives an indication of the graphical representation of trichromatic analysis of achromatic color. At the point 80 the blue, the green and the red would each have an intensity of 80, indicating a whiteness of 80 in the sample tested.

CHROMATIC COLORS AND HUES

We come now to the consideration of chromatic colors. According to our definition, chromatic colors possess hue. Therefore, to understand what chromatic colors are, we must know what hue is.

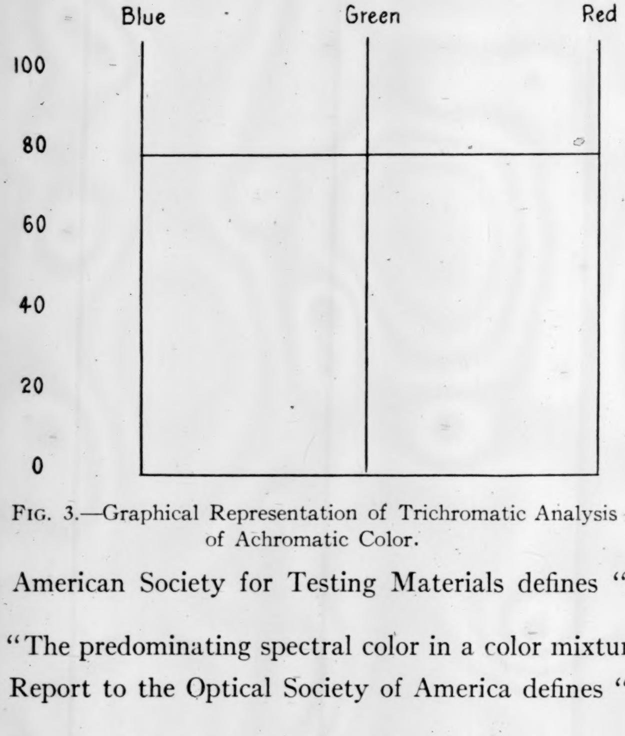

FIG. 3.—Graphical Representation of Trichromatic Analysis of Achromatic Color.

The American Society for Testing Materials defines "hue" as follows:

> "The predominating spectral color in a color mixture."

The Report to the Optical Society of America defines "hue" as follows:

> "Hue is that attribute of certain colors in respect of which they differ characteristically from the gray of the same brilliance and which permits them to be classed as reddish, yellowish, greenish or bluish."

Neither of these definitions seems entirely satisfactory. The A.S.T.M. definition was written with the idea that spectral colors are rather limited in number and that between these spectral colors there are other colors which differ from the spectral colors in tone. Thus we would have a red with an orange tone, a greenish yellow, a bluish green, etc. For practical purposes this definition has served a good purpose.

The Report to the Optical Society of America's definition makes no reference to spectral colors or color constituents, and is in terms of sensation only. This is all right as far as it goes. If, however, we are correct in holding that for a proper understanding of color it must be studied both as a stimulus and as a sensation, a definition of hue to be complete should include both of these aspects. We have already indicated that a stimulus color produces the sensation of hue whenever the constituents of the stimulus color are sufficiently unbalanced, that is, unbalanced beyond the differential threshold. In a properly produced spectrum each color seen is completely unbalanced, that is, it contains balanced constituents only to a negligible extent. Spectral colors, therefore, possess hue to a marked degree. In them the property of hue is intense.

At this point we must pause and review some of our knowledge of this phase of the subject to be sure that we will not argue in a circle. The visible rays of light begin to be produced whenever an object is heated to about 900° F. The visible rays produced at this temperature are red—they are the longest visible rays. As the temperature increases shorter rays are produced, until, when the temperature is high enough, all of the rays of the visible spectrum are produced. This takes place at what is called "white heat." Apparently, during this advance in temperature the intensity of each of the rays is increased and at white heat the intensity of each of the rays is fairly close to the proportionate intensity of the rays from the sun. We see this in the ordinary incandescent lamp. If we put resistance into a circuit and then gradually diminish the resistance we first get the red rays, then the red and yellow rays, and so on up until we get white light. Apparently there is no further effect upon the visible rays by increasing the temperature, except to increase the intensity of each of the rays. Of course the character of the object heated affects to some extent the intensity of the constituents of the white light produced at high temperatures, and it is usual in attempting to imitate sunlight to introduce certain color filters which make the transmitted light as close as is possible to sunlight. The first point that we wish to make is this: that, except for its minor differences, the balancing of the constituents of color so that white light is produced depends upon the temperature to which the object is subjected.

We say that the constituents of color are balanced when white light is produced, but we have no means of knowing whether they are balanced or not except by the sensation that is produced. Therefore, it seems somewhat like arguing in a circle to say that hues are perceived when the constituents of the stimulus are unbalanced. We get, however, a picture of the constitution of color, but we must guard against any assumption that conditions of balance or unbalance in the present state of our knowledge rests on any basis other than sensation. The fact that heat produces light, the constitution of which is a function of temperature, gives us just a little correlative support to the dictum that we are laying down.

Hues are distinguished in two ways: first, as to quality or character; second, as to intensity. The differences in character are those which are exhibited in the spectrum. The differences of intensity are exhibited whenever the intensity of the light, which is split up to make the spectrum, varies. One sensation of hue differs from another sensation of hue either in character or intensity.

It is important that we should make clear and definite a distinction as to optical sensations. Optical sensations differ from auditory sensations in this particular respect: if we have a number of sounds stimulating the sense of hearing, we can pick out each of these sounds and we hear them, at least to some extent, separately. On the other hand, if the eye is stimulated by a composite color the effect is to produce a single color, which may be achromatic or chromatic. If it is chromatic we see a single hue the intensity of which is more or less affected by the amount of achromatic color accompanying it. In the case of sound, we may have a number of instruments each producing a single definite sound wave differing from the others. These sound waves come to the ear and the ear can distinguish them. In the case of light we have innumerable light waves differing in wave length, and when they enter the eye they are combined in some way so that so far as hue is concerned at least only a single hue sensation results. To illustrate: If two disks—one red and the other yellow—are fitted together and spun on the Howland machine, there is produced a single hue sensation, orange. We may say that the orange is similar to the red and similar to the yellow; but as to sensation, it is a single sensation.

At this point we are led to make use of a word which we have not used very much heretofore. This is the word "component." The word "constituent" implies something ultimate. The word "component" implies something proximate. We have used the word "constituent" to describe the spectral colors into which a stimulus color may be decomposed. We have indicated that sensation color, according to one theory, may be decomposed into three constituents—the primary physiological colors. The components of a stimulus color, however, are any colors which combined produce that color. In the above spinning operation where red and yellow were spun together to produce an orange, the components of the orange were red and yellow. Now it is obvious that the components of a stimulus color may be infinite in number; thus, a certain neutral gray may be produced by spinning any two complementary colors in proper proportions with the addition of white and black as may be necessary. In this case, the components of the stimulus color may be any of the colors into which the color produced may be resolved.

On the other hand, it should be noted that sensation colors are much more limited as to their components. In general form it may be said that sensation colors have only one or two components. Achromatic sensation colors have only one component, that is white; chromatic sensation colors may have one or two components, that is, white and a hue, or, theoretically at least, a hue unit. This is in accordance with the quotation of Luckiesch which we have already given. It is obvious, therefore, that stimulus colors act in two ways upon the eye—first, to produce balanced sensation and, second, to produce unbalanced sensation; the unbalanced sensation being that of a single hue.

How Hues Are Related

Considered from the standpoint of stimulus, the relation between hues is simply the relation of wave length. In a visible spectrum we are able to distinguish about 128 hues, each having, as indicated above, what may conveniently be called "a single wave length."

Considered from the standpoint of sensation, we have the same number of hues; that is to say, each wave length produces its own characteristic sensation. It would appear, however, that we are able by blending blue and red to produce hue sensations about which there is some doubt as to whether they exist in the spectrum or not; at least, they are so faint—that is, so lacking in intensity—that it is difficult to distinguish them. These purples and violets which are produced by the blending of blue and red and which are very difficult to see in the spectrum may be considered as "extra spectral."

It has already been pointed out that spectral colors as they are arranged in the spectrum insensibly blend into each other. This arrangement of colors in the spectrum is what may be considered "the natural arrangement" in this particular: If we start at the blue end of the spectrum and allow the eye to travel towards the red end the successive steps are not in great contrast with each other. If, however, we compare the blue with each of the succeeding steps that contrast increases until the yellow is reached and then decreases until we pass from the red into the extra spectral mixtures of violet and purple. This indicates the first relation we wish to call attention to between hues: Hues differ in the degree that they are in contrast with each other.

If we arrange in a circle a series of colors which have hue characteristics as nearly as may be to spectral and extra spectral hues, and in their spectral order, we develop a circular relation between hues which has a very important bearing upon color problems. Fig. 4 is such a color circle. If the colors have been selected discreetly, those colors which are opposite to each other in the color circle will be found to be in the greatest contrast with each other. Such colors are said to be complementary and they possess the capacity of neutralizing each other to the highest degree. In each case in the color circle illustrated here, opposite colors when blended by spinning in proper proportions produce a true neutral gray. All other colors, that is to say those that are not opposite to each other, when spun in pairs produced colors having intermediate hues. After performing a number of these spinning operations we reach these three general conclusions: first, any two contiguous colors spun together, no matter in what proportion, give a color having an intermediate hue of an intensity nearly as great as that possessed by the hues in the original colors. Second, any two opposite colors spun together in proper proportion give a color in which the hues neutralize each other, with the result that a neutral gray is produced having no hue intensity. Third, any two colors, neither contiguous nor opposite, spun together in equal proportions give intermediate colors of reduced hue intensity,

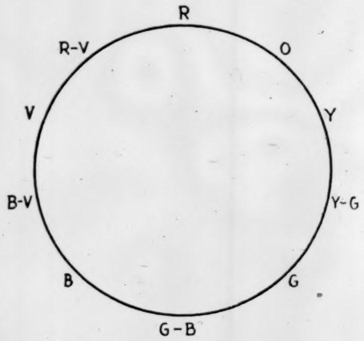

FIG. 4.—Diagrammatic Color Circle

this reduction of hue intensity being due apparently to partial hue neutralization. These three observations lead to the general statement that the capacity of hues to neutralize each other is proportional to their dissimilarity.

The spinning of colors dissimilar to each other as to hue with the production of a neutral gray is not limited as to the number of colors that may be spun. Three or more suitable colors may be spun together producing a neutral gray; thus, if two colors when spun in proper proportions produce a color complementary to a third color, then the three colors may be spun together in proper proportions to produce a neutral gray.

The spinning of two or more colors together for the purpose of matching some other color is what takes place in most colorimetric work involving the use of secondary standards, as is done for instance with the Howland machine. The colors spun may be considered the components of the color which is matched.

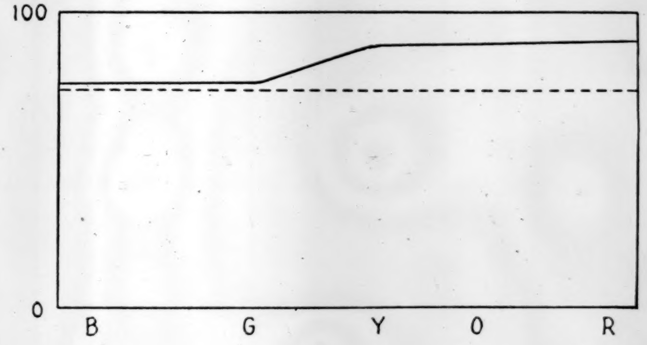

FIG. 5.—Chart of Spectrophotometric Analysis of a Pink

The combination of two or more colors to produce any color, as is done with the Howland machine, may also be done in other ways. The mixing of pigments dissimilar in color to produce some other color is a well-known procedure. In a great deal of artistic work dissimilar colors are applied in spots. These dissimilar spots of color when seen at a proper distance blend into each other and stimulate an intermediate sensation. This applies to achromatic colors as well as to chromatic colors.

We must always bear in mind, particularly in the study of hues, that no matter how many constituent hues there may be in a stimulus color no more than a single hue sensation results therefrom. Suppose, for instance, we examine a certain pink spectro-photometrically and that we get a chart representation of the intensity of the hues present, such as is illustrated in Fig. 5. If we consider the portion of the curve above the dotted line as representing the unbalanced constituents of this pink, we will see that there are present in unbalanced combination rays having the yellow, orange and red characteristics. How is it then that the sensation produced is only that of red when we look at this pink? The simplest answer that we can give is that the organ of vision combines all stimulus hues to the extent that they are unbalanced into a single resulting sensation hue. This combination presumably takes place in the eye. In reaching such a presumption, however, we are dealing really in a species of theory that we should not accept too conclusively. How a number of stimulus hues become combined to produce a single hue sensation, we shall try to explain as simply as possible.

If we consider the circular relation of hues which we have endeavored to explain above and we think of this circular relation as though it represented certain forces, the directions of which operate from the center towards the location of the hue in the circle, we may get some explanation of what takes place when hues become combined. Obviously, hues that are opposite to each other in the circle, which produce achromatic sensation on spinning, on this principle involve two directly opposing forces, and if these forces become equalized we have as a resultant a force having an intensity of zero. Similarly, with any other two hues which are not opposite to each other in the color circle, we will get an intermediate hue having a direction and an intensity determined by the direction and intensity of the hues combined. What is true of any two hues should also be true of any greater number of hues, because by the methods of vectors any number of forces operating in different directions can be combined to obtain a single resultant. We will not attempt at this time to present illustrations of how this takes place, but in the case of the pink, of which we have shown a diagram obtained by spectro-photometric analysis, all the hues in unbalanced state bear angular relations to each other and their direction and intensities may be represented by lines drawn from a common point as a center to points representing in distances their respective intensities. These lines may be considered vectors which may be combined to produce a single resultant hue of determined intensity.

ATTRIBUTES OF COLOR

If we consider a color, as originally defined, is any distinguishable optical sensation, we come to the conclusion that colors may have three attributes. The first of these attributes is, according to the latest nomenclature, brilliance. Other terms describing brilliance have been used by writers on color and in the main, brilliance, it is agreed, is synonymous with the words "luminosity" and "value." All of these words represent the total optical intensity a color may possess measured in terms of achromatic color, either in stimulus or sensation steps. The customary method for the determination of brilliance is to compare the color with some series of achromatic colors arranged either in stimulus steps or in sensation steps. In colorimetric work it is a matter of choice to use either the stimulus series or the sensation series for comparison. In artistic work, the sensation series is to be preferred. The method of procedure is this: a series of achromatic colors is arranged with white at one end and black at the other, with grays between differing from each other in definite steps. The color, the brilliance of which it is desired to measure, is placed alongside of these achromatic colors and compared at a suitable distance until one is able to say that the color being tested corresponds in brilliance to a certain achromatic color. This seems to some a confusing operation—for how can you compare a certain red with a certain gray and say that the brilliance of the two is identical? The answer is simply this: that, practically, it works out, particularly with reference to the use of colors for artistic purposes.

We must here emphasize again the distinction between stimulus colors and sensation colors. Stimulus color, strictly speaking, has simply those attributes which are disclosed by the intensity of its constituents. The attributes of color that we are here speaking of relate only to sensation color. Brilliance is solely a matter of sensation. In the case of achromatic colors it is the only attribute that such colors possess. Therefore, it has been found convenient to compare chromatic colors and say that they correspond in brilliance to certain achromatic colors.

The second attribute of color is hue. Of course only chromatic colors possess hue. We need not here discuss hue very fully. We have already explained what hue is to the best of our ability. We will simply say that hue is that peculiar quality of color which distinguishes a color from a gray of the same brilliance. The extent to which such a color is distinguishable from such a gray depends upon the degree of saturation of the hue. Saturation, therefore, which is considered a third attribute of color, is related to the intensity or concentration of the hue. The word "purity" has been used as synonymous with "saturation." Inasmuch, however, as the word "purity" has so many shades of meaning, it has generally been discarded and the word "saturation" used in its place. The word "purity" also has been presumed to indicate the degree of absence of white light. It is preferable to avoid such a presumption.

Considering these three attributes of chromatic color, in what way are they related to each other? Brilliance is the total sensation effect in a scale of achromatic color in terms of intensity. There are two components of chromatic color—white and hue. The total effect therefore is the effect of the white plus the effect of the hue. As affecting brilliance, Mr. Arthur Howland, if I understand him correctly, holds that the hue component gives character to chromatic color but does not affect brilliance. Whether this is strictly true or not, I am inclined to believe that it is substantially true.

If we limit ourselves to the consideration of colored surfaces illuminated under standard conditions, we can readily understand that the sensation produced by any one of these colored surfaces is made up of the intensity of the white and the intensity of the hue. Obviously, to the extent that we increase the intensity of the white we may decrease the intensity of the hue, and *vice versa*. We find, therefore, that there is a reciprocal relation between hue intensity and white intensity—as one goes up the other tends to go down. For each hue there is an apparent maximum of intensity; such a hue is highly saturated. Usually speaking, for any specific hue the highest degree of intensity is accompanied by a loss of brilliance. This is in accordance with the old conception of the word "purity," for when a hue is very intense it is also very pure; that is, it is free from the white component.

If we consider the two words "saturation" and "purity" as having somewhat distinct meanings we get the following interesting data: The strongest yellow that we can get by the use of pigments, that is, the most highly saturated yellow, is very impure in that it reflects about 75 per cent of the incidental white light as white light. The strongest reds and greens that we can get by the use of pigments, that is, the most highly saturated reds and greens, reflect about 25 per cent as white light, while in the case of blue, the strongest blue that we can get by the use of pigments, that is, the most highly saturated blue, reflects only 5 per cent as white light. This blue therefore, as compared with the yellow, red and green, is the purest.

We give in Fig. 6 a diagram based upon a diagram in the Report of the Committee to the Optical Society of America and referred to by the following title: "The Dimensions of Psychological Color Solid." In this diagram it is presumed that each chromatic color has a brilliance plane which intersects somewhere the vertical scale of brilliance which runs from white to black. Obviously, with any single hue we have a simple two-line representation of brilliance and saturation as A B C D in Fig. 6. For the complete representation of all colors having a given hue we get a triangular representation as A C E in Fig. 6. Here we see represented all that we have already referred to, that is, as brilliance increases, the intensity of the hue must decrease. The saturation, however, is limited by the hypotenuse of the triangle for any brilliance plane.

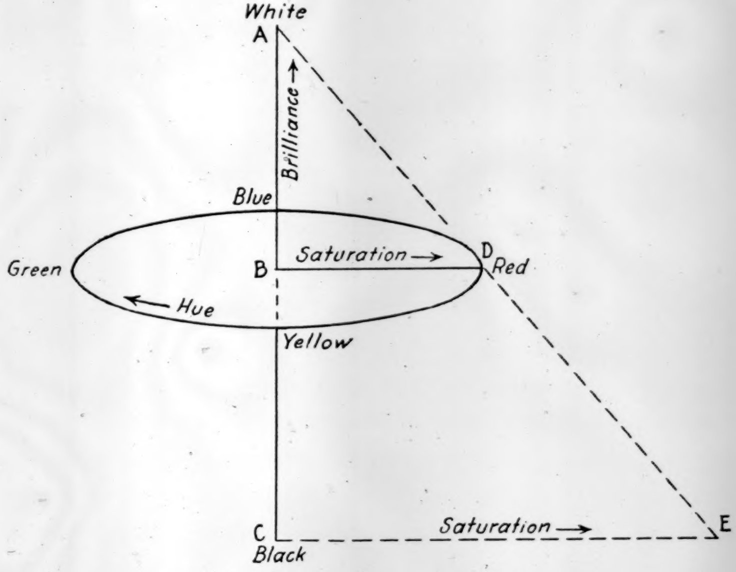

FIG. 6.—Dimensions of Psychological Color Solid

In all that we have said with reference to the attributes of color we have attempted to be strictly consistent with the three definitions given in the Report of the Committee to the Optical Society of America, as follows:

> "Brilliance is that attribute of any color in respect of which it may be classed as equivalent to some member of a series of grays ranging between black and white."
>
> "Hue is that attribute of certain colors in respect of which they differ characteristically from the gray of the same brilliance and which permits them to be classed as reddish, yellowish, greenish or bluish."
>
> "Saturation is that attribute of all colors possessing a hue, which determines their degree of difference from a gray of the same brilliance."

A METHOD FOR THE ESTIMATION OF THE COLOR OF OILS

By D. C. Cox[1]

It is hardly necessary to go into detail in discussing the many difficulties encountered in attempting to accurately estimate the color of an oil. The principal causes responsible for this may be summarized as follows:

1. The standard, whether a liquid or solid color filter, is difficult to duplicate.
2. The standard is liable to change.
3. The readings vary slightly with the color of the light source.
4. Different observers will frequently check themselves but not each other.

In the colorimeter herein described the standard is absolutely invariable and may be easily and accurately copied. The color of the light source is constant. By actual test it has been proved that an observer afflicted with color blindness can check an observer with normal vision. The instrument is in current use in the laboratory and has given complete satisfaction.

Principle of Method.—The measurement made by the instrument is the thickness of a layer of oil which absorbs 50 per cent of monochromatic blue light. If any multi-chromatic light, such as white light or daylight, is used in a colorimeter, it requires of the observer a mental integration of the intensities of the various colors present. Although we perform this easily and quickly, unfortunately we do not all arrive at the same result. Color blindness is a more or less exaggerated case of failure to integrate colors properly. Hence monochromatic light is necessary if all observers are to agree on results. The use of blue light is based on the fact that it is only the very short waves which are absorbed by all colored oils. Green light[2] is absorbed by dark oils, but as we examine lighter and lighter oils the absorption of green becomes zero long before the oil is colorless. Yellow light[3] is absorbed by very dark oils. All other oils transmit yellow light readily.

The measurement of the depth of oil which transmits a certain fraction of incident light has been chosen because from it we can easily obtain the reciprocal, and this latter figure is quite closely

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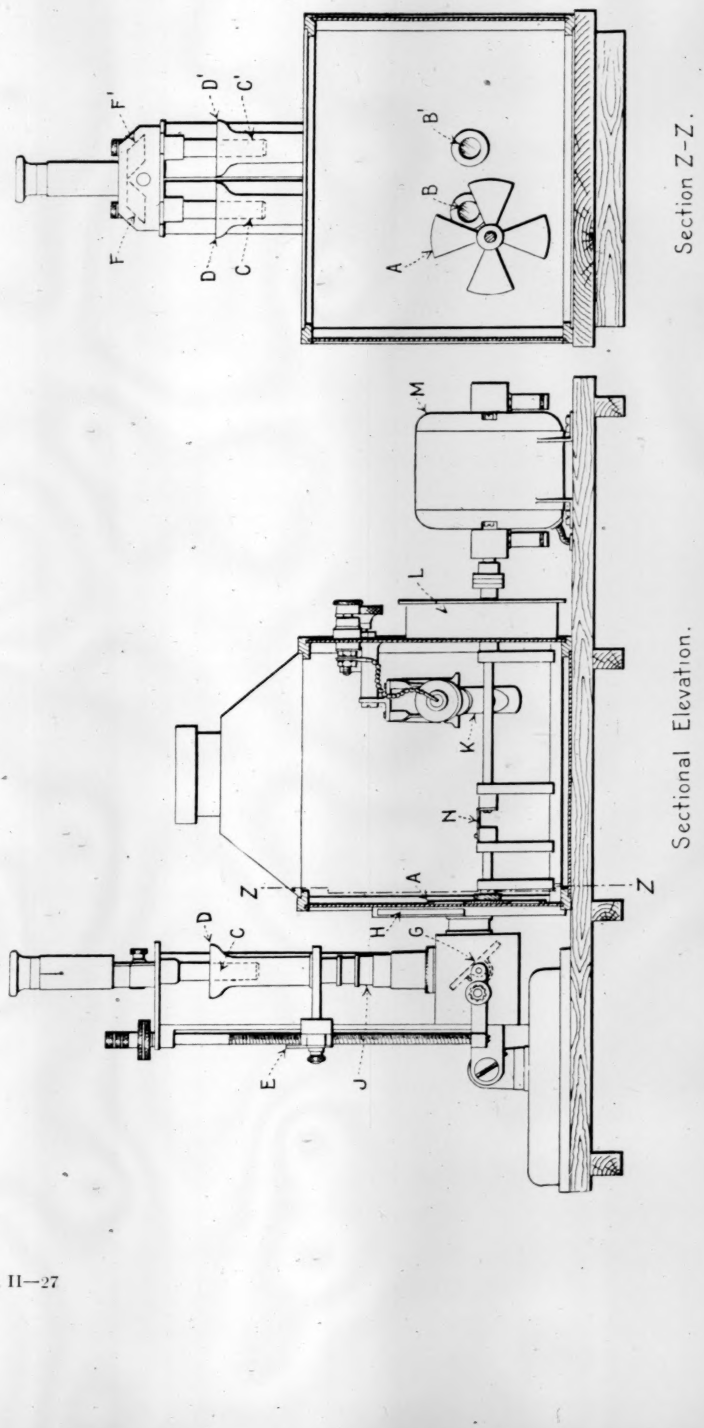
[1] General Electric Co., Pittsfield, Mass.

[2] Monochromatic green light may be obtained by substituting Wratten filter No. 62 for No. 50.

[3] Monochromatic yellow light may be obtained by substituting Wratten filter No. 22 for No. 50.


F'
D'
C'
F
D
C
B
B'
A
M
L
K
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Z
Z
D
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H
G
E
J

Section Z-Z.

Sectional Elevation.

FIG. 1.—Colorimeter.

proportional to the concentration of coloring matter in similar oils, and hence is very convenient when mixing or blending to produce certain colors. As the absorption of light by oils is a very complex phenomenon, caution must be observed not to overestimate the range over which blending may be accurately done.

An example of what may be expected in blending is given in Table I. The figures in the last column are proportional to the percentages of dark oil present.

TABLE I.—COLORS RESULTING FROM VARIOUS MIXTURES OF DARK AND COLORLESS OIL

Designation	Volume of Dark Oil	Volume of Colorless Oil	Color Observed	Color Calculated from Concentration of Coloring Matter
A	0	100	0	0
B	5	100	0.30	0.31
C	10	100	0.60	0.59
D	20	100	1.04	1.08
E	40	100	1.80	1.85
F	80	100	2.80	2.87
G	160	100	4.00	3.98
H	100	0	6.73	6.44

Description of Instrument.—The apparatus consists essentially of a special illuminator and shutter designed to be used in connection with a Kober colorimeter or any other instrument operating on the same principle. The illuminator, K, Fig. 1, is a mercury vapor lamp.[1] The lamp is contained in a suitable metal housing which serves to confine the light to the interior. A blower, L, driven by a small motor, M, serves to ventilate the housing and thus keeps the lamp from overheating. Light from the lamp passes through two openings B, B' containing suitable fixed diaphragms. Thence it passes through a blue filter (Wratten No. 50) which removes all rays except a deep blue line. This gives absolutely monochromatic light of a wave length which is most readily absorbed by colored oils. The two beams of monochromatic blue light are reflected from the sloping adjustable mirrors, G, through the telescoping tubes, J, to the glass cups D, D'. These may be raised and lowered to vary the thickness of the layers of oil under observation.

The glass tubes C, C', the prisms F, F', and a lens system in the eyepiece serve to give the observer a field consisting of a split circle the right half of which is illuminated by light passing through the right oil sample, the left half being illuminated by light passing through the left sample.

The motor is arranged also to drive a rotating shutter, A, whose purpose will be explained later. A simple clutch, N, serves to release the shutter when desired.

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[1] The quartz tube lamp is convenient because of its size.

Balancing the Instrument.—Before beginning each day's work with the instrument it is important to ascertain that the instrument is accurately "balanced." This is done by placing some water white oil (benzol) in the cups *D* and *D′*. Their position is here immaterial. The shutter is moved to a position such that its blades do not intercept light entering *B*. The lamp is started and the observer looks into the eyepiece. If the balance is correct the two halves of the circle will look alike. If they do not the mirrors *G* are rocked until a balance is obtained, preferably with both halves brightly lighted.[1]

Operation.—Having balanced the instrument we are assured that the light rising through the two telescopes is of equal intensity. (The oil used in balancing is of course removed before making a test.) The specimen of oil to be examined is now placed in cup *D′*. By varying the position of this cup the sample may be made to absorb more or less of the blue light. The shutter is now set rotating and, because of its design, serves to intercept exactly half of the incident light from *B*. The position of *D′* is adjusted until the two halves of the circle match. The depth of oil in *D′* is read from a vernier scale *E*. This is the thickness in millimeters which absorbs 50 per cent of this standard blue light. A movement of the decimal point of course gives the thickness in centimeters and the reciprocal of this latter figure is the "Color" according to our scale.

It has been found convenient to use the shutter only about once a week for standardizing a sample of oil. Measurements are then made by comparing the unknown in *D′* with the standard in *D*, the shutter being, of course, stationary and wide open.

Mr. Priest of the U. S. Bureau of Standards showed the writer his excellent apparatus for measuring the transmissivity[2] of oils for light of all wave lengths. I am indebted to him for the idea of the mechanical filter, the shutter, which is the nucleus around which the present apparatus was designed. The transmissivity is, however, not a factor suitable for our purpose, not being any simple function of the concentration of coloring matter.

The apparatus as described has proven very satisfactory for oils varying in color between 0.05 and 10. Darker oils may be diluted say 10 to 1; the color will then be one-tenth of the original. This method is being used occasionally for very dark oils, but it is believed that another method now being developed, which utilizes thin films of oil, will be found more rapid and accurate.

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[1] It has been found easier to get accurate balances by covering each mirror with a sheet of unglazed white paper which gives softer illumination but lower intensity.

[2] The percentage of light traversing a layer 1 mm. thick.

DISCUSSION

Mr. Wilson

MR. ROBERT E. WILSON.[1]—Mr. Cox has given us a very accurate and reliable method for determining the color of oils with the one great advantage of requiring no standard which must be checked up from time to time or kept constant. There are, however, one or two statements that are probably a little broader than he really intended. Take, for example, the statement that if monochromatic light is not used, the integrating effect of different eyes will give different results. That is quite true if the two things to be matched have different absorption spectra, but if the absorption spectra of two oils are the same, and they are brought to a match with any kind of light, the eye cannot possibly differ in integrating the color of the light from the two oils because it is identical in all parts of the spectrum. One person might say that the color was green and another call it yellow, but the depth of one oil required to match the other would be agreed upon by both observers. If, however, the attempt is made to match materials with different absorption spectra, as, for example, oil and colored glass, different eyes will differ greatly, although each will get what he considers a fair match. It is my experience that with most petroleum oils the absorption spectra, if brought to a match, are surprisingly close to identical, so that the average deviation between different observers in carefully matching two oils is only about one per cent. This fact was the basis of the method on which Mr. Parsons and I reported some time ago.[2] That method has one advantage as compared with the proposed method in measuring dark colored oils in very thin films. It is extremely difficult to get a variable thin film whose thickness is accurately known, and yet it is necessary to have a variable film if the match is to be made on the basis of 50 per cent light absorption; whereas if an oil is used as the standard, it is possible to have a film of *constant* thickness, or a set of two or three films, whose thickness is accurately known, and vary the depth of the standard to get a match.

Just one other point might be mentioned: the statement was made that when oils are diluted ten times with some other solvent, the color is one-tenth as great. In the paper referred to above, we showed

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[1] Assistant Director of Research, Standard Oil Co., Ind.

[2] Parsons and Wilson, "A New Method of Color Measurement for Oils," *Journal of Industrial and Engineering Chemistry*, Vol. 14, p. 269 (1922).

Mr. Wilson that this could not be correct, because the same concentration of heavy oils in different solvents gave different colors. It is true that mixtures of two mineral oils of similar viscosity check up fairly well, but if any of the light solvents are used, the variation is likely to be quite considerable.

Mr. Flowers MR. A. E. FLOWERS.[1]—I understood from what Mr. Cox said that the oils, while absorbing the same in the blue and violet range, differed in absorption in the red, yellow and green, and I should anticipate that oils showing a markedly different color to the eye would give the *same* measure on the instrument. I should like to have Mr. Cox take up that point if he will.

Mr. Cox MR. D. C. COX.—First, I should like to say in regard to the variable thin films, that in the normal operation of this colorimeter, we do not use the shutter; the shutter is used but once a week for standardizing a batch of oil. Aside from the weekly standardization, the color measurement is based on the ratio of the depth of the standard oil and the other, and using a thin film, say 0.5 mm., we can vary the depth of the standard and in that way use the same thin cells which you would use in another instrument. That is the method we are using at present.

In regard to dilution, I can refer, for example, to Table I of the paper, which shows about what can be expected from such dilution. In that case we used two different oils; one a dark oil and one a colorless oil, and obtained results as close as we were interested in obtaining. Now it is true that if an oil is mixed with another oil which changes the state of the coloring matter, such as commencing to coagulate it or change its colloidal state, you get very different colors, but we aim to use an oil similar to the one under observation, and in that way we have not encountered any trouble.

As to the question in regard to two oils which would give the same absorption of blue light and yet appear different to the eye: To be extremely accurate that is quite possible, but we found before we adopted the monochromatic blue light, that certain oils could not be matched under any circumstances in white light. There was no depth of one oil which would exactly match some depth of the other oil. One would appear reddish and the other brown, and you might match the candle power but you could not match the color. One observer would have a tendency to make two halves equally brilliant and the other would try to get one about as dark as the other along the spectrum. In monochromatic light, all oils can be matched. There is no oil but what has some definite absorption of blue light.

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[1] Engineer, In Charge of Development, The De Laval Separator Co., Poughkeepsie, N. Y.

The Chairman

THE CHAIRMAN (MR. F. R. BAXTER).[1]—Sub-Committee VI on Color of Committee D-2 on Petroleum Products and Lubricants[2] has centered on the use of two particular instruments for measuring the color of oils, the Saybolt chromometer for the refined oils and the Union colorimeter for the lubricating oils. Mr. Cox's apparatus will differentiate to a finer degree than either of these. The Saybolt chromometer has been used for a good many years and is definitely tied up with the refined oil business, so that that one is particularly useful. The Union colorimeter is a shop method; it does not pretend to give a very close distinction like Mr. Cox's apparatus does, but it is very useful for standardizing the colors of commercial oils.

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[1] Chief, Testing Laboratory, Vacuum Oil Co., Rochester, N. Y.

[2] *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 352 (1923).

ELECTRICAL INSULATING VARNISHES

By J. A. Findley,[1] Dean Harvey,[2] and S. T. Rodgers[3]

Up to the present time there have been but few authoritative data available relative to insulating varnishes. Most of the information which has been published has been a record of personal experience or practice with a particular varnish in a given application and not a discussion of the fundamentals necessary to a more complete understanding of the insulating varnish problem in its entirety. There have been wide differences of opinion among the users of varnish as to the characteristics most required in the various types of varnish and there is but little uniformity in the practices now being followed by the electrical manufacturers who use varnish. The varnish manufacturers have followed their own ideas and ideals in developing their particular lines of varnishes in the past and for these reasons there has been neither standardization of the materials themselves or of their uses. This condition makes it practically impossible to draw up standard specifications for insulating varnishes and it has been necessary for each user and producer of varnish to make his own tests, draw his own conclusions from the results of such tests, and establish his own standards.

The American Society for Testing Materials, in the Tentative Methods of Testing Insulating Varnishes (D 115 – 22 T),[4] has developed standard tests which are invaluable to both users and producers of varnish. These tests may be used by the varnish manufacturers in their experimental research work and thus point the way to new and improved varnishes. When used by both the users and producers of varnish, they afford a common ground for discussion of all points involved in the purchase and sale of such materials. Also, certain of the tests indicate very closely the actual working characteristics of the varnish, from which specific directions for their use can be outlined.

At present, the Society has outlined merely the method of making the tests and it is necessary for every one to interpret the results of his tests and determine whether the characteristics indicated are those which are desired for the particular problem in hand. It is hoped that the discussion of the characteristics of varnishes with their relation to the tests as outlined in this paper will assist the reader to a better understanding of this whole problem and make possible a more accurate interpretation of the test results.

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[1] Electrical Engineer, Rochester, N. Y.

[2] Materials Engineer, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.

[3] Insulation Engineer, Sherwin-Williams Co., Cleveland, Ohio.

[4] *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 892 (1922).

NATURE AND COMPOSITION OF INSULATING VARNISHES

Before taking up the various tests in detail, it will be helpful first to obtain a clear understanding of the nature and general composition of insulating varnishes. Insulating varnishes may be described as liquids of varied color and consistency, composed of a useful semi-solid part or "base," made up of resins, asphalts, pitches, drying oils, driers, etc., and a solvent in which this base is dissolved or uniformly incorporated. They are used to fill or cover paper, fabric and other materials, thus improving their physical and insulating properties by replacing the occluded air with a dielectric of higher order or helping to maintain the constancy of the initial insulation of the parts treated by affording an impermeable covering to these parts through which deleterious substances cannot pass. They should be of such nature that, after application, they may be changed from the liquid to the solid state, or practically so, by some convenient means such as heat. In order to perform their function, they must possess numerous and varied qualities, both before and during setting, as well as in their subsequent useful life.

Spirit varnishes consist of varnish gums or resins dissolved in a suitable solvent, usually alcohol. The varnish may or may not be colored by the addition of dye and usually contains ingredients to improve the flexibility. These varnishes dry by the evaporation of the solvent, which leaves a film of the gum on the surface to be coated. By some, spirit varnishes are considered more as finishes than as insulating varnishes, for their principal use is as a finishing coating over other insulating varnishes. In some applications, however, they are used for insulating purposes where little flexibility and great binding and cementing properties or exceptionally quick drying are required.

Oil type insulating varnishes consist of varnish gums or resins in combination with vegetable drying oils (generally China wood oil and linseed oil) dissolved in a suitable thinner, usually petroleum naphtha. In the black varnishes, the clear gums are replaced with asphaltic materials or pitches. The oil type varnishes dry by the evaporation of the solvent and oxidation of the vegetable oils. The drying time is governed by the proportion of gums and oils and by the quantity of oxidizing agents or driers in the varnish. The greater the proportion of oils, the longer the drying time. They are classed as air drying or baking varnishes, depending on the method of drying and length of drying time.

Some of the quick air-drying black varnishes may contain no drying oil. Such varnishes are neither oil type nor spirit varnishes and may be termed "asphaltic varnishes," consisting of an asphaltic base and solvent which may be a petroleum or coal-tar distillate, or a mixture of the two.

In the manufacture of spirit varnishes, the gums are cut cold in the solvent, while the oil type varnishes and asphaltic varnishes are made by fluxing the various ingredients together in a kettle at a high temperature. Variations in the physical characteristics are produced by variations in the formulas and in the heat treatment.

Liquid varnish changes to a solid by drying and during this process passes through all the intermediate stages. To thoroughly understand the characteristics of a varnish, it is necessary to study it both in the liquid and solid states. The most important qualities or characteristics of insulating varnish in the liquid, intermediate, and solid states are:

LIQUID

- Color
- Viscosity
- Flash point
- Specific gravity
- Percentage of base (volume)
- Percentage of solvent (volume)
- Nature of solvent
- Penetration
- Flowing
- Draining
- Ease of thinning
- Tendency to skin over
- Loss by evaporation

INTERMEDIATE

- Flowing
- Draining
- Uniformity of coating
- Time of drying
- Surface drying
- Drying throughout
- Chemical reactions with copper, iron and other insulating materials

SOLID

- Hardness
- Plasticity
- Binding properties
- Flexibility
- Adhesiveness
- Resistance to
  - Oils
  - Water
  - Alkalies
  - Acids
  - Continuous heat
- Dielectric strength
- Resistivity
- Dielectric constant
- High softening temperature

It has not been possible to combine all the desirable qualities in one product and for this reason many distinct types of insulating varnishes have been produced in which certain qualities have been improved at the expense of others to make them most suitable for specific uses. Many of these characteristics are interdependent while others are opposed and cannot be obtained in the same material. A few of these are shown in the following tables:

Interdependent Characteristics

Heat endurance Flexibility	Drying time
Oil resistance	Hardness
Uniformity of coating	Viscosity Volatility of solvent
Thickness of coating	Viscosity Volatility of solvent Percentage of base
Penetration	Viscosity
Loss by evaporation	Volatility of solvent

Opposed Characteristics

Hardness Oil resistance Binding properties High softening temperature	Plasticity
Flexibility Heat endurance	Quick drying
Small evaporation loss Uniform thickness of coating	Rapidly evaporating solvent Low flash point

Discussion of Tests of Insulating Varnishes

Standard tests have not been developed to measure all the qualities mentioned above, but all the important controlling characteristics can be measured and the others approximated from observation and comparison. As no varnish can possess all the desirable qualities to the maximum extent, it is necessary to consider the varnish from the standpoint of the use which is to be made of it and interpret the test results with the view of selecting the most suitable varnish for the purpose in mind.

The tests outlined for the liquid varnish in the A.S.T.M. Tentative Methods are specific gravity, viscosity, flash point and evaporation, the last-named test being recommended this year by Committee D-9 on Electrical Insulating Materials.[1]

Specific Gravity is a measure of the weight of varnish in relation to that of water. The determination gives a figure that expresses this weight but does not take into account the factors that make it up. High specific gravity might indicate by itself either a high percentage of base (oils and gums) or a solvent of high gravity combined with a normal percentage of base. It is not an indication of the volume of covering or filling material per gallon, or, in other words, of the proportion of base and solvent.

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[1] Report of Committee D-9 on Electrical Insulating Materials, *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 431 (1923).

Viscosity is of principal value in comparing samples of the same or similar varnishes. This characteristic is greatly affected by the heat treatment given the oils in the varnish and is not a true indication of the proportion of base and solvent. It is, however, an important factor affecting the thickness of coating produced by the varnish.

Flash Point indicates the probable nature and volatility of the solvent, or in the case of varnishes having a blended solvent, the volatility of the most volatile flammable ingredient.

The Evaporation Test indicates the relative quantities of solvent which will be lost through evaporation and which will have to be replaced to maintain the varnish at the proper consistency in the dipping tank. This is a factor affecting the cost of using the varnish.

We now come to the point where we must consider the changing features of the varnish as it passes through its other phases toward hardening. As a general rule, the varnishes are applied by dipping until the pieces treated are well saturated. They are then withdrawn from the bath and allowed to drain until the setting stage is reached. At this stage, some of the solvent has evaporated and the viscosity of the varnish base is sufficient to stop the flow.

The Draining Test[1] is of great value in indicating the working characteristics of the varnish during this time. It is important that the various elements be so combined that the thickness of film on surfaces and the volume throughout a mass be as evenly distributed as possible from top to bottom. Of course, mechanical means such as turning the pieces may be resorted to, but this takes time and adds to production cost. In such arrangements as conveyor ovens it is next to impossible. The thickness of coating is controlled by the viscosity, percentage of base, volatility of thinner and speed of withdrawal. When a coil is withdrawn from the varnish, the quantity of liquid varnish then adhering to it is governed by the original viscosity of the varnish itself and the speed of withdrawal. As draining begins, the relation of values is constantly changing due to the continual evaporation of the solvent until the time comes when enough solvent has evaporated to allow the inherent strength of the varnish base to maintain stability. Then movement ceases and setting really begins to produce a final outside film which gradually hardens. It should be remembered in this connection that rapidly evaporating solvents give a shorter flow and varnishes containing them show a greater increase of deposit from the top to bottom of the treated piece, while heavier solvents tend to give longer flow and therefore a more uniform thickness of coating. A solvent of too high a distillation range would introduce objectionable features. For instance, it might slow the drying and affect the enamel on enameled wire.

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[1] Report of Committee D-9 on Electric Insulating Materials, *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 431 (1923).

The Time of Drying Test is important, as the results of the other tests may be affected greatly by it. Varnishes dry by evaporation of the solvent and oxidation and polymerization of the oils. Oxidation begins at the surface and progresses inwards. Most varnishes contain drying agents to accelerate oxidation, and due to differences in composition the drying characteristics may not be the same. A strong drying action may produce surface drying, which would be indicated by a hard surface free from tack and soft unoxidized varnish underneath. This is an undesirable characteristic and in determining the drying time and preparing the test specimens, precautions should be taken to make sure that the varnish is hardened throughout. Testing the film with the finger nail and examination of the bead or fatty edge give some indications as to the degree of skin hardening as compared to that throughout the mass. It is to be noted that hardening is progressive and continues to the point of final break-up of the varnish at the end of its usefulness.

Dielectric Strength, unfortunately, cannot be determined as accurately as would be desired. Although in the tests two dip coats producing four separate films of varnish are specified, it is very difficult to secure a perfectly uniform continuous coating. Dust present in the varnish, or settling on the test specimens while draining, and small air bubbles will produce minute perforations in the film and lower the dielectric strength. The degree of baking also has an important bearing on the dielectric strength. As the varnish hardens through baking, the dielectric strength increases and in making this test it is important that all varnishes be hardened to the same extent. All films must be of the same thickness, as thinner films harden better and show a higher dielectric strength per mil.

The Water Absorption Test is of great value, probably being second only to the heat endurance test. While it is subject to the same conditions affecting the dielectric strength test, the latter test affords a correction factor, for the reduction in dielectric strength due to immersion in water is the basis of comparison in the water absorption test. It is very important that the films be of the same thickness and of the same degree of hardness. The loss in dielectric strength apparently is governed by the depth to which the water penetrates and the measured dielectric strength is in reality the dielectric strength of the unpenetrated film. A heavier initial film will give a higher dielectric strength, as there will be a greater thickness of unpenetrated varnish at the expiration of the 24-hour period.

Heat Endurance is of great importance and is only relatively indicated by the test. The test is a measure of the time a film of given thickness may be baked and still not break when stretched a specified amount. It is an indication but not an exact measure of the life of the varnish in actual service. In making the bending test, it is important that the test specimens be cooled to the proper temperature and bent over the rods in a uniform manner. If the test specimen is bent quickly, the varnish film will crack more readily, and the test specimens must be bent around the rods at the same speed to secure comparable results.

The Acid, Alkali and Oil Resistance Tests are in the same category with the water absorption test and equally important. While, of course, varnishes may be chosen for their resistance to any one of these products, the ideal varnish would withstand all to the greatest possible degree because it is used to protect the rest of the insulation. Different types of varnishes show widely different results depending upon their composition. In general, varnishes containing fairly high percentages of asphaltic ingredients best resist the action of both acids, alkalis and water, while those which are high in the drying oils are found to be the most oil proof. Acids tend to further oxidize the oils until they are finally burnt, alkalies to saponify them. The black gums are seemingly very little affected by either and are generally hydrofuge. The hardened oils are, however, only very slightly affected by transformer or lubricating oils and even by their original solvents. The oil resistance of any varnish is increased by over-baking. In such a case, one can even obtain an oil-proof coat from a varnish containing practically no drying vegetable oils. It is essential, therefore, that the tests for oil resistance be made on varnishes baked to a uniform degree of hardness in relation to their surface drying times.

The Test for Non-Volatile Matter[1] is an indication of the proportion of base and solvent by weight. Varnishes are sold by volume and this test does not indicate the proportion by volume. It is of value in making comparisons only where the varnishes tested are of the same type and have solvents of the same specific gravity. The heavy gravity solvents show a lower percentage of base when computed by weight, and as the varnish manufacturers use solvents of widely varying specific gravities this test should be used with discretion. It is of value and is frequently incorporated in specifications covering the same or similar varnishes.

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[1] Report of Committee D-9 on Electrical Insulating Materials, *Proceedings*, Am. Soc. Testing Mats., Vol. 23, Part I, p. 431 (1923).

PHYSICAL CHARACTERISTICS OF INSULATING VARNISHES

To adequately meet the many requirements of the electrical industry, insulating varnishes having widely different physical characteristics are necessary. It has not been possible to develop standard tests by which these physical qualities could be definitely measured, and it is necessary for the user to supplement the standard tests discussed above by others to determine whether the physical characteristics are suitable for the purpose intended. Generally, these tests are a practical application of the varnish to the apparatus for which it is being selected. If intended for use in making varnished cambric, the important physical characteristics would be elasticity and flexibility. The varnish could be applied to cloth and these characteristics noted. The ability of the varnish to withstand stretching without cracking could be determined by using bias-cut strips of treated cloth. For use on small high-speed armatures where toughness and binding and cementing qualities are most essential, the best test is of course the actual application to the apparatus, but this characteristic can also be observed by cutting or tearing a film of the varnish. For large form-wound coils, particularly those which may be held in stock a considerable time before use, a plastic type varnish is generally used. The varnish should soften slightly when heated so the coils can be formed to exact dimensions and be flexible so they can be readily assembled in the slots.

Only a few of the many uses of insulating varnish with the necessary qualities have been suggested. There are numerous others that require various combinations of qualities and each case will have to be considered individually and the characteristics of the varnish studied with reference to the requirements of the apparatus on which it is to be used.

These tests which have been passed in review will give some general idea of the choice of varnishes to meet the many requirements of the electrical industry. Most users, by a slight change in their processes, could employ one of a number of varnishes successfully, especially if they would give a little thought to the test results and comparisons. This is, of course, difficult without a basis of evaluation. Such a basis, however, has already been offered to the Society.[1]

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[1] J. A. Findley, "Numerical Tabulation and Comparison of Varnish Tests." Appendix, Report of Committee D-9 on Electrical Insulating Materials, *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 479 (1922).

CONCLUSIONS

The standard tests are of great value to both producers and users of insulating varnish, affording a common basis for discussion of the characteristics and merits of such materials.

The tests of the liquid material indicate the approximate composition, relative value and general working characteristics of the varnish.

The tests of the hardened films indicate the electrical characteristics and resistance to the various destructive agents.

These tests should be supplemented by other tests, preferably the actual application of the varnish to the apparatus on which it is to be used, to verify the suitability of the varnish for the purpose intended.

Qualities as measured by the standardized tests may be made the basis of specifications for varnishes.

SYMPOSIUM ON CONSISTENCY

INTRODUCTION: A DISCUSSION ON CONSISTENCY

By F. G. Breyer[1] and Henry Green[2]

The term "consistency," whenever used, refers to some property of a certain class of materials. Just what that property is and what class of materials possesses it forms the subject matter of the present paper.

The question which naturally arises first, when considering the nature of consistency, is whether the term is of general application or not. For instance, is it correct to speak of the consistency of the atmosphere or to refer to the consistency of a piece of oak? Strictly speaking it might be permissible to do so, but it is certainly neither customary nor necessary. The "consistency of the atmosphere" can mean but one thing and that is its viscosity, while the application of the term consistency to brittle solids, such as oak, is quite vague, and perhaps would generally be referred to some function of its tensile strength. In either case the extra term "consistency" would be superfluous. Furthermore, viscosity and tensile strength are accurately measurable and definitely understood quantities, while consistency cannot be measured and its exact meaning has not been decided upon.

The reason for this lack of definiteness becomes apparent upon inspection. On the one extreme are fluids, definable by their viscosity coefficients; on the other, are brittle substances possessing the strength that characterizes solids. Midway between is a third class that retains the attributes of both solids and fluids and as a consequence possesses the property of plasticity. It is to this intermediate class, where the terms "viscosity" and "tensile strength" are not readily applicable, that the expression "consistency" has been most generally applied.

As it happens, then, consistency must take into consideration two distinct and non-combinable attributes of matter; whereupon it ceases to be a measurable and comparable quantity, such as viscosity. That is, it can not be stated that one substance has, for instance, twice the consistency of another, though frequent attempts have been made to establish it upon such a basis. A lack of the realization of this complex nature of the term has obscured its meaning to such an extent that the general opinion prevails that consistency has no very definite meaning, and so each investigator is privileged to use it in a manner that will best suit his own purpose.

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[2] Research Division, New Jersey Zinc Co., Palmerton, Pa.

The authors believe that such a state of affairs need not exist. First, custom has restricted the application of consistency to that class of materials possessing plasticity. We have pointed out that it is neither advisable nor necessary to make the term of general application so that it covers the two remaining classes. Next it remains to ascertain, if possible, the particular property of plastics which the majority of investigators either knowingly or unconsciously have in mind when they employ the term.

The consistency of a material like paint, for instance, is judged by stirring, or by allowing it to run off the end of a spatula. When rubber or hot steel are broken down on a mill the pressure and work done are observed. What the investigator is actually doing by this procedure is nothing more than noting the resistance offered to flow. We believe that this exemplifies the general situation, and that the testing of any plastic substance for consistency, whether accomplished by hand or by some mechanical device, is essentially the same procedure, that is, an observance of the resistance offered to deformation or flow by the plastic material.

The resistance to flow is best analyzed by a study of the pressure - rate-of-flow curve. In the case of fluids, an analysis of that kind reveals the fact that resistance is defined by the viscosity coefficient; with plastics it is defined by the now well-established and easily comprehended quantities, yield-value and mobility.[1] These factors constitute the measurements of the two non-combinable attributes of matter to which reference has been made.

"Yield value" and "mobility" may be defined as follows:

Yield value is the minimum tangential force per unit area, that, when applied to a plastic material, is able to cause a lateral displacement of adjacent parts, that is, flow.

Mobility is analagous to the term "fluidity," the reciprocal of viscosity. It is applicable only to plastic materials. Its exact definition is as follows: Mobility is that area over which a unit of tangential force in excess of yield value, when applied, will cause a relative velocity of one centimeter per second between two parallel layers of the material one centimeter apart, the intervening space being filled with the material.

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[1] E. C. Bingham, "An Investigation of the Laws of Plastic Flow," U. S. Bureau of Standards, *Scientific Paper No. 278.*

E. C. Bingham and Henry Green, "Paint, A Plastic Material and not a Viscous Liquid; the Measurement of Its Mobility and Yield Value," *Proceedings*, Am. Soc. Testing Mats., Vol. XIX, Part II, p. 640 (1919).

Sufficient facts have now been brought forth to enable us to establish a definition of consistency:

Consistency is that property of a plastic material which it possesses by virtue of its yield value and mobility, and manifests itself as resistance to flow.

There is no unit of consistency, for it is a non-comparable term. At best it can be visualized, though with some difficulty, through a knowledge of the characteristic effects imparted to a material by yield value and mobility. An attempt to describe these effects briefly would be futile; they must be studied in the laboratory in order to be comprehended. The terms "plastic" and "flow" should be defined in order to eliminate the possibility of any misunderstanding arising in regard to the precise meaning of the consistency definition.

"Plastic" is used in the sense originally employed by Maxwell in his Theory of Heat and later developed by Bingham. The term is not restricted, therefore, to that class of commodities commonly referred to in industry as plastics, but includes such mobile materials as paint on the one hand and substances as firm as the malleable and ductile metals on the other.

Flow occurs in two well-recognized forms: turbulent and orderly. The latter type, when taking place through a tube, is often called "telescopic," and is the form that must be employed in determining yield value and mobility. The plastic factors, however, influence both kinds of flow, and so the consistency definition does not depend on whether flow is turbulent or orderly. The mere motion of a body through space in which no relative readjustment of its parts has been made during the act, does not constitute flow. Flow implies a relative lateral displacement of adjacent parts, and it is due to the opposing forces of displacement that yield value and mobility arise, thus creating consistency.

The nature and origin of the opposing forces is a matter of importance, but one to which comparatively little attention has been given. In a crystallin substance, strength to oppose an externally applied pressure originates in the forces tending to keep the atoms in a fixed and definite position. It is conceivable that such a material could be plastic, but the majority of crystallin substances are brittle solids and constitute a class which has not generally been associated with the term "consistency." In crystals where plasticity does occur, a second force of opposition arises *during flow*, caused by molecular interference.

It has been the authors' experience that those materials possessing the property of plasticity are invariably composed of two or more phases. With a plastic, such as paint or rubber, the individual particles of the solid or pigment phase are not deformed during flow. The force of atomic orientation, however, gives strength to the pigment particle which in turn must be imparted to the paint, thus influencing consistency. In other words, if the pigment was liquid and the interfacial surface tension zero, then the yield value would vanish and the paint cease to be plastic. All pigments possess such a high degree of solidity, however, that the paint or rubber technologist is not concerned with their tensile strength; therefore in practice this particular force of atomic orientation is of relatively little importance.

Of far greater import to the investigator is that force which causes the pigment particles to tend to cling together into groups after incorporation and dispersion in the vehicle. This force has been called the "force of flocculation" and its pronounced influence on both yield value and mobility, and therefore on consistency, has been published elsewhere by one of the authors and need not be entered into here.[1] Sulman[2] has ascribed the force of flocculation to interfacial tension. Whether his hypothesis is correct or not is a question irrelevant to the present discussion; at any rate it forms an interesting explanation and working basis for the solution of many problems arising in the field of plastics.

In addition to paint it has been suggested that surface tension materially influences the nature (consistency) of metals[3] and also of rubber.[4] Just how universally this idea will be applied to other substances cannot be stated at present, but the authors believe that continued investigation along this line will reveal that it is applicable to practically every form of material possessing plasticity.

Besides the forces of atomic orientation and surface tension, there is still another force to be considered as one of the controlling factors of consistency, and that is the resistance to flow produced by the interference of the units of the dispersed phase with themselves. Where the ratio of dispersed to continuous phase is high and close packing ensues as a consequence, the effect of this third force will appear perhaps in both yield value and mobility; but where the ratio is low, interference will simply lower the mobility and manifests itself in no other way. In regard to this phenomenon it should be noted here that E. C. Bingham originally attributed the yield value of clay-water mixture to the friction of the clay particles on one another.[1]

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[1] Henry Green, "Factors Governing Paint Consistency," *Journal, Industrial and Engineering Chemistry*, Vol. 15, No. 2 (1923).

[2] Sulman, "A Contribution to the Study of Flotation," Inst. of Mining and Metallurgy, *Bulletin*, 182 (1919).

[3] F. C. Thompson, "Surface Tension Effects in the Intercrystalline Cement in Metals and the Elastic Limit." *Journal*, Iron and Steel Inst., 1916.

[4] F. G. Breyer, "Zinc Oxide in a New Physical Condition and its Use in Rubber Compounding." Read before Pittsburgh Meeting, Am. Chem. Soc., 1922.

It is quite obvious that the other factor affecting consistency is the viscosity of the continuous phase.

Consistency has been qualified in several ways. For instance, certain paint pastes are referred to as having a "short" or "buttery" consistency. To this list might be added "gelatinous" consistency and perhaps even one more, "elastic" consistency. If these qualifying properties influence flow in any manner, they will be recorded in the yield value and mobility measurements; but in view of the great lack of experimental data it cannot be stated that these measurements are capable of an interpretation that will inform us as to the particular type of consistency with which we are dealing.

Very little can be said in regard to the methods for determining consistency, that is, yield value and mobility. For the paint investigator and the ceramist, the plastometer has been found satisfactory and serviceable and is now being developed for rubber. With rapidly setting materials, such as cements, the plastometer is perhaps too slow to yield results of value, and another method no doubt will be developed in time.

In considering metals, it should be pointed out that the stress-strain curve, though a function of resistance to flow, is derived from stretching, which does not necessitate a lateral displacement of parts in the same manner as occurs in the model of viscous flow and so the interpretation of results in terms of yield value and mobility is not an advisable procedure. It is quite possible that the plastometer will be developed to such a degree eventually that it will successfully handle metals, but whether it can be made a practical piece of apparatus or not remains to be seen.

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[1] E. C. Bingham, "An Investigation of the Laws of Plastic Flow," U. S. Bureau of Standards *Scientific Paper No. 278.*

E. C. Bingham and Henry Green, "Paint, A Plastic Material and not a Viscous Liquid; the Measurement of Its Mobility and Yield Value," *Proceedings*, Am. Soc. Testing Mats., Vol. XIX, Part II, p. 640 (1919).

SUMMARY

1. In this paper the authors have endeavored to show that the term "consistency" is most correctly used only when applied to materials possessing plasticity.

2. Consistency has been defined as a property of plastics depending on yield value and mobility.

3. The forces giving rise to consistency have been discussed.

4. Different types of consistency have been considered and also methods of measuring consistency.

RESUMÉ ON CONSISTENCY: NOMENCLATURE AND METHODS OF TESTING

PREPARED BY THE SUB-COMMITTEE, OF COMMITTEE E-1, ON CONSISTENCY, PLASTICITY, AND RELATED PROPERTIES

A. T. GOLDBECK, Chairman

INTRODUCTION

One of the important functions of Committee E-1 on Methods of Testing is to consider carefully all of the methods of test now standardized or about to be standardized by the Society, to insure the inclusion of all of the fundamental principles involved in these various standards. The Sub-Committee, of Committee E-1, on Consistency, Plasticity and Related Properties now has before it as a primary consideration the establishment of the principles underlying the measurement of these properties in order that the present or future specific tests for consistency and like properties might be judged or at least titled correctly in accordance with their agreement with the essential principles.

Differences of opinion exist among those having to do with the measurement of consistency, plasticity, viscosity, etc., as to just what are proper definitions for these terms and consequently as to what might be the proper principles underlying the physical measurement of these properties. It is hoped by the sub-committee that the present discussion will clarify the atmosphere and will lead to a sound basis for their future progress.

The terms "consistency," "plasticity," "workability," "viscosity," etc., have been used by various standing committees of the Society and these committees prepared standard methods for their measurement. Very naturally the standard in each case has been devised in such a way as to measure the desired properties of the particular material under consideration and the details of the test of necessity are arranged to suit the material.

What particular properties of the several materials did the standing committees have in mind in outlining their standard tests for consistency and related properties? A brief review of the consistency measurements used on various materials will serve as a means for direct comparison of these methods and to indicate what properties in common are being measured by the several tests and what are the essential properties which the tests aim to measure.

The following discussions have been prepared by a number of members of the Sub-Committee on Consistency, in all but one instance as representatives of various standing committees of the Society:

C-7 on Lime;
C-9 on Concrete and Concrete Aggregates;
C-11 on Gypsum;
D-2 on Petroleum Products and Lubricants;
D-4 on Road and Paving Materials;
D-8 on Waterproofing Materials;
Member of Advisory Committee of Committee E-1.

These discussions will, therefore, reflect the viewpoint of the committee responsible for the original preparation of the test method.

CONSISTENCY OF LIME PASTES AND MORTARS

By W. E. Emley,

Representing Committee C-7 on Lime

When speaking of the consistency of lime, the greatest difficulty is encountered in distinguishing between this term and the words "viscosity" and "plasticity."

A milk of lime, or cream of lime, is a suspension of lime in water, having a pouring consistency. When speaking of such suspension, it is possible that we should use the term "viscosity." At any rate, the yield point is low enough to be negligible. In the usual region of thicker consistencies, however, the yield point is no longer negligible, and it is obvious that here the term "viscosity" is not sufficiently descriptive.

Plasticity may be described as the ability of the lime to maintain its consistency under the conditions of use. Lime is used by application to absorptive surfaces. It is first applied at such consistency that it can be readily worked. The absorptive surface sucks water out of the lime, makes it of drier consistency and less workable. A lime which can maintain its water and consistency long enough to permit completion of the work is said to be plastic. From this definition, it is seen that plasticity is a property inherent in the lime itself and has nothing to do with consistency. It is true that if the initial consistency is unusually wet, a lime which is not plastic may be able to hold some of this excess water long enough to permit completion of the work, and to this extent plasticity seems to be dependent on consistency. Closer analysis, however, will show that this is a dangerous delusion.

Starting with the assumption that a thin milk of lime is practically a liquid, that is, that the yield point is negligible, and adding more and more lime it is evident that the consistency of the mixture becomes thicker. There is no evidence as to whether or not the added lime has changed the mobility of the mixture. There is direct evidence that it changes the yield point, for eventually this becomes greater than the force of gravity, as the mixture changes from a cream to a paste. From this point on, throughout the plastic range, it may be assumed that further additions of lime will increase the yield point of the mixture. The mobility is probably decreased at the same time.

Many experiments have been made by attempting to measure the force required to turn a paddle immersed in lime paste or the centrifugal force required to spread the paste over a revolving disk. Such experiments have been uniformly unsuccessful because the force required to start the motion is so much greater than that required to continue it, that we have thus far been unable to measure the latter. This would indicate that the yield point is of so much greater magnitude than the mobility that possibly the latter can be safely neglected.

It would seem then that a method for measuring the yield point might be used as a method for measuring consistency. The slump method contained in our present Tentative Specifications for Hydrated Lime for Structural Purposes (C 6 – 22 T)[1] may be considered in this light. If a cylinder of lime mortar 4 in. high is of such a consistency that it will slump to a height of 3½ in., it might be concluded that a 4-in. "hydraulic head" is sufficient to overcome the yield point, while a 3½-in. head is not. Unfortunately, this conclusion is faulty from both the theoretical and practical standpoints.

The practical difficulty arises from the fact that it is a physical impossibility to remove the mold from the cylinder without more or less distortion. This is aggravated by the tendency of the material to stick to the side of the mold.

The theoretical difficulties are many: The decrease of height of the cylinder must be accompanied by an increase of area. Increase of area can be accomplished only after overcoming the friction between the mortar and the plate on which it rests, hence the same mortar will give different readings when tested on a glass plate or a brass plate. The decrease in height involves motion, so that the momentum of the mortar would tend to carry it past the theoretical equilibrium. The active hydraulic head depends not only on the height, but also on the density of the mortar, and this density varies not only as between lime and cement, but also between different limes because of the varying amounts of water required to bring them to the same consistency.

The Vicat needle is similar to the slump test, and subject to some of the same criticisms, but it does eliminate the practical difficulties of the latter. When the needle sinks into the mortar, there are three forces acting against gravity, which tend to bring the needle to rest. One is the yield point which we are trying to measure. Another is the adhesion between the needle and the mortar, which increases as the needle sinks and larger surfaces come in contact. The third is the buoyancy exerted by the liquid mortar upon the solid needle immersed in it. Of course, momentum is a source of error here, as in the slump test, and, if the vessel containing the mortar is too narrow there may tbe some error due to the movement of the mortar along the sides of the vessel.

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[1] *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 691 (1922).

If the needle is of large diameter, the increase in the area of contact between it and the mortar, due to increase of penetration, will be proportionately less than if the needle is of small diameter. Consequently, the use of a large needle will tend to bring the adhesion between it and the mortar to a constant value.

The above law will evidently tend to stabilize the buoyancy also. However, here the density of the mortar must be considered, and it is evident that a heavy needle would be less affected by differences in the density of the mortar than would a light needle.

The needle should, therefore, be as large and heavy as possible. The size is limited by practical considerations of manipulation, and, the size having been determined upon, the weight must be such that the needle will penetrate the mortar without going clear to the bottom.

On the above principles, experiments conducted by Mr. J. W. Stockett of the National Lime Association, show that (1) a needle of $\frac{1}{2}$-in. diameter is practicable, and (2) with this diameter, the needle should weigh 30 g. when testing lime pastes, or 100 g. when testing lime mortars.

Such a needle will penetrate 20 mm. in a paste or mortar of good working consistency. It is quite accurate, being sensitive to less than 1-per-cent variation in the water content of the specimen. It is simple, portable, and the results can be readily checked.

Committee C-7 on Lime has, therefore, adopted this method for measuring consistency, and has defined "normal consistency" as that which permits the needle to penetrate 20 mm.

IMPORTANCE OF CONSISTENCY OF MIX IN CONCRETE WORK

BY DUFF A. ABRAMS,

REPRESENTING COMMITTEE C-9 ON CONCRETE AND CONCRETE AGGREGATES

The dictionary definitions of "consistency" and other terms which are used in this connection do not cover the requirements. The writer prefers the following definition of "consistency" as applied to concrete work:

> The relative plasticity or workability of freshly mixed material.

While it has been recognized for some time that the quantity of mixing water in concrete is an important factor, it is only due to extensive researches of the past seven or eight years that it has been definitely proved that the quantity of mixing water is probably the most important factor in a concrete mixture, assuming, of course, that the constituent materials are of good quality. The new feature developed by researches carried out by the writer is that the strength and other properties of the concrete are actually determined (other factors being equal) by the ratio of the quantity of water to the quantity of cement used, regardless of the reason for the change in the quantity of mixing water; that is, the quantity may be changed due to (1) variations and grading of the aggregates, (2) change in the quantity of cement, (3) change in the consistency of the concrete (or any combination of these) so long as the concrete is plastic. This has given rise to a factor which we call "water ratio" in dealing with concrete mixtures. This fundamental relation has been brought out by a number of independent series of tests carried out in the Structural Materials Research Laboratory, Lewis Institute, and has been verified by many tests made by other laboratories. These tests show the uniform relation obtained between strength and water ratio when all possible combinations of quantity of mixing water, size of aggregate and quantity of cement were used. In spite of the wide variations used in composition of the concrete, the test results conform to a uniform relation as expressed by an equation which contains two variables: the strength and the water ratio.

If the relation between the compressive strength and the quantity of water used is plotted, it is seen that the strength drops off very rapidly if too little water is used (as compared with that giving the maximum strength) and also if too much water is used. Ordinarily it is not practical to mix concrete of the consistency giving the *highest* strength, since the mixture would be somewhat harsh working under usual conditions. An increase of 25 per cent in the mixing water over that required for maximum strength reduces the strength to about 50 per cent of the highest value. Unfortunately in much concrete work, consistencies corresponding with strengths as low as 20 per cent of the possible strength are frequently used. It has been shown by experience that concrete of this type does not have the necessary resistance to weather and other destructive agencies.

CONSISTENCY OF CONCRETE

A complete analysis of the water requirements of concrete shows that the quantity of water required is governed by the following factors:

1. The condition of "plasticity" or "workability" of concrete which must be used—the relative consistency;
2. The normal consistency of the cement;
3. The size and grading of the aggregate;
4. The relative volumes of cement and aggregate—the mix;
5. The absorption of the aggregate;
6. The contained water in the aggregate;
7. The time of mixing.

While it will not be feasible to discuss each of the foregoing factors, it is apparent that a most complex relation is involved.

METHODS OF MEASURING CONSISTENCY OF CONCRETE

The measurement of consistency of a material like concrete offers a very complex problem, due to the extremely wide variety of types of materials used and to the almost endless variations and characteristics of these materials. Variations in consistency are generally made by changing the quantity of mixing water, although changes in quantity of cement, size and grading of aggregate, time of mixing, etc., also exert important influences. The water used in concrete serves two entirely different functions:

(*a*) Hydration of cement;
(*b*) Production of a plastic mixture.

It appears that the second requirement accounts for the bulk of the water actually used in concrete.

It is difficult to arrive at any theoretical basis of discussion of the consistency of concrete. The presence of an aggregate serves greatly to complicate the problem. Many different mechanical methods have been suggested and used for approximately determining this factor. The following methods have been tried at various times:

1. Penetration of metal rod;
2. Measurement of angle of repose;
3. Rate of flow down chutes;
4. Flow through orifices;
5. Slump test;
6. Flow table.

Penetration of Metal Rod.—A definite method is described for the determination of "normal consistency" of the cement paste in the Standard Specifications and Tests for Portland Cement (Serial Designation: C 9 - 21.)[1] This method of testing consistency of concrete was, no doubt, suggested by the use of the Vicat needle for determining the normal consistency of portland cement. A number of experimenters have tried the penetration method for concrete, but these attempts have in general proved unsatisfactory. The U. S. Bureau of Standards is now carrying out further work in which the penetration method is combined with the flow table described below. These studies promise to give valuable results.

Angle of Repose has been tried, but does not offer a satisfactory method.[2]

Flow Down Chutes.—A great many attempts have been made to study the consistency of concrete by measuring the rate of the flow down chutes. These trials have not developed a satisfactory method, due to the wide fluctuations in the observed rate under apparently uniform conditions. The problem is greatly complicated by the fact that slight variations in the type of material used in the chute, smoothness of surface, whether the chute is wet or dry, etc., exert a very important influence on the resulting rate.

Flow Through Orifices.—This method has been tried but in general has not proved satisfactory.

Slump Test.—The slump test consists of measuring the change in form produced by the action of gravity on plastic concrete upon the removal of the form in which the specimen is molded. The slump is measured in terms of the subsidence of the fresh concrete from its original height and is expressed in inches. This method probably dates back to the device invented and patented by Cloyd M. Chapman, primarily for studying the consistency of sand mortars.[1]

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[1] 1921 Book of A.S.T.M. Standards, p. 530.

[2] H. A. Thomas, "Concrete Consistency Measured by Simple Field Test," *Engineering-News Record*, Vol. 78, p. 244 (1917).

In dealing with concrete, two methods of making the slump test have been used:

(*a*) Cylindrical mold, 6 in. in diameter and 12 in. high.

(*b*) Truncated mold, 4-in. top diameter, 8-in. bottom diameter, and 12 in. high. This cone method is used in the A.S.T.M. Tentative Method of Test for Consistency of Portland-Cement Concrete for Pavements or for Pavement Base (D 138 - 22 T).[2] This method is now widely used in field tests.

Flow Table.—The use of the flow table as a measure of the consistency of concrete was developed by the U. S. Bureau of Standards, probably following the suggestion of Mr. Chapman for measurement of consistency of sand mortars. This method consists of a metal table attached to a frame in such a way that it can be raised and dropped by means of a cam. The test is made by puddling the freshly mixed concrete into the mold on the table, using a truncated cone having a top diameter of $6\frac{3}{4}$ in., a bottom diameter of 10 in., and a height of 5 in. After the mold is filled it is immediately withdrawn and the table raised and dropped $\frac{1}{2}$ in. 15 times in about 10 seconds. The base diameter of the mass of concrete after the test, expressed as a percentage of the original diameter, is the "flow."

IMPORTANCE OF CONSISTENCY IN RESEARCH WORK ON THE PROPERTIES OF CONCRETE

It will be seen from the foregoing discussion that the consistency is a fundamental requirement of concrete mixtures and that any variation in the proportions, characteristics of the aggregates, the method of manipulation, etc., is reflected at once in the resulting consistency of the mix. The correct determination then of the consistency becomes a condition precedent to proper testing and is absolutely the most important feature of concrete investigations. The effect of other variables on the quality of the concrete may be entirely masked by what may appear to be slight variations in consistency. The importance of an accurate method of control of consistency for laboratory work is at once evident. Unfortunately we do not have an entirely satisfactory method at this time. The flow table furnishes the most satisfactory device which has thus far been developed.

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[1] C. M. Chapman, "Method and Apparatus for Determining Consistency," *Proceedings*, Am. Soc. Testing Mats., Vol. XIII, p. 1045 (1913).

[2] *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 805 (1922).

FIELD CONTROL OF CONSISTENCY

The growing recognition of the importance of the control of consistency of concrete and the realization of the poor results which may be secured from using too much mixing water are gradually leading to greater care in field control of the consistency. The use of concrete in road construction has probably done more than any other one thing to call the attention of engineers and contractors to the extreme importance of restricting the quantity of mixing water. In accordance with the latest information available, 17 of the 41 state highway departments which have published specifications for concrete road construction require some form of a slump test of the concrete used in the wearing surface of a road. The exact slump permitted varies somewhat with the method of finishing used and with other factors.

The Progress Report of the Joint Committee on Standard Specifications for Concrete and Reinforced Concrete[1] suggests the use of concrete with a slump of 1 to 8 in., depending on the nature of the work. For the usual reinforced concrete building construction, concrete with a slump of 6 to 8 in. is about the driest consistency that can be used.

It should be understood that a field test for consistency should not be too strictly interpreted. For example, if the specifications call for a slump of 6 in., a batch of concrete should not be rejected if the slump is found to be 7 or 8 in.

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[1] *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 212 (1921).

NOTES ON GYPSUM MORTARS AND MORTAR CONSISTENCY

By H. E. Brookby,

Representing Committee C-11 on Gypsum

Committee C-11 is concerned with the use of plaster of Paris and water, a mixture of a solid and a liquid. The scope of this use is confined quite largely to the realm of mortars—trowelling and casting mortars. Therefore, the first step in approaching a definition of consistency as applied to mortars would logically be to analyze the so-called "mortar range" in which as a committee we are most interested.

Gypsum mortar is made up of water and powdered solid in such proportion as to be trowelled, spread, modeled, cast or worked out into definite position. It requires the application of force other than gravity to manipulate it.

Thus with water at one end of our scale and solid gypsum at the other, we start with a liquid of definite mobility at the one end of the scale, and work through the properties of liquid into the combination or resultant of properties of a liquid and solid in homogeneous mixture, and finally, in the set gypsum mortar at the other end of our scale, into the properties of the solid alone. Therefore, it will be advisable to consider in detail some established properties of the liquid, or fluid, water, and of the solid.

Properties of the Liquid (Water)

1. *Fluidity.*—A fluid has particles which easily move and change their relative position without separation in the mass of liquid. Fluidity, therefore, is that property of a substance that permits it to yield to any force, however small, that tends to change its form. Liquids are inelastic or incompressible fluids, while gases are elastic or compressible fluids. Under fluidity there are two component properties worthy of special mention in our discussion:

(*a*) Mobility.—This can be defined as that degree of fluidity in a liquid that permits of rapid change of place, or ease of setting in motion. Flowability, as far as I can ascertain, is not an established term.

(*b*) Viscosity.—This can be defined as the resistance offered by a liquid to the relative motion of its particles. It may be further defined as the internal friction of a liquid, and thus the property that tends to limit mobility.

2. *Cohesion.*—The force by which particles are held together in a mass, so that the mass resists being pulled to pieces, is called cohesion. It is exerted at infinitesimal or small distances, while gravity is exerted at infinite or great distances. Therefore, cohesion is that form of attraction by which particles of a body are united throughout the mass, whether such particles are like or unlike. I would further qualify its definition by saying that cohesion is that form of attraction whereby unlike particles in a mixture are aggregated or held in a united state. Thus, it is specifically defined as the attraction of aggregates, as differentiated from the attraction of gravity. Adhesion denotes a mere cleaving of one object to another, and supplies little or nothing as to resulting unity. For example, a stamp adheres to an envelope. Cohesion suggests a much more unified mass.

3. *Density.*—The weight of a unit volume of the mass divided by the acceleration of gravity. Density in this instance relates to compactness.

PROPERTIES OF THE SOLID

1. *Hardness.*—Hardness is that property of a mass of matter by which it is solid and firm in outline. It is interpreted by its capacity to receive surface indentation. Under this division of properties of solids, there are two sub-divisions, which should be considered since they bear something of the same relation to hardness as the subdivisions under "fluidity" due to that property.

> (*a*) Solubility.—The resistance offered by a mass of matter to the relative motion of its particles when in contact with a liquid. Of course, it can be argued that liquids possess solubility, and are also miscible, but in this paper discussion of solute and solvent is avoided. We are discussing calcined gypsum and water.
>
> (*b*) Flowage.—This may be defined as that property of slow continual yielding through change of shape, under the action of continued stress, with deformation through molecular shear. Simpler, flowage is mass deformation through intermolecular shear, as distinguished from fracture. This property is analogous to what is termed "ductility".

2. *Cohesion.*—Cohesion can be defined under properties of the solid exactly as under properties of the liquid, and will not be given again.

3. *Density.*—Density can be defined under properties of the solid identically with its definition under properties of the liquid.

It will be noted by an analysis of the elemental properties of matter set forth, that each of the two substances under consideration have two properties in common, and each has a property not only different but in a sense directly opposed to the other. Further, the term "plasticity" has not been mentioned as a fundamental property of either, for a very good reason—I could not make any accepted definition fit either the solid or liquid alone, without introducing a time effect or a temperature effect. Thus, this property seemed to be out of the realm of fundamental properties.

Under certain temperatures, liquids begin to acquire plastic properties, as also do solids. Under diminishing temperature, the liquid has its property of fluidity diminished by the loss of mobility and by increase of viscosity, while under augmented temperature, the solid tends to lose its hardness through increased flowage, or by the acquisition of some degree of fluidity, or by its solubility in liquid coming into play. For instance, slush ice or snow is an example of the liquid losing fluidity while the glacier flowing down the mountain side and ending in a torrent is an example of the solid acquiring fluidity.

From my viewpoint, when internal friction or viscosity increases, mobility diminishes. And when mobility has reached the vanishing point, the property of fluidity is greatly lessened but still may have a positive value. Past this point, we come to that no man's land which we all recognize but find it hard to discuss, but which we know, however, is the realm of plasticity.

Plasticity is that property of substance through which the form or outline of a mass is readily changed by application of force in addition to gravity. Analysis of this definition reveals that its weak point is in the use of the word "readily." This vagueness in time effect can be obviated if expressed differently as: Plasticity is that property of substance, whereby its "yield value," or the measure of the internal force required to start change in form or outline of the mass, is greater than gravity. In other words, a plastic substance cannot flow by gravity—it requires additional force to change its shape or form. Then to measure plasticity, we must determine the force in addition to gravity necessary to readily deform the mass—or the "yield" value during a definite time element of application of sufficient force to deform, when continuously applied.

Now Committee C-11 has used the term "consistency," both "normal" and "testing," and the gypsum industries and building trades recognize another, perhaps more important term, "mortar consistency"; so the committee use of the word "consistency" has undoubtedly arisen from its desire to catch hold of this elusive term, and so define it that it can be determined by testing methods and thus put mortar itself, as far as this committee is concerned, into a definite niche where it can be identified and its identification can be proved scientifically. If we turn back and classify the properties of the solid and liquid, we find only two in common: first, cohesion and second, density. In this discussion, we are not concerned with density, which leaves us cohesion only. From our definition of cohesion it is quite evident that the mortar range in which we are interested is dependent in some way upon this property of cohesion, since each component of the mortar possesses it. It is also quite logical to assume that in defining consistency, some reference to cohesion will possibly be made, for a mortar to be mortar and perform the functions of mortar must be in some state of cohesion. Thus, consistency, or "consistence" as the dictionaries put it, may be defined as "standing together or firmly united, not loose or fluid." This definition, of course, does not satisfy because it is only relative in scope and must be further qualified by some kind of consistency or some degree of consistency. We refer to a substance as having the consistency of butter, but we cannot define this consistency of butter except in relation to something else. We all, nevertheless, think of butter in about the same relative stage of consistency, so then it has come to be accepted that there is a butter consistency, which would be associated with butter but which, however, might not be associated with cheese or cream. In other words, there is a normal or standard or accepted consistency for butter, and likewise consistencies for other substances.

Therefore, we must define "mortar consistency" rather than a mere consistency, "testing consistency" as a definite consistency, and if necessary "normal" or "standard consistency." Consequently, by continual analysis and classification of the factors entering into our mortar range, we can build up these definitions rather than state them loosely and the synthesis of these definitions can be followed logically.

"Mortar consistency," therefore, is that range of consistency in a mixture of positive fluidity, whereby cohesion is maintained and gravity does not destroy plasticity. In other words, while a mortar mass possesses fluidity, the mobility of the liquid member has diminished to the vanishing point, and gravity is unable to cause any separation of liquid from solid in the mortar mass which would thus destroy the plastic properties of the mass. The mortar is a homogeneous mixture and stays so.

"Testing consistency" would be that particular designated consistency in the range of cohesive mortars of measurable plasticity whereby the effect of gravity on fluidity is measured by a designated method. An example of this would be the slump test, where the slump is an effect of gravity caused by some portion of the cylindrical mass height. The diameter of a patty is an effect of gravity, measurable in another way.

COMMENTS ON THE MEASUREMENT OF CONSISTENCY, WITH PARTICULAR APPLICATION TO GREASES AND PETROLATUM

By R. E. Wilson and F. R. Baxter for K. G. MacKenzie,

Representing Committee D-2 on Petroleum Products and Lubricants

The "consistency" of plastic materials, such as grease, involves four major factors. The following might be suggested as suitable names and *qualitative* definitions of these four factors:

1. *Yield Point.*—The shearing force which must be applied to a plastic solid before any flow begins.
2. *Mobility.*—The ratio between flow and shearing force above that required to start flow.
3. *Elasticity.*—The ability of a body to absorb energy and give it back elastically instead of consuming it entirely in flowing.
4. *Texture.*—Determined by the presence, or absence, of a cellular or fibrous structure, and the size and comparative properties of the cells and cell walls or fibers.

The first two of these factors are, of course, those pointed out by E. C. Bingham in his book on "Plasticity and Fluidity" and the third is also one which E. C. Bingham has mentioned in a recent paper and is now studying intensively.

Using only the range of materials commonly employed in making commercial greases, it is possible to make very wide variations in any one of these factors without greatly changing the others, and in order to make certain of securing a grease of absolutely definite properties *by physical tests alone*, it would be necessary to make a quantitative measure of all four of these factors and possibly of still others. This would require highly specialized apparatus and skilled operators, and does not appear to be practicable under present conditions.

The second possible way to secure a grease of specified properties is to first divide greases into certain classes where the composition and method of preparation are in general similar, and then to use simple tests to detect the variations to be anticipated within the range covered by a general class. Thus, we may take cup greases as a class, defining them either by their composition or by generally accepted trade understanding. These greases are all smooth in consistency and possess very little elasticity. Furthermore, their yield point goes up steadily as their mobility goes down so that it is probably quite feasible to apply only a single test, measuring either the force required to give a certain flow above the yield point, or the flow produced by a given force. A special case of the latter method is the measurement of the penetration of a cone of definite weight released with the tip in contact with the top surface of the grease, and such a test seems to be quite feasible for this class of greases.

If this same test were applied to another class of greases, such as sponge greases, exactly the same penetration might be given by two greases, one cup and one sponge, which differed enormously in properties—the sponge grease possessing the property of elasticity to a considerable extent and having a more fibrous structure. Even if the yield point and mobility of the greases were measured separately the latter two factors would constitute an obvious difference between these two types. It is quite probable, however, that the penetration test as applied to sponge greases would be quite satisfactory in distinguishing different grades of this type, although it might be found advisable to include a measurement of elasticity if this property were found to be important in itself.

The distinction between "long" and "short" greases could probably be made adequately by separating the two factors of yield point and mobility. The property of "tackiness" is probably dependent to a considerable extent upon elasticity, while "stringiness" is probably more a question of texture.

In 1919, the late Thomas O. Organ was consulting engineer for the Keystone Lubricating Co., and was also chairman of Sub-Committee IV on Grease, of Committee D-2. He submitted to some of the members of his sub-committee the problem of measuring the consistency of greases, and suggested the use of a type of apparatus somewhat similar to that devised by Kissling, but in place of the aluminum needle he used a cylinder open at both ends made from a piece of brass tubing. The upper end of the tubing was attached to a rod graduated in 0.01 in. by means of a narrow crosspiece. The upper end of the rod was prepared to receive weights of various sizes. The upper guide for this graduated rod was an arm attached to a vertical rod fastened to a base which could be levelled with ordinary leveling screws. In other words, the apparatus was similar to a penetrometer except that the needle was replaced with the cylinder. For the soft greases, and up to the No. 2 grade, the container for the grease under inspection was a 1-in. cylinder, $1\frac{1}{2}$ in. high, and for the harder greases a 2-in. cylinder, 2 in. high. After securing the samples, the containers were placed on the base of the testing device under the cylindrical needle, the surface smoothed with a spatula, the plunger weighted, and after the cylinder had been placed in contact with the upper surface of the grease, a stop-watch was started at exactly the instant of release of the needle and the distance of penetration for 30 seconds noted. The results were computed from the formula:

$$\frac{\text{Weight} \times \text{distance}}{\text{Time}} = \text{Coefficient}$$

A number of tests were carried out on greases with this apparatus. The figures were found to be directive, but did not give all the information desired. A better type of plunger, one that can be readily and completely cleaned, or possibly the cone-shaped one previously mentioned is more desirable. Mr. Organ's formula was expected to equalize the results, and it may be found on further experimentation that there is a relation between the weight, size of needle, and consistency of grease that can be determined.

While petrolatums have usually been sold on melting points, there has arisen a need for measuring the consistency of these bodies rather apart from the melting point, and which more particularly approaches viscosity. The old method of testing this characteristic with the finger was efficient enough at that time, but the extended use of petrolatum calls for some more refined method.

For purposes of information the same apparatus mentioned above has recently been used in the testing of samples of commercial grades of petrolatum, using two methods and two different weights, in connection with the work of Sub-Committee I on Petrolatum, of Committee D-2. These tests were all made at 25° C. (77° F.), the time being 30 seconds. In one group, the petrolatum was melted, poured into the 2-in. cup and chilled, the top being smoothed off with a spatula. Another group of petrolatums was taken from larger containers and forced into the cup with a spatula, the surface afterward being smoothed with the same instrument and the test applied. After each test in both methods, the surface of the petrolatum was again smoothed with the spatula and the cylinder also carefully cleaned. It is quite evident that a 50-g. weight is too light, and even with the 70-g. weight the travel of 0.1 in. on the stiffer grade and 1 in. on the softer is not much of a travel. The tests show that for the proper valuation of a petrolatum, tests other than plasticity or viscosity may be required. It may also be found that the Kunkler viscometer for greases, described in Redwood's Treatise on Petroleum, in which the sample is placed in a tube which can be heated in a water bath and there held at any required temperature, may yet be found more desirable in obtaining consistent results than any of the other forms proposed.

CONSISTENCY AS APPLIED TO ROAD MATERIALS

By A. N. Johnson,

Representing Committee D-4 on Road and Paving Materials

I understand from the A.S.T.M. definition for consistency, as relating to tars and pitches,[1] that consistency is "the degree of solidity or fluidity of bituminous materials." No definition of consistency applying to non-bituminous materials is given. In the Standard Specifications and Tests for Portland Cement (C 9 – 21) of the Society[2] is given the method of determining normal consistency for cement paste:

> "The paste shall be of normal consistency when a plunger settles a given amount in a given time, the results being construed to indicate the amount of water necessary to add to the cement to indicate the character of normal consistency."

In the Standard Specifications for Materials for Cement Grout Filler for Brick and Stone Block Pavements (D 57 – 20) of the Society,[2] reference is made, under Mortar Strength Test, that briquettes shall be made of mortar of the same "consistency," without further indications as to what is meant specifically by consistency.

In the Standard Method of Test for Penetration of Bituminous Materials (D 5 – 21) of the Society[2] occurs the following:

> "Penetration is defined as the consistency of a bituminous material, expressed as the distance that a standard needle vertically penetrates a sample of the material under known conditions of loading, time and temperature."

This method of determination of consistency is seen to be similar in character to that given for consistency of cement paste. In concrete mixtures, consistency is a commonly used term modified often by the word "workable" consistency, and in all cases refers to a condition of the concrete that varies according to the amount of water added. Specifications may define consistency as "jelly-like" or "just quaking." In a number of specifications, the relative consistency is measured by the amount of slump or settlement observed in a mass of freshly mixed concrete upon the removal of the container or mold. This may be designated as the gravity method, as distinguished from the plunger method. Consistency of concrete has also been measured by observing the spread of the mass of freshly-mixed concrete, placed upon a horizontal surface which is agitated in a given manner for a given time. This, perhaps, might be classed as an accelerated gravity method.

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[1] Standard Definitions of Terms Relating to Materials for Roads and Pavements (D 8 – 18), 1921 Book of A.S.T.M. Standards.

[2] 1921 Book of A.S.T.M. Standards.

In Webster, we have the definition of consistency as "condition of remaining at rest or quiescent"—this definition being indicated as obsolete; second, "condition of standing or adhering together, or being fixed in union, as the parts of a body; existence; firmness; coherence; solidity." Bacon uses the term as follows: "Water, being divided, maketh many circles, till it restore itself to a natural consistence." Also, "a degree of firmness, density or spissitude; thus, the consistence of a sirup."

We thus see "consistency" commonly used as a generic term. In general, the word "consistency" indicates a condition to which a material, or mixture of materials, may be brought and does not indicate any inherent property of the substance or substances involved. This in contradistinction to the terms "viscosity" and "plasticity," which express definite properties.

A definition of consistency, or, more properly, a specification for consistency, would depend upon the purpose of determining this phenomenon. Thus, as applying to the consistency of concrete, experimental data have shown that the essential element is the amount of water. It becomes then a matter of experimentation to determine if the method to be specified determines accurately what it is desired to ascertain.

We have an illustration of a different purpose in the case of bituminous materials, where we ascertain the consistency by the penetration of a needle. If experiment has proved and demonstrated that the relative qualities of bituminous compounds are accurately determined in this manner, we shall then have a specification for the consistency of those certain materials.

The word "consistency" is a word of such common usage that it would not seem to be desirable to employ a narrow physical definition, as describing quantitatively any exact property of matter as has been suggested.

No ambiguity arises where, in the generic use of the word, consistency is applied to a single material, if the precise method by which we determine the condition of consistency of the material is outlined. It would then be necessary only to determine that the method for observing the consistency for a given substance or mixture actually gives relatively true indications of the value of the material for the purpose for which it is to be used.

CONSISTENCY

BY PRÉVOST HUBBARD,

REPRESENTING COMMITTEE D-8 ON WATERPROOFING MATERIALS

Committee D-8 has adopted no definition for consistency nor has it recommended methods of test which are considered to cover the property of consistency. It has, however, adopted certain methods put forward by Committee D-4 on Road and Paving Materials which include consistency tests and definitions.

In reviewing the data so far assembled upon the subject of consistency the definition as relating to tars and pitches put forward by Committee D-4[1] would seem to the writer to express the general conception of the term consistency more closely than any other. According to this definition consistency is "The degree of solidity or fluidity of bituminous materials." This definition is limited to apply to bituminous materials but would appear susceptible to rewording and explanation to make it more widely applicable.

The writer therefore offers the following definition and explanation as a basis for discussion:

"Consistency is the degree of solidity or fluidity of a substance."

The term "consistency" covers those characteristics of materials which may be measured by their resistance to deformation or flow without structural change or failure and includes such properties as penetrability, plasticity, workability, cohesiveness and viscosity. The properties of hardness, toughness, brittleness, adhesiveness, ductility, malleability, pliability, compressibility, elasticity, density, texture, etc., are descriptive of various phases of consistency, but unless measured in such manner as to indicate resistance to deformation or flow they do not come within the general meaning of the term.

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[1] Standard Definitions of Terms Relating to Materials for Roads and Pavements (D 8 – 18), 1921 Book of A.S.T.M. Standards.

CONSISTENCY

By A. W. Dow,

Member-at-Large of Committee E-1

Consistency.—Consistency is that property of a liquid which causes it to resist deformation under pressure.

The word "liquid" in this definition is used in a broad sense, covering all materials, excepting gases, that will flow under pressure. This, of course, would include any solid that can be deformed under pressure and retain its new form. To explain this by example, I would say that lead has a consistency because when pressed out of shape it will not come back to its original form, whereas rubber will spring back to very nearly its original form. Of course, rubber and like materials will deform under pressure and the extent of this deformation is its consistency.

In using the word "pressure" in the above definition I intend to include even the slightest pressure, such as that caused by the attraction of gravitation. The forces in a liquid (used in the above broad sense) which cause it to resist deformation are very likely the same in all cases, that is, considering as liquids, true solutions, colloidal solutions, complex solutions, solutions in other various forms, emulsions and suspensions. In the case of colloidal solutions, emulsions and suspensions, there is no doubt whatever but that interfacial tension between the particles is the primary force governing their consistency. It is reasonable, therefore, as there is no real dividing line between colloidal solutions and true solutions, to consider that even in true solutions the consistency is dependent on the interfacial tension of the molecules. There are various means by which consistency is measured; among the common might be mentioned viscosity of liquids, penetration of semi-solids, and the force required to stretch metal to its elastic limit. The slump test on concrete mixtures is a test of consistency, for in this case gravity is the acting force and the degree to which the material slumps is the measuring factor.

DISCUSSION

Mr. Breyer

Mr. F. G. Breyer.—We test plastics, not for the sake of testing, but to find out how to make them do the things we want them to do. It is only through the development of a theory from which subsequent action may be taken that progress can be made in making plastics do service to mankind. The theory suggested in our paper is very important to my mind—more important than the actual facts that have been developed so far. Plastic materials have a certain resistance to the start of flow, and then offer further resistance after flow is established at a given rate. Now what is the force that resists the start of flow, and what is the force that resists flow after it is established? If we can find that out, we have something. It is established in the case of paint and rubber very definitely and very strongly indicated in the case of metals, that much of the force that is resisting this flow, especially the force that is resisting the start of flow, is the force that has been put into that mass as the consequence of a two-phase system and that force is nothing more or less than our old friend, surface tension. The stiffness of any two-phase system is proportional among other things to the surface of the discontinuous phase or phases and the degree of wetting. It is our theory that the degree of actual chemical union between the one phase and the other determines the amount of that surface energy, and anything that we can put in the continuous phase that will increase the chemical attack on the discontinuous phase (that is, increase the wetting power) will lower the surface tension of the continuous phase and cause a decrease in the resistance to initial flow. Likewise, anything we can put in that will go into or condense on the discontinuous phase—the dispersed phase—which will affect the solubility of it in the continuous phase affects surface energy and hence the consistency. Now that, as I say, applies quite definitely to paint and rubber, and the analogy to metals is quite striking. In the case of metals, the continuous phase is the intercrystallin material; the discontinuous phase is the crystal mass. Alloying elements that dissolve in the crystals have a different effect than those that are soluble only in the intercrystallin material. Stiffening by cold working resolves itself simply into an increase of surface energy due to its decreasing the crystal size with a consequent increase in crystal surface.

Mr. Emley

MR. W. E. EMLEY.[1]—We have at present in our tentative specifications for different kinds of gypsum, an instrument known as the Southard viscosimeter for consistency measurement. That name was given it some years ago before we recognized the distinction between these different terms. That is, in effect, a slump test, except that it works the other way; the material is put into a well and the bottom of the well is pushed up. More recently, however, a more refined means of making a slump test has been developed, I believe largely through the efforts of Mr. Brookby, and a very nice machine for that purpose has been made. It is a machine that does away with a great many of the difficulties of making slump tests, especially when we are testing pastes and mortars.

There are greater objections to using the slump test for mortars than for concrete, because the specimens of pastes and mortars are smaller, and the results are not so apt to be accurate with the smaller specimens as with the larger ones. When the mold is lifted off the specimen, the specimen is apt to be distorted. There is a time factor involved in lifting the mold; it is not done at the same rate always, especially by different operators. The new machine does all this automatically. All that is necessary is to put the material in the mold and press a button, so all the human variables are eliminated. It seems to me very much better than the Southard machine, from what we know at present.

Mr. Bingham

MR. E. C. BINGHAM[2] (*presented in written form*).—To say that a certain building is a "40 house" is not very helpful because there are so many different ways in which houses may be measured. It may occupy 40 front feet, be 40 ft. high, have 40 rooms, or cost 40 thousand dollars. Or again the 40 may be an abstract number, as, for example, the location on a street. So "consistency" is at present a loose term which has a variety of meanings. We are now trying to frame a definition which will (*a*) be as informative as possible and at the same time (*b*) be accepted generally and, if possible, (*c*) give us definite, that is, quantitative information. We shall be compelled to abandon the attempt unless there is some fundamental property which is not already clearly defined, for it is a mistake to deliberately bring about the duplication of terms. This, of course, means that the term "consistency" in this eventuality would have to be abandoned for the more accurately descriptive terms already in use. On the other hand, we must do no violence to the traditional, even though somewhat vague conception of the term, as it is now used.

Mr. Bingham

We are probably all agreed that viscosity and its reciprocal, fluidity, are fundamental in our conception of the flow of all fluids. Perhaps we will also agree that plasticity is a property confined to solids, that it is complex in character, being made up of the two fundamental properties "yield value" and "mobility." The mobility is strictly analogous to the fluidity of fluids but in the yield value, or friction, we have a new fundamental property. These two properties of solids are sufficient to define the flow characteristics of inelastic solids. In elastic solids we must take into consideration the elasticity and its reciprocal term "rigidity."

We have at present no name for the property which is the reciprocal of the mobility. This property may be defined as *that property of a solid which causes it to resist continuous rapid deformation*, and we believe that this definition accords very closely to the meaning usually accorded to the term "consistency." If this definition of consistency were adopted, it would have a desirable analogy to viscosity in fluids and to rigidity in solids, within their elastic limit. Consistency would then be quantitatively defined as follows: *Consistency is the shearing stress in dynes per square centimeter in excess of that stress required to start continuous deformation, required to give either of two parallel planes one centimeter apart, a velocity of one centimeter per second in reference to the other, the space between the planes being filled with the plastic material.*[1]

By this definition, the term "consistency" fills a need which has already been felt, no violence is done to current usage, and a loose conception is given a meaning which can readily be expressed numerically and the term "consistency" at once relates itself naturally to our other nomenclature, particularly plasticity, viscosity, and mobility. Undoubtedly, if consistency is not allowed to fill this natural place for it, some other term will have to be invented to represent the property involved.

The objection will at once be raised that the yield value also helps to oppose deformation in solids. This is, of course, true but we are only concerned with the definition of fundamental properties. When this task has been accomplished it will be a very simple matter to calculate the force which opposes the deformation in any given case. Were we to call this resistance the consistency, the consistency would not be a constant of a given material but would depend entirely upon the shearing stress used. In other words, the term would be superfluous. **Mr. Bingham**

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[1] In the paper by Booge, Bingham and Bruce on "Relation of Yield Value and Mobility of Paints to Their So-Called Painting Consistency," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 420 (1922), it was observed that paints of painting consistency have usually nearly the same yield value. The consistency as noted by technical men was principally dependent upon the mobility, which falls in line with the above definition.

To make these ideas still more definite, let us now express them in mathematical language. Let us consider a small parallelopiped filled with material subjected to the shearing stress F, the thickness of the parallelopiped being r. Several things may happen. In the first place, if the material is elastic, shear will immediately take place and one surface will be displaced by a distance s in reference to the other, and according to Hooke's law of elasticity

$$s = e\,F\,r \qquad \text{(1)}$$

where e is the coefficient of elasticity of the material. If the shearing stress is removed, the material will spring back to the former shape so long as we remain within the limit of perfect elasticity. In elasticity the time factor does not enter. This formula applies to all solids and, we believe, to many liquids.

But in many cases the deformation continues to increase with the time, and the velocity at which one surface is sheared over the other is found sooner or later to be constant, $s/t = v$. We have now the fundamental law of viscous flow

$$v = \phi\,F\,r \qquad \text{(2)}$$

where ϕ is the fluidity. This applies to all fluids but to no solids. The energy lost in viscous flow is a function of the time, which is in sharp contrast to elastic deformation.

A further possibility exists that beyond the immediate elastic deformation no further deformation will take place unless the shearing stress exceeds a certain value, called the "yield value," but that then the material will be deformed continuously according to the fundamental law of plastic flow

$$v = \mu\,(F - f)\,r \qquad \text{(3)}$$

This formula applies to all solids. And here we have the true distinction between a solid and a liquid, for a solid becomes a liquid as soon as the yield value becomes zero.

Omitting here for the sake of brevity combinations of the kinds of deformation which we have exemplified in the "elastic after effect," we have finally for the sake of completeness the case of a solid which suffers no further deformation after the immediate elastic deformation until the shearing stress reaches the point of rupture. This shearing stress at which the rupture takes place is evidently a measure of the strength of the material, ϑ.

We have then as independent or dependent properties recognized up to the present: $e = 1/n$ where n is the rigidity, $\phi = 1/\eta$ where η is

Mr. Bingham the viscosity, $\mu = 1/\zeta$ where ζ is the consistency. There remain f and ϑ for which no reciprocal relations appear to be needed at present.

All of the operations for the measurement of plasticity, viscosity, hardness testing, etc., regardless of their outward form, involve the above conceptions derived from the elemental parallelopiped. Many industrial operations, whether stretching, drawing, punching or pouring or bending are related to them no less intimately.

We have thus far measured plasticity crudely by watching a pile of the material slump down under the shearing stresses set up by the action of gravity or by observing the appearance of a material as it flows off from a spatula, but even if the values obtained are inexact from a scientific point of view, there is every reason that we should know whether these values are simply related to properties of a fundamental character. In other words, our tests of flow must differentiate yield value, mobility, fluidity, elasticity from each other because these are the fundamental properties of flow. If our tests are not sufficiently crucial to do this, they should be abandoned as soon as possible in favor of tests which will give more indubitable evidence of what we wish to know.

In measuring the plasticity of a substance like hydrated lime, it is, of course, of no interest from the scientific point of view that the material is often applied to porous surfaces which will cause the plasticity to be subject to change. The rate at which water will be absorbed is a separate question presumably dependent upon the fluidity of the water, the size of the pores, and the surface tension of the water. If the two properties of plasticity and water absorption are dependent upon different variables or even upon different functions of the same variables, is there not a positive loss, with confusion of our ideas, in attempting to represent them under the single term of "consistency"?

A final word now in regard to the word "rapid" in our rough definition of consistency, will not be out of place. When the shearing stress is large enough so that the deformation is rapid, the yield value is relatively unimportant and the resistance to deformation is principally due to what we believe should be called the "consistency" of the material. In many of our practical tests such as in stirring or allowing a material to run from a spatula, the deformation is undoubtedly rapid. However this may be, it is to be noted that we will never be under the necessity of defining the word "rapid" in this connection, since consistency is accurately defined above without its use.

Mr. Herschel MR. WINSLOW H. HERSCHEL[1] (*presented in written form*).—It is ordinarily assumed that there are three states of matter, gaseous, liquid and solid. There is no question in regard to the consistency of gases since they are all compressible and viscous. Crystallin solids may also be left out of consideration. **Mr. Herschel**

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[1] Associate Physicist, U. S. Bureau of Standards, Washington, D. C.

According to Breyer and Green, "Custom has restricted the application of consistency to that class of materials possessing plasticity." It is difficult to prove or disprove what custom has decreed unless some particular time and place are referred to. Turning first to the Bible, we find that there is a clear idea that wet clay may be molded, or as we would say, is plastic. One passage speaks of making clay, implying that the word "clay" should only be used when the material is wet.[1] Bacon[2] used consistency in a very broad sense, saying, "The consistencies of bodies are diverse, dense, rare, tangible, pneumatical, volatile, etc." It is natural to use words vaguely at first and afterwards to restrict their meaning, but I believe it would be going too far to apply the word "consistency" only to plastic materials.

Breyer and Green propose to exclude from plastic materials, all viscous liquids. Brittle substances are excluded, but malleable and ductile metals are included, and this leaves it in doubt where to place metals like steel which may be either brittle or ductile to almost any degree. In the case of varnishes, also, some are viscous and some plastic, and it is convenient to speak of testing the consistency before the result of experiment has revealed which type of varnish is at hand.

Unfortunately "viscosity" is sometimes used as the general term, mainly by those who do not realize that some apparent liquids are plastic. "Viscosity" is a much newer term than "consistency" and can not be justified from custom or usage in non-technical circles. The man in the street may know what is meant by "slower than cold molasses," and he may say that one oil is thicker, or heavier than another, but he is not familiar with the word "viscosity," and has nothing to unlearn in order to use it correctly as the name for a definite physical constant, which varies to be sure with temperature and to some extent with pressure but may be exactly defined mathematically.

The c.g.s. unit of absolute viscosity is known as the poise and does not vary with the rate of shear. Indications of such a variation may usually be traced to defects in the method of test, or to the fact that the material under test was plastic. In first suggesting the term "poise," Deeley and Parr[3] pointed out that "in the case of a soft solid (plastic substance) the so-called viscosity is not the same

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[1] Jeremiah, 18 : 5–6. John, 9 : 6.

[2] *Bacon*, "Natural History," p. 839.

[3] R. M. Deeley and P. H. Parr, *Phil. Mag.*, Vol. 26, p. 87 (1913).

Mr. Herschel for all rates of shear, whereas the viscosity of a liquid is a physical constant and should be named." It is evident that they never intended that the poise or the term "viscosity" should be applied to the consistency of plastic materials, and such use is to be deplored.

The Century Dictionary and Cyclopedia frequently makes use of the word consistency to indicate viscosity as well as plasticity, and this usage is of great convenience when the nature of the material is different in different samples, or changes from one state to another with the temperature. Thus "syrupy" is defined as "having the consistency of syrup," and in a quotation from *Popular Science Monthly* reference is made to wells which yield "an oil of the consistency of treacle." Syrup and treacle may be considered viscous, but beeswax is of more doubtful consistency and is thus described: "Its consistency varies with the temperature; it is ordinarily a pliable solid, readily melted. It softens at 80° F., becoming extremely plastic, and retaining any form in which it may be molded like clay or putty, and melts at 158° F."

Abraham[1] does not hesitate to use the word "consistency" for materials of a wide range of characteristics. Thus he says that pitch is of "viscous to solid consistency," and that bituminous cements are of "plastic, trowelling consistency."

Although the difference between a liquid and a plastic can not always be recognized by sight or touch, the distinction between them is very clear. According to Maxwell:[2]

> "If the form of the body is found to be permanently altered when the stress exceeds a certain value, the body is said to be soft or plastic, and the state of the body when the alteration is just going to take place is called the limit of perfect elasticity. . . . If the stress, when it is maintained constant, causes a strain or displacement in the body which increases continually with the time, the substance is said to be viscous. When this continuous alteration of form is only produced by stresses exceeding a certain value, the substance is called a solid, however soft it may be. When the very smallest stress, if continued long enough, will cause a constantly increasing change of form, the body may be regarded as a viscous fluid, however hard it may be."

It may be expected that mixtures of liquids, or true solutions, will be viscous, but that mixtures of finely divided material with a liquid will be plastic. In case of doubt, tests are necessary, the essential requirement being that successive trials should be made at different average rates of shear. The velocity of shear, with any form of apparatus, varies at different points of the material under test, and it would be only by a rare chance that the average rate of shear in a rotary instrument would be the same as the average rate of shear

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[1] Herbert Abraham, "Asphalts and Allied Substances," pp. 24, 473 (1913).

[2] J. C. Maxwell, "Theory of Heat," p. 302 (1899).

Mr. Herschel

with the dropping ball or the efflux method. Hence, a single test with different types of apparatus would not give comparable results with plastic materials, although any type might serve to determine whether or not a material were plastic.

It is to the failure to distinguish between liquids and plastics to which must be largely ascribed the confusion in regard to consistency with which the colloid chemists are now struggling.[1] It is futile to try to explain the cause of changes in consistency until some approach to accuracy in the measurement of consistency is attained, and this can never occur as long as the attempt is made to measure the viscosity, so called, of plastic materials.

While it is a great step forward to recognize that plasticity is a composite property which must be expressed by at least two values, it is not certain that two values are enough. Buckingham[2] finds three constants, at least when the capillary tube method is used, and Breyer and Green admit that in view of the great lack of experimental data it cannot be stated that measurements of yield value and mobility "are capable of an interpretation that will inform us as to the particular type of consistency with which we are dealing."

It seems desirable to use some such term as "mobility" for the readiness of flow of a plastic material, reserving the term "fluidity" for the reciprocal of the viscosity expressed in poises, and applying it only to true liquids having a zero yield value. There is great danger that in using the term "yield value," the elastic properties of plastic materials will be overlooked. It will be remembered that Maxwell did not use the expression "yield value" but spoke of the "limit of perfect elasticity." It can no more be assumed that all plastic materials are entirely lacking in elasticity than that all hard solids are perfectly elastic. Schwedoff[3] found that of the apparent liquids tested, some like glycerin and sugar solutions showed only viscosity, but that a solution of 5 g. of gelatine in one liter of water was as elastic as a steel spring, provided the stress was small enough and not too long prolonged. Jelly strength is sometimes tested roughly with a viscosimeter, but Sheppard and Sweet[4] find that it may be determined as accurately as absolute viscosity and expressed in terms of Young's modulus of elasticity.

It appears to be quite a general idea among ceramists that plasticity depends upon the nature of the clay and is not a property,

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[1] E. F. Burton, "Physical Properties of Colloidal Solutions," p. 163 (1916).

[2] E. Buckingham, "On Plastic Flow Through Capillary Tubes," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1154 (1921).

[3] Th. Schwedoff, *Rap. Cong. Int. Phys.*, Vol. 1, p. 478 (1900).

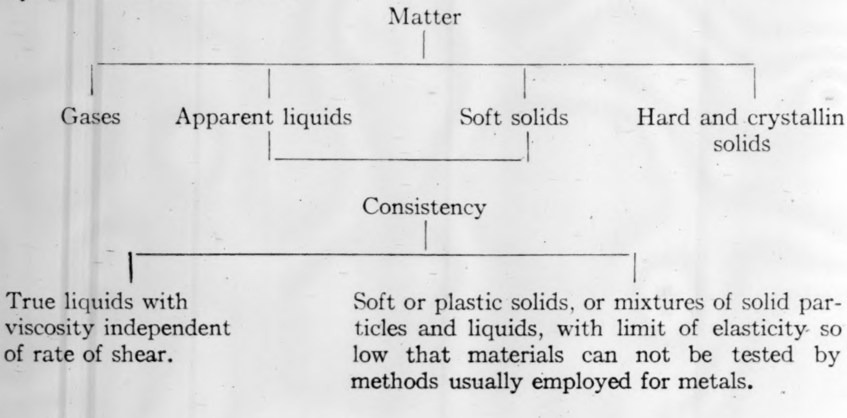
[4] S. E. Sheppard and S. S. Sweet, Ind. and Eng. Chem., Vol. 15, p. 571 (1923).

Mr. Herschel like consistency, which is changed by the addition of water. This view can be reconciled with the definition of plasticity as a type of consistency if it is noted that what the ceramists really mean is the same as in the Gospel of John, that certain clays have the inherent property of becoming plastic when mixed with the right amount of water, and the ceramists say for the sake of brevity that certain clays are plastic.

Reference is made on pages 440 and 450 of the Resumé on Consistency to the force of gravity as the lower limit of the yield value of a plastic material. Unless some upper limit is set, all solids must be considered as plastic above the elastic limit. According to the Century Dictionary a plastic solid is "A solid whose limit of elasticity is far below its point of rupture so that it can be shaped: thus putty and wrought iron are plastic solids." This definition is not altogether satisfactory because one could ask "How far below?" but it has the virtue of clearly stating a principle upon which a satisfactory definition could be based, and it contains only terms which are generally known in all industries, which is not the case with mobility and yield value.

From the point of view of the testing laboratory, then, all materials may be divided into four classes, 1, viscous gases, 2, apparent liquids, 3, soft solids, and 4, hard solids. All of class 1 and some of class 2 would be tested for absolute viscosity. Class 4 would include all materials which could be adequately described by elastic limit, Young's modulus of elasticity, ultimate strength and Brinell hardness. Doubtless the methods of testing materials in classes 1 and 4 could be improved upon, but the greatest problems lie in testing the soft solids of classes 2 and 3, a great variety of materials which will eventually be subdivided when the necessary data are available.

The following diagram may serve to summarize the proposed system of nomenclature:



True liquids with viscosity independent of rate of shear.

Soft or plastic solids, or mixtures of solid particles and liquids, with limit of elasticity so low that materials can not be tested by methods usually employed for metals.

Mr. Emley

MR. EMLEY.—After a careful study of Mr. Bingham's discussion, I want to state that I believe he has arrived at what comes nearest a scientific definition of the term "consistency" that I have yet seen, so far as the lime industry uses the word. When a plasterer or mason wants to determine whether or not the material he is using is of the proper consistency, he cuts it down with the trowel and then runs the trowel through it. The original cutting down he pays no attention to, but what he pays attention to is how the trowel works through the material.

There is a marked distinction between plasticity and consistency in the lime industry. It is generally recognized in the trade that by proper proportioning of lime and water, any lime can be brought to any predetermined consistency, but certainly one cannot bring them to the same plasticity, no matter how much water is used. The ability of lime material to become plastic when it is wet is recognized in the price of the lime, whereas the consistency depends merely on the amount of water you add in mixing it.

There is a paragraph in Mr. Bingham's discussion having to do with our method for measuring the plasticity of hydrated lime. We have a machine for that which measures the resistance of the lime to the suction of a porous base plate. It does not measure its plasticity. I do not think any member of Committee C-7 on Lime would ever state it as a method of measuring plasticity. It measures resistance to suction. We have found from experience that this resistance to suction is a property that always accompanies plasticity. I do not think there should be any confusion there.

Referring to an earlier paper, we object seriously to the statement that consistency is noncomparable. In the lime industry that would be a most serious thing, and I think that is true of most cementitious materials. I know if I went to a concrete man and said that the strength of certain samples of concrete was too low because the consistency was too wet, he would certainly understand exactly what I meant. The word "consistency" is comparable and is used in that way in the trade.

Mr. Kitts

MR. J. A. KITTS[1] (*by letter*).—Records of tests made by experienced operators show (when taking average results) that the "normal-consistency" mix of neat cement, cement-sand mortar, and of concrete is that which gives maximum density and strength. In the development of the technique of concrete testing it is my impression that the term "normal consistency" originated from that characteristic of neat cement when the cement was so wetted as to produce mortar of max-

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[1] Concrete Technologist, Pacific Portland Cement Co., Gerlach, Nev.

Mr. Kitts imum density and strength. It appears, however, that the invention and general use of the metal rod penetration machine to make the determination of "normal consistency" of neat cement, and the extension of consistency tests to sand-cement mortars and concretes, obscured the maximum density-strength basis by ideas of plasticity, flowability, workability, etc.

Maximum density is an understandable and established physical value; it may be easily and accurately measured at time of mixing; it is a scientific basis for "normal consistency" or for basic water content of the mix; and, "normal consistency" may be defined, without departing from the present standard, as that water content which produces maximum density mortar or concrete.

With maximum density as the basis of "normal consistency" of mix in concrete work, the properties designated as "flowability", "workability", "slump", etc., should be distinguished as approximately measurable characteristics of "consistency" and not as bases of determination of "consistency".

Mr. Breyer MR. BREYER.—I am surprised that Mr. Emley takes the standpoint that yield value is of no importance to consistency. The amount of mortar that he can put on a wall is dependent, not on the figure Mr. Bingham gives as consistency (that is, mobility minus yield value), but is dependent on the yield value alone. If it has no yield value, it is no good as mortar, because it will run down. If paint has no yield value you cannot put it on the wall. I, therefore, object seriously to leaving the yield value out of the definition of "consistency." You must consider that in the practical use of certain plastics they must stand up for a certain length of time until they dry, cure or set, whichever term is used. Rubber samples must have a certain amount of stiffness to stand up before they are cured, and yield value is a fundamental property of that material and is commonly spoken of and considered as one part of the consistency of that material. I cannot see that you can leave out such a simple conception as the force holding the lime on the wall—leave out yield value.

Now as to the question of whether we shall accept Mr. Bingham's proposal or whether we shall go further with Mr. Wilson and add "elasticity" and "texture"—it seems to me the last two terms are already very well taken care of. "Elasticity" has a perfectly definite physical meaning to-day and needs no further definition and is not commonly considered under consistency. It might be said by the layman that a substance like rubber has an elastic consistency, but you are then expressing one property which is already well taken care of and needs no further definition at all. Elasticity is perfectly definite and has a meaning, and so does texture; why put them under consistency? We can speak of a soft fibrous texture or a grainy texture or an oriented or non-oriented texture; these are all well defined and are not commonly taken into consideration under the heading "consistency." **Mr. Breyer**

MR. ROBERT E. WILSON[1] (*by letter*).—Mr. Breyer objected to my bringing in factors such as "elasticity" and "texture" as a part of the concept of consistency. In doing this, I have merely stated a fact well known to those in the grease business, namely, that terms such as "a stringy consistency" or "a spongy consistency" are frequently used in the trade. In other words, the general concept of consistency, at least in this connection, does involve factors of structure or texture as well as mobility and yield point and we must not close our eyes to this fact. **Mr. Wilson**

Regarding Mr. Bingham's proposed definition of consistency—in the first place, to take a term which is already in common usage with a loose meaning and attempt to give it an accurate meaning is practically certain to fail because no one would ever know which usage was intended by the speaker or writer. For example, if in one of our meetings a speaker uses the term "plasticity" we must find out whether he belongs to Committee C-7 or Committee D-4 before we have any idea of what he means. Whereas, if he uses the terms "mobility" and "yield point" we realize at once that his words have a definite and clean-cut significance.

The second objection to his proposed definition of consistency is even more important. Mr. Bingham has done a great service by showing that the characteristics of plastic solids could not be defined in terms of a single variable such as viscosity but must be separated into the two factors of yield point and mobility. It would certainly, therefore, be a step backward to ignore one of these factors arbitrarily in defining consistency, placing us just where we were before we had the valuable results obtained with the plastometer and similar devices.

MR. JEROME ALEXANDER.[2]—It seems to me that in a case like this what we have to do is to apply common sense or pragmatic philosophy. In James' book on "Pragmatism" he relates the following story: "The whole camp was divided into two factions over a very interesting point. The facts were these; there was a squirrel supposed to be on a certain tree, and you know a squirrel will always keep the tree between you and him. A man tries to sneak around behind that squirrel, and the man circles the tree; but as the man circles the tree, **Mr. Alexander**

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[1] Assistant Director of Research, Research Laboratory, Standard Oil Co., Ind.

[2] Consulting Chemist and Chemical Engineer, New York City.

Mr. Alexander the squirrel continually goes around the trunk, always keeping his nose toward the man. The question was, after the man had made a circuit of the tree, has the man gone around the squirrel or has he not?" Of course this question was put up to James, who was supposed to be an authority, and he, taking the cue of the old schoolmen, sought to resolve the difficulty by making a distinction. He said, "If by going around the squirrel, you mean being in front of him, to his right side, behind him and to his left side and in front of him again, he has not gone around him. But if you mean going to the east, north, west and south, he has gone around him."

The situation is quite the same here. I think it was Aristotle who said that one of the most difficult things to do was to frame an exact definition, and in our language, which has had very gradual growth and came into existence before science did, words have acquired very general meanings, and we cannot very well do violence to the language. Nature is full of practical things which are the result of a great many different factors which vary in different ratios, and while it would be lovely and beautiful if we could put the ultimate results of these complex factors into one simple formula or expression, it is not practical to do it and we cannot do it, and the sooner we awake to the realization that we cannot, the quicker we will come down to earth.

The basic principles involved in all these matters as I see them (and what I am saying now is said without malice prepense or aforethought) revolve about the fact that in the first place, all matter is discreet and is very complex—I will sidestep the question as to whether or not electrons are complex; I will assume for the sake of argument that they are not. Atoms, being composed of large numbers of electrons, are certainly complex; molecules, being composed of atoms, are still more complex; then we have molecular groups which are even more complex, and these are apt to form colloidal complexes which are still more complex. But it is only when we have aggregates of secondary colloidal particles that we come into the realm of *visible* particles. So there is a whole series of complexities underlying what we see, and those who have used the microscope know that there is a whole series of steps below what you see. But these things that you do not actually see, nevertheless influence the ordinary working properties of things.

Coming down to these basic principles, we might consider the kinetic motion of the particles. I think that is a very important factor to be taken into consideration in viscosity, plasticity and similar properties of matter. Then again, let us consider multi-

Mr. Alexander

phasic systems, something like concrete, having series of phases whose particles are of a totally different order of magnitude. In some phases the kinetic motion is very violent; in others it is very slight and others have no kinetic motion.

Another factor to consider is the question of time. Ordinarily time is omitted from consideration. I understand it has been proved experimentally that if you take a cork and bury it in a barrel of tar and give it enough time, it will float to the surface of the tar. Then Robert-Austen took a bar of lead and a bar of gold and put them together and let them lay side by side for five years, and at the end of five years they had diffused, each into the other. Popularly speaking, those metals are solids, yet they had diffused just as though they were liquids or gases. A great many things are quite negligible unless you take enough time in your experiments. This building was built on the assumption that plumb lines are parallel, and for all practical purposes that is so; yet a plumb line at Atlantic City and one at Constantinople are about at right angles to each other. The slight difference is cumulative.

It seems to me that generally speaking Mr. Herschel has the right point of view: "Consistency" is a general term, a term which exists in the language and has been applied to what we call "body," "viscosity" and many other properties, all depending on what particular feature we are stressing, and what particular use we are considering. It is very difficult to find any one term which is going to cover a variety of phenomena and still have that term fairly definite. I think we will be compelled to be inconsistent enough to keep consistency in its original meaning.

Mr. Herschel

Mr. Herschel.—I should like just a minute to answer Mr. Breyer in regard to why you have to speak about elasticity. Some tests of Schwedoff[1] with a solution of gelatine, with only five grams of gelatine in a liter of water, showed that this material, if subjected to a very small stress for a very short period of time, was just as elastic as a steel spring. I raise this point in general, that you can no more assume that all plastic materials have no elasticity than you can assume that all crystallin solids have perfect elasticity. It would be very convenient if we could make that assumption, but unfortunately there are classes of material that show plasticity and elasticity in all the possible variations, and to exactly define these materials, we must say to what extent they possess plasticity and to what extent they possess elasticity. It is a very complicated problem. We cannot help it. The trouble is that in defining "plasticity," or "con-

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[1] Th. Schwedoff, Congres international de physique, Vol. 1, p. 478 (1900).

Mr. Herschel sistency" if you like, by mobility and yield value, we are overlooking the fact that materials which show yield value may have elasticity; in fact, Maxwell, in the quotation to which I referred before, says nothing about yield value; he speaks of the limit of perfect elasticity. In using this new term "yield value" we are in danger of getting away from the fact that these so-called plastic materials which show yield value may also show elasticity.

THE PROPERTIES OF METALLURGICAL COKE[1]

BY G. ST. J. PERROTT[2] AND A. C. FIELDNER[3]

INTRODUCTION

For the past hundred years it has been believed that other properties of coke as well as its chemical composition affect its utility as blast-furnace fuel. A considerable amount of work and thought has been expended in an attempt to define quantitatively certain physical properties of metallurgical coke and determine their relation to blast-furnace operation, but furnace men in general have put little faith in the results of such testing methods. Consumers are unanimously agreed that coke is a potent factor in affecting the blast-furnace process, but they are by no means agreed as to the properties which a good coke should possess, or as to the methods by which these properties may be determined quantitatively.

In evaluating coke, account is taken of its chemical analysis, certain of its physical properties—that is, true and apparent specific gravity, porosity, mechanical strength—and of its combustibility in air and in carbon dioxide. Methods of making chemical analysis have been standardized, and the results are easily interpreted in terms of blast-furnace operation. Physical testing methods are well on the way to standardization, but the meaning of the data in terms of blast-furnace operation is by no means clear. The importance of the relative combustibility of the coke used as fuel, or of its relative rate of oxidation by carbon dioxide of the stack gases, has been much discussed. Work on methods of quantitatively evaluating this reactivity and relating it to results of actual furnace practice has been a history of words rather than of valuable experimental data.

The Bureau of Mines, in cooperation with Committee D-5 on Coal and Coke of the American Society for Testing Materials, is engaged in an attempt to formulate standard testing methods, and thus to provide the measuring sticks by which the suitability of coke for blast-furnace use may be judged. It is hoped that standardization and acceptance of these testing methods, by providing blast-furnace and coke-plant operators with a mutually intelligible language, will eventually lead to a solution of the mystery of the blast furnace.

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[1] Published by permission of the Director of the U. S. Bureau of Mines.

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Interpretation of the meaning of test results in terms of blast-furnace operation cannot be consummated in a day, and will require the cooperation of operating men over a considerable period of time.

This paper sets forth the prevailing ideas as to the function of coke in the blast furnace and the properties possessed by a desirable coke, and then discusses the problem in the light of recent data secured by the writers and their colleagues, both in the laboratory and at the blast-furnace plants.

FUNCTION OF COKE IN THE BLAST FURNACE

Coke performs two classes of duties in the blast furnace, namely, physical or mechanical, and chemical.

Physically, coke is a fuel of low density that (1) keeps the other constituents of the burden so separated as to permit easy passage of the gases; (2) resists mechanical attrition due to the weight and grinding action of the descending stock; (3) does not spall or decrepitate, fuse, or give off tarry matter under the influence of heat; and (4) descends unchanged to be burned at the tuyeres.

Chemically, coke provides heat for the furnace reactions and carbon monoxide gas for reducing the ore in the shaft. Coke is burned in a relatively small zone (about 30 to 40 in. in diameter) around the tuyeres,[1] forming carbon monoxide and small quantities of hydrogen as the final products of combustion.

Combustion takes place in two stages, as follows:

$$\begin{aligned} C + O_2 &= CO_2 + 97,200 \text{calories per gram-atom of carbon} \ldots\ldots (1)\\ CO_2 + C &= 2CO - 38,800 \text{ calories per gram-atom of carbon} \ldots (2) \end{aligned}$$

Reaction (1) is complete at about 24 in. from the nose of the tuyeres, and (2) is complete at about 40 in. (See Fig. 3.) The coke ash is here melted, and combines with the limey slag from above to form the final resultant slag, which is tapped off at the cinder notch. In the zone of spongy iron, considerable sulfur is probably taken up from the ferrous sulfide in the coke.[2] In the combustion zone, the sulfur released from the coke is probably entirely taken up by the slag.

Ore in the stack is reduced according to the equations:

$$\left.\begin{aligned} 3\,Fe_2O_3 + CO &= 2Fe_3O_4 + CO_2\\ Fe_3O_4 + CO &= 3FeO + CO_2\\ FeO + CO &= Fe + CO_2 + 2340 \text{ calories} \end{aligned}\right\} \ldots\ldots\ldots (3)$$

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[1] G. St. J. Perrott and S. P. Kinney, "Combustion of Coke in Blast-Furnace Hearth," *Preprint 1222-S*, Am. Inst. Mining and Metallurgical Engrs., New York Meeting, February, 1923.

[2] A. R. Powell, "Forms of Sulfur in Coke, and Their Relation to Blast-Furnace Reactions," *Preprint 1219-S*, Am. Inst. Mining and Metallurgical Engrs., February, 1923.

This has been called "indirect reduction," as distinguished from "direct reduction" by solid carbon—

$$FeO + C = Fe + CO - 36{,}540 \text{ calories} \ldots\ldots\ldots\ldots (4)$$

As the coke proceeds down the stack it is surrounded by the stack gases containing carbon dioxide formed from the reactions in (3). At temperatures above 900° C., the following reaction begins to take place in the stack:

$$C + CO_2 = 2CO - 38{,}800 \text{ calories} \ldots\ldots\ldots\ldots\ldots (5)$$

This oxidation of carbon by carbon dioxide has been called "premature combustion" or "solution" of carbon and is generally believed to be detrimental to furnace operation, because it absorbs heat from the stack and prevents carbon from reaching the tuyeres.

A coke for blast-furnace use must (1) be of sufficient mechanical strength to resist crushing in the furnace, (2) must be low in ash and sulfur, (3) must be resistant to the oxidizing action of the carbon dioxide in the stack gases, and (4) must burn rapidly at the tuyeres—this, in brief, is the opinion of those versed in the theory and practice of the blast furnace. Whole volumes have been written elaborating these ideas, but the court of last resort for judging the value of a coke is the blast furnace itself.

A word as to the difficulty of determining the relative merits of cokes from blast-furnace operating data may not be out of order. The efficiency of coke is judged by the weight of coke required to produce a ton of iron and the quantity of iron made per day. This is determined in turn by the ratio of coke to ore in the furnace, the iron content of the ore, and the volume of blast blown per minute. The first and last of these factors are determined by the blast-furnace operator, who is guided by the regularity of descent of the stock, by the appearance of the combustion zone at the tuyeres, and by the composition and apparent temperature of iron and slag. The operator must keep his furnace in continuous operation, and must keep his ratio of fuel to ore sufficiently high to prevent "freezing" of the furnace.

It is evident that the performance of a furnace depends in great measure upon the operator's opinion as to the minimum amount of coke which may be used. When to the operator variable we add the variables of different ore burden, furnace lines, grades of iron, and uncertainty as to the true analysis and weight of materials going into the furnace, we evidently have a difficult problem in attempting to isolate the effect of various kinds of coke by making use of data obtained at different furnaces.

Consideration of a large amount of furnace data from all parts of the country shows that there are furnaces making good records and bad records on cokes apparently identical in physical characteristics, and the data afford little indication of the importance of the physical properties of the fuel. Coke has undoubtedly been blamed for many ailments of the blast furnace for which it was not responsible, a natural occurrence, because no accepted tests exist by which a good coke may be distinguished from a bad coke. But the belief that the quality of coke does influence blast-furnace operation is so widespread that it must be assumed for the present at any rate to rest on some basis of fact. A brief account of testing methods and their probable interpretation will serve to show the present state of our knowledge.

PHYSICAL PROPERTIES OF COKE

Crushing Strength.—This test is made on 1-in. cubes of coke. Crushing strength of cokes made in this country varies from 400 to 2000 lb. per sq. in. The actual pressure on the coke in the hearth of a modern blast furnace is 35 to 40 lb. per sq. in. This would seem to provide a sufficient factor of safety with the weakest coke. A modern blast furnace has been run with charcoal as fuel with no trouble experienced from crushing.[1] Many separate determinations must be made if an average value is to be obtained, and it is doubtful whether the results secured at the expenditure of much time and labor have any meaning in terms of blast-furnace operation.

Shatter Test.—This test, designed by D. T. Croxton and described by Belden,[2] if properly conducted, probably gives as good a measure of the mechanical strength of coke as any test yet devised. The shatter-test apparatus and procedure are described in the methods of the society.[3] A 50-lb. sample of coke is dropped four times from a height of 6 ft. upon a steel plate. The material is then screened over a 2-in. square-mesh screen, and the percentage of the original sample remaining on the screen is reported as the shatter-test result. The necessity of making a number of duplicate determinations if results are to have any meaning has been pointed out by one of the authors.[4] Twenty-five duplicate determinations were made on a number of samples of well-mixed coke, and it was found that the average deviation of results from the mean varied from 1 to 3 per cent, while the maximum deviation was in one case as high as 13 per cent. Fig. 1 shows the variation of single determinations from the average in one series of tests and for comparison the daily data from the shatter-test records of a by-product plant where a single determination was made each day. Daily results show a somewhat higher deviation from the mean value for the month than is shown by experiments run on coke from a single oven. This indicates that there is probably some difference in the quality of coke from day to day, but the difference is evidently too small to be detected by a single determination.

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[1] R. H. Sweetser, "Charcoal and Coke as Blast-Furnace Fuels," *Transactions*, Am. Inst. Mining and Metallurgical Engrs., Vol. 39, pp. 228–235 (1908).

[2] A. W. Belden, "Metallurgical Coke," U. S. Bureau of Mines, *Technological Paper No. 50*, p. 29 (1913).

[3] Tentative Method of Shatter Test for Coke, *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 822 (1922).

[4] S. P. Kinney and G. St. J. Perrott, "The Shatter and Tumbler Tests for Metallurgical Coke," *Journal Industrial and Engineering Chemistry*, Vol. 14, pp. 926–941 (1922).

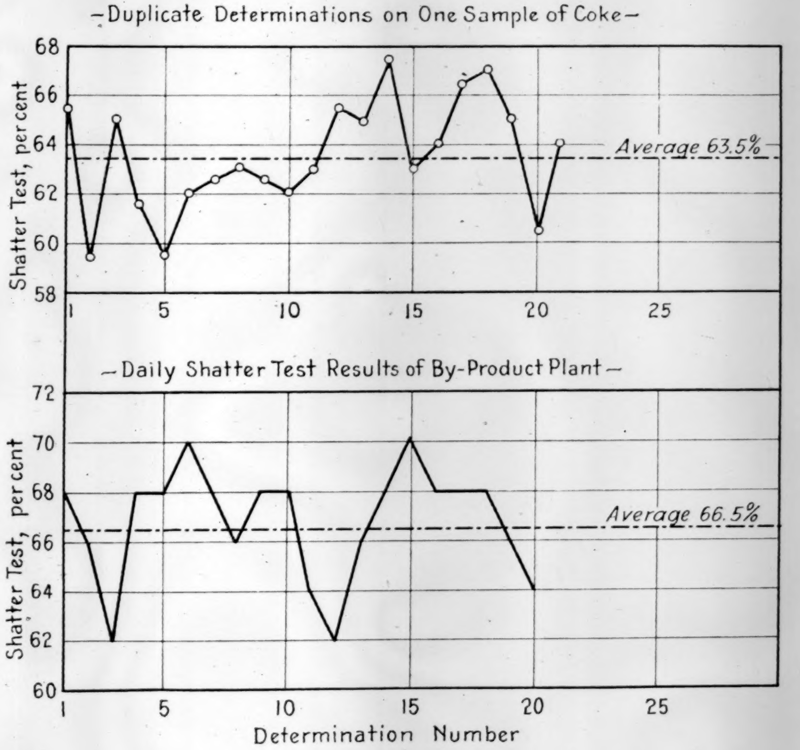

FIG. 1.—Comparison of Variation Between Duplicate Determinations on Same Sample of Coke and Daily Variations at By-Product Plant Where One Test Is Made Each Day.

A lower shatter-test value was obtained when pieces half the width (7 to 10 in.) of the oven were used than when run-of-oven coke was employed. It was recommended that pieces half the width of the oven be used when testing by-product coke and that pieces 7 to 20 in. in length be employed with beehive coke. Several screens should be used as indicators of the shattering effect: namely, 2, 1½, 1 and ½ in.

The shatter test indicates the resistance of coke to breakage in handling. Cokes made in the Pittsburgh and Birmingham districts usually show a value between 65 and 70 per cent. A well-known coke made from Kentucky coal, with which blast-furnace records have been made, gives a value of about 60 per cent, while a coke made from Illinois coal, with which excellent operating records have been made, gives a value of about 42 per cent. From the available data it would seem that considerable variations in the shatter-test result have little influence on blast-furnace performance.

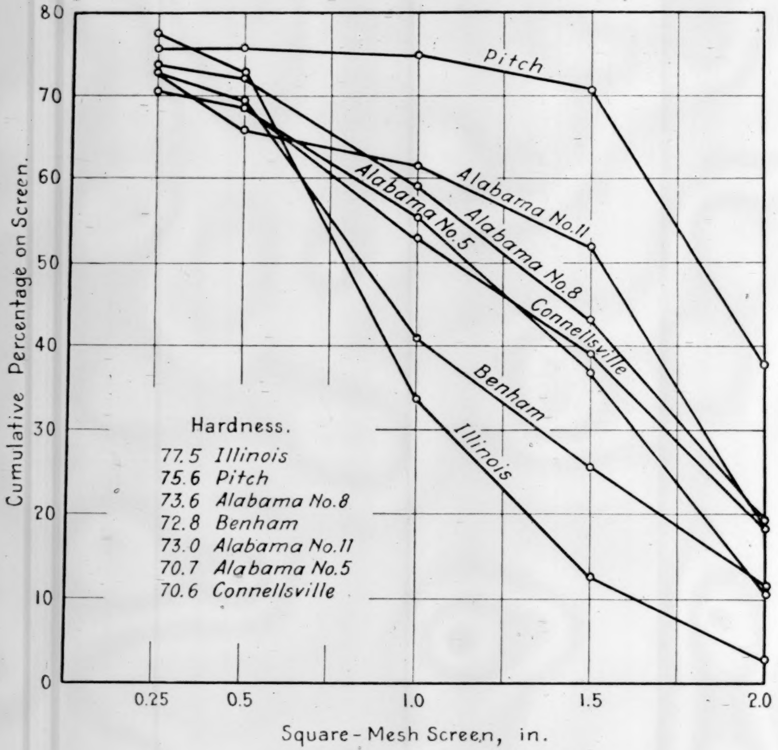

FIG. 2.—Screen Analysis of Cokes After Tumbler Barrel Test.

The Tumbler Barrel Test.—After the coke is discharged into the bell of the blast furnace, it is no longer subject to breakage by impact. The action is then one of slow abrasion as the coke travels down the furnace under the increasing weight of the burden above. The tumbler barrel test apparatus[1] was designed to give a relative value for the resistance of cokes to mechanical attrition in the furnace. In the test procedure, a 25-lb. sample of 3 to 2-in. coke is placed in the apparatus, and the drum revolved 1200 times. The so-called hardness number is reported as the percentage of the original sample remaining on a $\frac{1}{4}$-in. screen after the test.

Kinney and Perrott[2] have shown that two factors determine the amount of coke broken to pass a $\frac{1}{4}$-in. screen, namely, impact and abrasion. The smaller the size of the coke pieces, the less will be the effect of impact, and consequently the higher the hardness number. A coke that shatters easily and is broken to small pieces during the test will come through the test with a high percentage remaining on the $\frac{1}{4}$-in. screen. This is shown in Fig. 2. If judged according to the standard procedure, we should say that the Illinois coke was extremely hard, since it shows 77.5 per cent remaining on the $\frac{1}{4}$-in. screen. However, we note that only 15 per cent of the Illinois coke was larger than $1\frac{1}{2}$ in. after the test, against 36 to 60 per cent of the other cokes.

Rice,[3] using an apparatus containing steel balls, found that over a period of months a low hardness number went hand-in-hand with high blast pressure required to blow a given volume of air through the blast furnace. While the test may have a value as applied to the daily testing of coke supplied to a blast furnace whose coke supply is not subject to large variations in resistance to shattering, it certainly has no meaning as at present carried out when applied to cokes of varied physical properties. If a screen analysis from 2 to $\frac{1}{4}$ in. be made after the tumbler test, an indication of the quality of the coke is obtained, but such data indicate no property of the coke which would not be shown by an ordinary shatter test.

True and Apparent Specific Gravity.—We distinguish between "true" specific gravity—that is, the specific gravity of the pore-free coke, or, in other words, of the carbonaceous material and inorganic matter making up the cell-walls—and "apparent" specific gravity—that is, the specific gravity of the coke substance plus cells. The values of these quantities for ordinary metallurgical cokes of this country are: true specific gravity, 1.84 to 2.10; and apparent specific gravity, 0.80 to 1.15.

The value for true specific gravity increases with the temperature at which the coal was coked, the geologic age of the coal, and the percentage of ash in the coke. The apparent specific gravity depends upon the true specific gravity and upon the percentage of the total volume of coke occupied by cells. For coke of a given size, this figure is directly proportional to the weight of a cubic foot of the loose coke. Selvig and Parker[1] have discussed methods of determining true and apparent specific gravity.

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[1] Designed after the apparatus of G. D. Cochrane described in *Journal*, Iron and Steel Inst., Vol. 97, p. 141 (1918).

[2] Kinney and Perrott, *loc. cit.*

[3] "Importance of Hardness of Blast-Furnace Coke," *Preprint No. 1030*, Am. Inst. Mining and Metallurgical Engrs. (1921).

Koppers[2] believes that the combustibility of coke increases as the true specific gravity decreases, therefore, he advocates carbonization at lower temperatures than at present practiced. His assertion is open to considerable doubt in so far as it pertains to combustion in the blast furnace.

Porosity.—By porosity is meant the percentage of the volume of the coke which is occupied by cells. It is usually computed as follows:

$$\text{Porosity} = \frac{\text{True density} - \text{apparent density}}{\text{True density}} \times 100.$$

This value varies from 40 to 60 per cent; the average figure is about 45 per cent.

It is generally believed that the porosity of coke is an important factor in determining its performance in the blast furnace. A porous coke is believed to present more surface to the blast than a dense coke, and hence is more combustible. Much time and labor have been spent in making photomicrographs of coke and in studying the details of cell structure. Under the discusion of combustibility it will be shown that the effect of porosity is practically negligible.

Size.—It is generally believed that coke containing any considerable percentage of breeze is not good blast-furnace fuel. Hanging of the charge, slipping, and high blast pressure are said to result when such coke is used. Coke is usually screened over a $\frac{3}{4}$ or $\frac{1}{2}$-in. screen before being used in the furnace. On the other hand, certain furnace men report using small coke—under 3 in.—for considerable periods with success. It would be expected that the extent of the combustion zone at the tuyeres would diminish as the size of the coke decreased. Further information is needed in regard to the effect of the size of coke on blast-furnace operation.

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[1] W. A. Selvig and W. L. Parker, "Determination of Specific Gravity of Coke," *Chemical and Metallurgical Engineering*, Vol. 28, pp. 547–550 (1923).

[2] H. Koppers, "Fortschritte auf dem Gebiete der Kokserzeugung, der Einfluss der Koksbeschaffenheit auf den Hochofenbetrieb und Vorschläge für die Verbesserung des letzteren," ***Stahl und Eisen***, Vol. 41, pp. 1173–1181, 1254–1262 (1921); "Vorschläge zur Prüfung des Kokses für Hochofen und Giessereizwecke, ***Stahl und Eisen***, Vol. 42, pp. 569–573 (1922).

An average screen analysis of beehive and by-product cokes used at a blast-furnace plant near Pittsburgh is as follows:

SCREEN, IN.	CONNELLSVILLE BEEHIVE	BY-PRODUCT
Retained on 2.5-in. screen	58.7 per cent	42.3 per cent
Retained on 1.25-in. screen	34.5 "	44.7 "
Retained on 0.75-in. screen	3.8 "	5.4 "
Passing 0.75-in. screen	3.1 "	4.0 "

At this furnace plant, considerable trouble was experienced with the by-product coke, due to hanging and slipping, and it was the belief of the furnace superintendent that the smaller size of the by-product coke was responsible for the trouble.

CHEMICAL ANALYSIS OF COKE

The constituents of coke which are of importance to the blast-furnace operator are those determined in the usual proximate analysis, that is, moisture, volatile matter, fixed carbon, ash, and sulfur. Of these, only the carbon is desirable, and the ideal coke would consist of pure carbon. Moisture must be evaporated, with consequent consumption of heat which might better be used for other purposes. Volatile matter is driven off before the coke reaches the combustion zone, and, aside from playing a small part in gaseous reduction in the stack, it is of no value. Ash must be melted at the tuyeres, and a sufficient amount of lime has to be added to flux it as a fluid slag. Richards[1] estimates about 0.23 lb. of fixed carbon as necessary to melt one pound of slag. On account of its deleterious effect on the properties of the iron, sulfur is the most undesirable impurity. Phosphorus, as a rule, is not present in sufficient quantities to be of importance.

No hard and fast specifications for chemical composition can be drawn. Iron has been made with coke containing as high as 2 per cent of sulfur and 20 per cent of ash. At present we should call a coke "good" which contained less than 11 per cent of ash and 1.10 per cent of sulfur. It is probable that as our supply of low-ash coal diminishes, coke considerably higher in ash and sulfur will be employed. Analyses of a number of samples of beehive and by-product coke are given in Table I. Table II shows physical properties of coke samples, and Table III shows composition of coke ash.

From the ultimate analyses in Table I, it will be noted that the term "fixed carbon" is a misnomer. The total carbon present is from 1 to 2 per cent less than the "fixed carbon." If that portion of the volatile matter containing carbon were deducted from the carbon of the ultimate analysis, the discrepancy would be still greater. The value for fixed carbon contains, in addition to carbon, all the hydrogen, oxygen, and nitrogen not driven off at 950° C., and the sulfur held in solid solution[1] in the coke carbon. The amount of carbon available for the blast-furnace reactions is, therefore, 1 to 2 per cent less than the fixed carbon even if we assume that no greater amount of carbonaceous material is volatile at the high temperatures in the neighborhood of the blast-furnace combustion zone than that indicated by the standard test at 950° C.

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[1] J. W. Richards, "Metallurgical Calculations," second edition, p. 246 (1918).

TABLE I.—ANALYSIS[1] OF COKE SAMPLES

Coke	Proximate			Ultimate						
	Moisture.[2] per cent.	Volatile Matter. per cent.	Fixed Carbon, per cent.	Ash, per cent.	Hydrogen, H_2, per cent.	Carbon, C, per cent.	Nitrogen, N_2, per cent.	Oxygen, O_2. per cent.	Sulfur, S. per cent.	Heat Value, B. t. u.
Alabama No. 1....	1.53	3.22	86.07	10.71	1.42	84.00	1.60	1.52	0.75	12 916
Alabama No. 2....	0.20	0.90	88.75	10.35	0.56	86.79	0.65	0.80	0.85	12 732
Alabama No. 3....	0.18	1.07	85.08	13.85	0.47	83.59	0.72	0.57	0.80	12 302
Alabama No. 4....	0.50	1.36	87.05	11.59	0.53	85.72	0.82	0.47	0.87	12 536
Alabama No. 5....	0.8	1.5	87.7	10.8	0.7	85.8	1.3	0.4	1.0	12 800
Alabama No. 6....	2.39	1.06	87.61	11.33	....	.....	....	....	1.30	12 839
Alabama No. 7....	0.40	0.48	84.71	14.81	....	.....	....	....	0.78	12 188
Alabama No. 8....	0.80	1.56	86.11	12.33	0.70	84.21	1.19	0.88	0.69	12 488
Alabama No. 9....	0.25	1.15	90.93	7.92	0.52	89.16	0.96	0.33	1.11	13 077
Alabama No. 10...	0.50	2.69	88.18	9.13	0.84	87.14	0.74	1.04	1.11	12 930
Alabama No. 11...	0.28	0.57	88.22	11.21	0.31	86.73	0.95	0.00	0.80	12 721
Benham..........	0.9	0.9	92.6	6.5	....	.....	....	....	0.7	13 440
Connellsville......	0.48	1.78	86.41	11.81	0.79	84.35	1.22	0.86	0.97	12 587
Illinois...........	1.3	1.5	87.1	11.4	0.3	86.2	0.4	0.8	0.9	12 620
Pitch coke........	0.33	1.07	97.98	0.95	0.55	96.66	0.72	0.62	0.50	14 144
Clairton..........	0.1	0.8	87.1	12.1	0.4	85.3	0.7	0.9	0.7	12 520

[1]Analysis on a moisture-free basis.
[2]As received at laboratory.

COMBUSTIBILITY OF COKE

By combustibility of coke from the standpoint of the blast furnace is meant the space rate at which oxygen and carbon dioxide disappear in the vicinity of the tuyeres. Assuming a constant volume of blast per minute, the mean combustibility of coke is inversely proportional to the volume of the combustion zone around the tuyeres. In the combustion zone, coke is being burned according to reactions (1) and (2); hence the limits of the combustion zone are marked by the disappearance of oxygen and carbon dioxide. The final product of combustion in the blast furnace is carbon monoxide only, therefore a given weight of blast burns a definite weight of carbon, irrespective of the combustibility of the coke. A highly combustible coke differs from a coke of low combustibility in completing the above reactions in a shorter distance from the point at which the blast first comes in contact with the fuel—that is, oxygen and carbon dioxide disappear at a shorter distance from the tuyeres, and the entire combustion process takes place in less space. Furnace men believe that the combustibility of a fuel has an important influence on blast-furnace operation. A fuel which burns rapidly to carbon monoxide at the tuyeres is best, because with such a fuel the heat is concentrated in a small zone in the hearth where heat is needed to carry on the smelting reactions. Koppers[1] has recently elaborated another explanation of the desirability of a highly combustible fuel in a theory, first proposed by Ebelmen[2] in 1841, which states that re-oxidation of metallic iron takes place in the combustion zone at the tuyeres, necessitating the consumption of carbon in the hearth for the reduction of the iron oxide so formed, and tending to cool the hearth.

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[1] A. R. Powell, *loc. cit.*

TABLE II.—PHYSICAL PROPERTIES OF COKE SAMPLES

Coke	Type of Oven	Shatter Test	Hardness		Specific Gravity		Porosity, per cent.
			(a)	(b)	True	Apparent	
Alabama No. 1	Koppers	70	70	....	1.75	0.96	45
Alabama No. 2	Beehive	74	73	89.1	2.10	1.02	52
Alabama No. 3	Semet Solvay	69	68	89.4	1.96	1.00	49
Alabama No. 4	Beehive	72	67	....	1.91	0.83	57
Alabama No. 5	Semet Solvay	69	71	90.6	2.00	0.99	51
Alabama No. 6	Beehive	64	74	90.2	2.13	1.15	46
Alabama No. 7	Beehive	72	71	88.8	1.91	0.98	49
Alabama No. 8	Semet Solvay	70	74	90.4	1.98	1.05	47
Alabama No. 9	Koppers	70	74	92.3	2.04	1.14	44
Alabama No. 10	Koppers	70	68	....	1.92	0.98	49
Alabama No. 11	Beehive	77	73	....	1.97	0.81	59
Benham	Wilputte	59	73	88.4	1.85	0.91	51
Connellsville	Beehive	66	71	92.0	1.89	1.02	46
Clairton	Koppers	70	..	....	1.09	1.95	44
Illinois	Roberts	42	78	90.5	1.84	0.87	53
Pitch coke	Beehive	78	76	92.9	2.00	1.16	42

[a] Method using 3 to 2-in. coke; result reported as percentage remaining on a 0.25-in. screen.
[b] Method using 0.75 to 0.25-in. coke; result reported as percentage remaining on a 0.17-in. screen.

TABLE III.—ANALYSIS OF MINERAL IMPURITIES

Coke	Moisture per cent.	Volatile Matter, per cent.	Fixed Carbon, per cent	Ash, per cent.	Sulfur, S, per cent	Silica, SiO_2, per cent.	Alumina, Al_2O_3, per cent	Iron, Fe, per cent.	Phosphorus, P, per cent.
Connellsville beehive	2.50	1.13	85.35	11.02	1.08	5.56	3.36	0.95	0.017
Pittsburgh by-product	3.30	1.69	84.74	10.27	0.93	5.43	3.24	0.70	0.026
Alabama by-product	2.5	1.15	90.70	7.90	1.09	3.55	2.38	1.05	0.04

As Royster[3] has pointed out, it is difficult to see the validity of either of these theories. The final temperature of combustion depends on composition of gas, specific heats, heats of oxidation, and relative masses of air, coke, and products of combustion. These are the same whether coke burns rapidly in a small volume or slowly in a large volume. As regards the re-oxidation theory of Ebelmen, the net heat effect is obviously identical, whether iron is burned by air, and carbon then oxidized by the iron oxide so formed, or carbon burned directly by air. Furthermore, analyses of gas at the tuyeres show that combustion of coke in the blast furnace is confined within a volume less than 200 cu. ft.—in other words, less than 1 per cent of

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[1] H. Koppers, *loc. cit.*, pp. 569–573.

[2] *Travaux Scientifique*, p. 359.

[3] P. H. Royster and T. L. Joseph, "Effect of Coke Combustibility on Blast-Furnace Operation," (Unpublished Report).

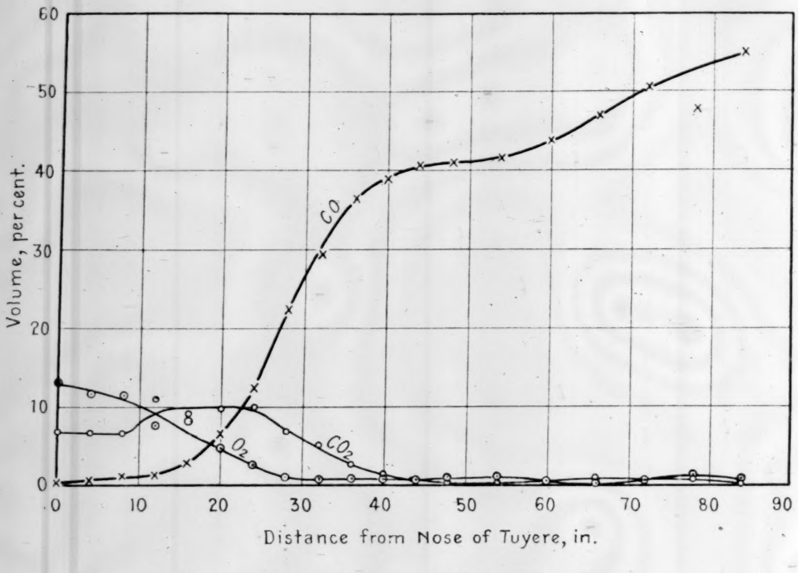

FIG. 3.—Average Hearth-Gas Composition From Ten Furnaces.

the furnace volume. Fig. 3 shows average hearth-gas composition at 10 furnaces.[1] Variations in the combustion zone at furnaces using cokes varying widely in physical properties are a matter of a few inches only, and it seems probable that large differences observed in blast-furnace operation with different cokes are due to other factors than the relative combustibility of cokes in the tuyere zone.

Combustibility Tests.—Methods for testing combustibility have been proposed from time to time, and results have been brought forward showing wide variations in the combustibility of different cokes. In the usual laboratory tube test, a weighed amount of coke is placed in a quartz or porcelain tube heated in an electric furnace to a definite temperature, and air or oxygen is passed over the coke for a definite time. The loss in weight is considered to be an indication of the relative combustibility of the sample. The errors inherent in such procedure are a definite weight instead of definite volume of coke is employed, and temperatures are considerably lower than in the blast furnace. In the blast furnace, combustion takes place in a zone of certain dimensions. In this zone a definite number of pieces of coke of any given size will be present, and the number will be independent of the apparent density of the coke. In the usual tube test, it is quite obvious that a given weight of a porous coke will contain more pieces than the same weight of dense coke, and that the greater surface so exposed will allow a greater weight of the porous coke to burn in a given time. The results will, however, have no relation to condition at the tuyeres of the blast furnace. As will be shown later, cokes vary considerably in their rate of oxidation by carbon dioxide at temperatures around 1000° C. In any combustion test in which the temperature of the fuel does not rise above 1300° C., a considerable difference in the amount burned may be observed, due to the variable rate of the reaction $CO_2 + C = 2CO$.

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[1] Perrott and Kinney, *loc. cit.*

In the salamander test of combustibility of coke, a salamander or cylindrical shell of sheet iron is filled with coke, the mass ignited, and allowed to burn by natural draft. The weight of coke oxidized in a given time is taken as a measure of its combustibility. Here, the amount of air passing through the coke is so dependent upon the size and packing of the coke pieces, and the temperature attained is so low, that it is difficult to see how such data can have any meaning in terms of combustion in the tuyere zone of the blast furnace.

Experimental Furnace of Kreisinger.—Kreisinger, Ovitz, and Augustine,[1] and Sherman and Blizard[2] describe experiments on the combustion of coke and other fuels in a furnace which seems to be free from objection. The furnace, shown in Fig. 4, consists of a steel shell, lined with fire brick, set on an ash box fitted with an air-tight door for the removal of ashes, and connected to a fan by a square air duct in which is placed an orifice for the measurement of the air supply. A fuel bed of constant height is maintained by firing at intervals. Samples of gas are taken at 1½-in. intervals in a vertical direction up to the top of the fuel bed.

These investigators found that combustion was extremely rapid, oxygen disappearing in 4½ to 6 in. from the grate. The rate of disappearance of oxygen was independent of the weight of fuel burned per hour, and varied but little with the different fuels. A summary of Sherman's results is shown graphically in Fig. 5. Their conclusions are in agreement with the results of Perrott and Kinney at the blast furnaces.

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[1] Henry Kreisinger, F. K. Ovitz and C. E. Augustine, "Combustion in the Fuel Bed of Hand-Fired Furnaces," U. S. Bureau of Mines, *Technological Paper No. 137*, pp. 15–16 (1917).

[2] Ralph A. Sherman and John Blizard, "Combustion of Blast-Furnace Cokes in Fuel Beds," *Preprint No. 1217–W*, Am. Inst. Mining and Metallurgical Engrs., February, 1923.

OXIDATION OF COKE BY CARBON DIOXIDE

Since the time of Bell it has been realized that not all of the carbon charged as fuel reaches the tuyeres to be burned by the oxygen of the blast. A variable amount is oxidized above the tuyeres by oxygen from other sources, which may take place as direct reduction of iron oxide by solid carbon, or as oxidation of carbon by carbon dioxide, the so-called "solution loss" of blast-furnace parlance.

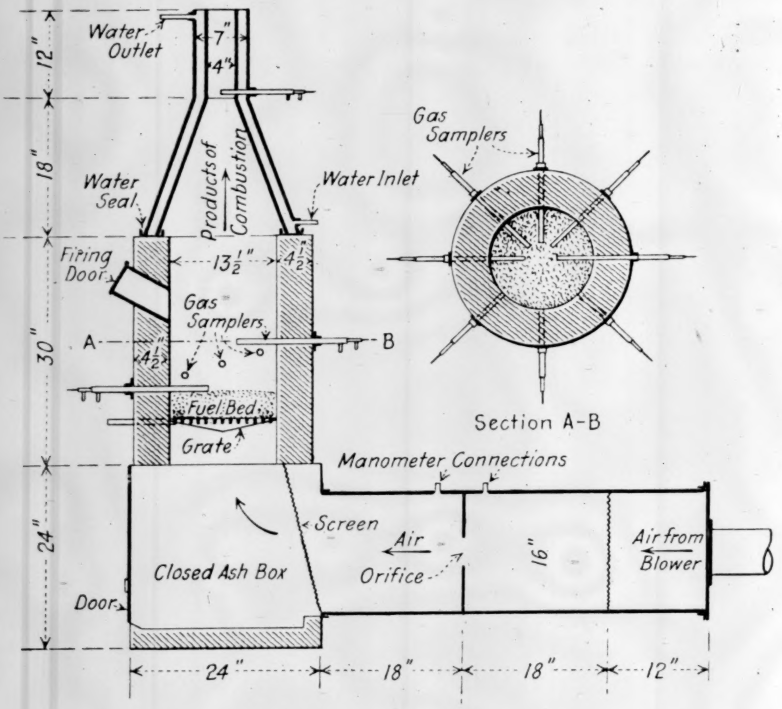
Water Outlet
7"
4"
12"
18"
Products of Combustion
Water Seal
Water Inlet
Gas Samplers
Firing Door
13½"
4½"
Gas Samplers
A
B
30"
4½"
Fuel Bed
Grate
Section A-B
Manometer Connections
Screen
Air
Orifice
16"
Air from Blower
Closed Ash Box
Door
24"
24"
18"
18"
12"

FIG. 4.—Cross-Section of Experimental Furnace.

According to Grüner,[1] most efficient operation is obtained when all the ore is reduced by "indirect reduction," that is, by carbon

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[1] M. L. Grüner, "Analytische Studien über den Hochofen," Wiesbaden, C. W. Kreidels Verlag (1875).

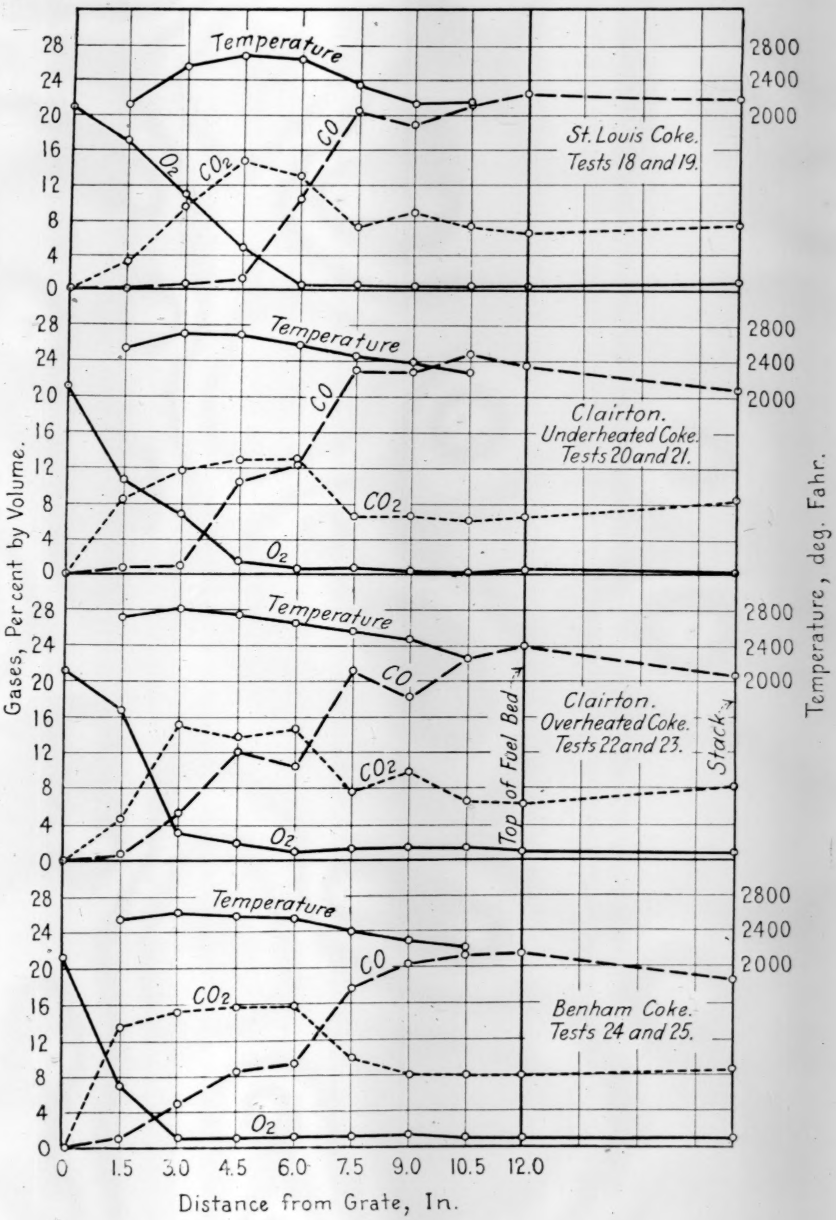

FIG. 5.—Average Composition of Gases and Temperatures in Fuel Beds of St. Louis, Clairton, and Benham Cokes

monoxide, and thus all the carbon charged (with the exception of that required to carburize the iron and reduce oxides of silicon, manganese, and phosphorus) burned at the tuyeres. This condition of operation has been referred to as Grüner's "ideal working."

Oxidation of carbon by solid oxides or by carbon dioxide in the stack is evidenced by a decreasing percentage of nitrogen in the stack gases. Dry air burns pure carbon according to the equation:

$$2C + O_2 + 3.78\ N_2 = 2CO + 3.78\ N_2$$

The resulting gases of combustion, therefore, consist of

$$\frac{2}{5.78} = 34.6 \text{ per cent } CO \text{ and } \frac{3.78}{5.78} = 65.4 \text{ per cent } N_2$$

Factors which diminish the percentage of nitrogen in the stack gas are:

1. Direct reduction:

$$FeO + C = Fe + CO$$
$$SiO_2 + 2C = Si + 2CO, \text{ etc.}$$

2. Oxidation of carbon by CO_2 formed above the tuyeres:

$$(a)\ FeO + CO = Fe + CO_2$$
$$(b)\ CO_2 + C = 2CO$$

3. Oxidation of carbon by water vapor:

$$H_2O + C = CO + H_2$$

4. Decomposition of limestone:

$$CaCO_3 = CaO + CO_2$$

5. Evolution of volatile matter from fuel.

The first two factors account for the greater part of the dilution of nitrogen. The stack gas of the modern American blast furnace analyzes 59 to 56 per cent of nitrogen, indicating an amount of carbon oxidized above the tuyeres of 20 to 30 per cent of the total carbon charged as coke (assuming 2 per cent of hydrogen). It is impossible to distinguish between "direct reduction" and "solution loss" without making certain assumptions[1] of doubtful validity.

A laboratory test for determining the relative reactivity of metallurgical cokes with carbon dioxide has been devised by one of the authors, and is illustrated in Fig. 6. Carbon dioxide from the tank *A* is passed through the quartz tube *J* (1-in. internal diameter) at a rate of 1 liter per minute for one hour. A 2-in. layer of 6 to 8-mesh

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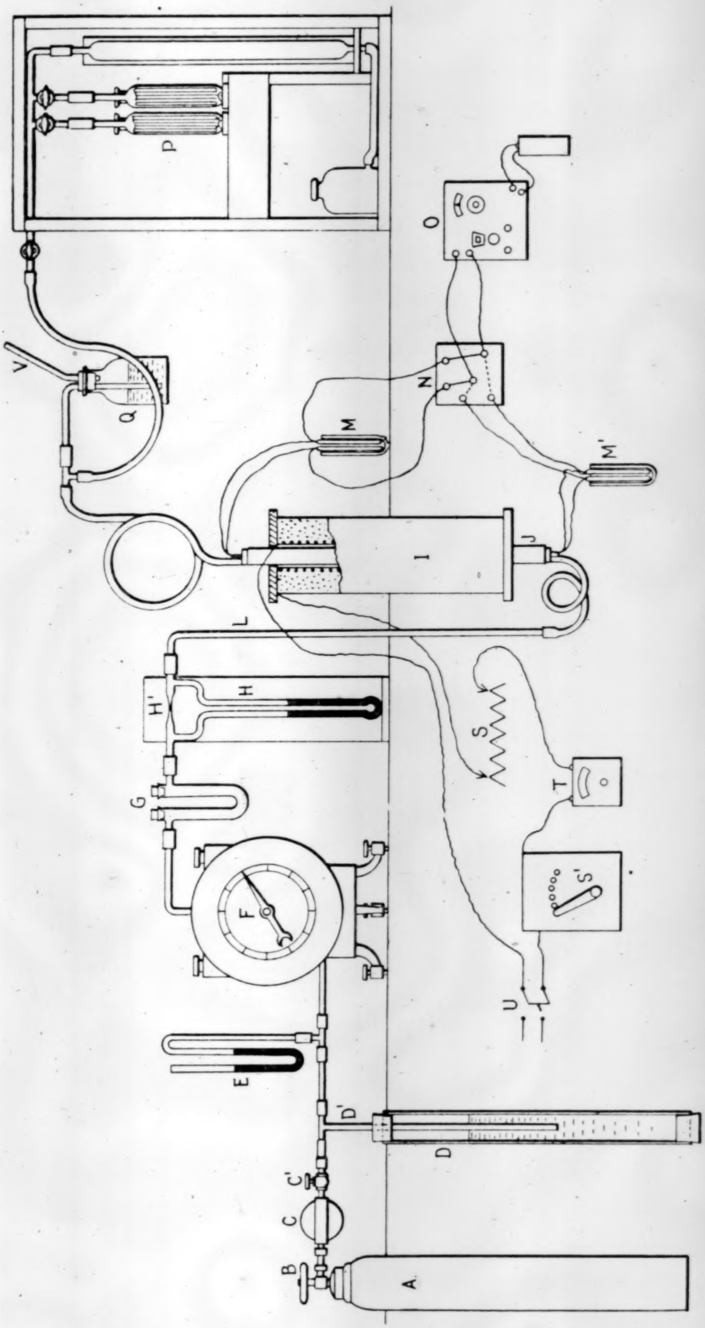
[1] Walther Mathesius, *Stahl und Eisen*, Vol. 30, p. 31 (1916).


A
B
C
C'
D
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H'
H
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J
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U

FIG. 6.—Apparatus for Determining Rate of Oxidation of Cokes by Carbon Dioxide.

coke is used. The temperature is kept at 1000° C. and is measured by thermocouples at the bottom and top of the coke layer. Analyses of the gas are made every five minutes for the purpose of following the course of the reaction. The result is reported as grams of carbon oxidized during the test. A comparison of the test results with actual operating data is shown in Table IV.

The data show a considerable difference in the reactivity of the cokes as measured by the laboratory test, but do not indicate any definite relation between this value and the percentage of carbon oxidized above the tuyeres. Charcoal, for example, shows a weight loss by the laboratory test 10 times that of Connellsville coke, but shows a low percentage of carbon oxidized above the tuyeres in actual furnace operation.

TABLE IV.—RELATIVE REACTIVITY OF COKES WITH CARBON DIOXIDE

Furnace	Type of Coke-oven	Coal		Tons of Iron per Day	Coke per Ton of Iron	Carbon Burned at Tuyeres per 100 lb. Carbon Charged as Coke	True Density of Coke	Apparent Density	Porosity	Loss by CO_2 on Standard Test	
		State	Seam							Grams	Relative
1	BH	Pennsylvania	Pittsburgh	220	2800	75	1.89	1.02	46	0.7	10
2	BH	Alabama	Pratt	322	2807	76.5	2.13	1.15	46	0.9	13
3	BH	Pennsylvania	Pittsburgh	580	1960	77	1.90	0.98	48	0.9	13
4	SS	Alabama	Brookwood and Meldale	258	3160	78	2.00	0.99	51	1.1	16
5	SS	Alabama	Mary Lee	260	2800	72	1.96	1.00	49	1.1	16
6	SS	Alabama	Mary Lee	356	3345	77	1.92	1.05	45	1.1	16
7	K	Alabama	Pratt	418[a]	1900	78	2.04	1.14	44	1.2	17
8	K	Alabama	Pratt	476	2750	78	2.01	1.13	44	1.2	17
9	K	Pennsylvania	Pittsburgh	512	1960	76	1.90	0.98	48	1.3	19
10	K	Pennsylvania	Pittsburgh	525	2100	78	1.99	1.04	48	1.5	21
11	R	Illinois	No. 7	515	1770	73	1.84	0.87	53	3.0	43
Charcoal Furnace[b]	..	Herrang, Sweden	Charcoal[c]	..	1600	79	...	...	..	7.0[c]	100
Charcoal Furnace	..	Alabama	Charcoal	50	2080	78	...	...	..	7.0	100

[a] 45 per cent scrap.

[b] J. W. Richards, "Metallurgical Calculations," second edition, p. 246 (1916).

[c] Average of several samples of wood charcoal made in the U. S. A.

CONCLUSIONS

1. The discussion shows that, aside from chemical analysis, we are not at present in a position to write specifications for metallurgical coke. If the properties of cokes being used successfully in this country be taken as criteria, we might say that any coke having an ash content of less than 13 per cent, sulfur less than 1.25 per cent, true specific gravity over 1.80, porosity less than 55 per cent, and a shatter test over 40 per cent will be satisfactory for blast-furnace use.

This does not mean that cokes falling outside the limiting physical properties will necessarily be unsatisfactory.

2. It has been shown by tests in the laboratory and at blast furnaces that combustibility of coke at the tuyeres of the furnace is practically a constant quantity, independent of the character of the coke. A laboratory test for determining the relative rate of oxidation of cokes by carbon dioxide has been developed. No relation between results of this test and the percentage of carbon oxidized above the tuyeres in actual practice has been as yet discovered, but further comparative data are needed. The effect of the size of coke on blast-furnace operation should be studied.

3. In testing coke the following determinations should be made:

(*a*) Percentage of various sizes as determined by screen test;

(*b*) Complete chemical analysis, including analysis of ash;

(*c*) Determination of true and apparent specific gravity, porosity, and weight per cubic foot of loose coke as delivered to the furnace;

(*d*) Shatter test (three to five duplicate determinations); and

(*e*) Relative rate of oxidation by carbon dioxide, using standard conditions similar to those described in this paper.

The meaning of (*a*), (*c*), (*d*), and (*e*), in terms of blast-furnace operation is not definitely known. It can be said, however, that these tests measure quantitatively certain properties of coke which vary over a fairly wide range; furthermore, it is reasonably certain that they measure all the properties of coke which can influence blast-furnace operation.

4. The difficulty of judging the efficiency of various cokes by making use of data taken from different blast furnaces has been indicated. Tests at one furnace under carefully controlled conditions are needed, and it is hoped that the proposed work at the experimental blast furnace of the Bureau of Mines at its Minneapolis Station will throw additional light upon the subject.

DISCUSSION

Mr. Haven

MR. WILLIAM A. HAVEN[1] (*presented in written form*).—It was with a certain amount of hesitancy that I accepted the invitation to discuss this excellent paper, as I realized the time would be entirely too short for properly aligning the new data submitted with the thousand and one ideas that every furnaceman has about the properties of coke. The U. S. Bureau of Mines has gone into this subject with a thoroughness never hitherto conceded it, and the results of their laboratory tests, together with the other data assembled, would seem to cover the subject thoroughly. Perhaps, however, with the point of view of the furnace operator, I may add a little to the information presented.

While there is a lamentable lack of standards for the classification of coke, it should not be assumed that coke good for one furnace will be satisfactory for all others. The size of the furnace, the quality of the ore mixture, the grade of iron to be produced, and other variable conditions are factors which do or should influence the selection of blast-furnace fuel. There are grades of coke good for some furnaces but not suitable for others. Brassert found the light, highly combustible Pocahontas coke much better for small than for large furnaces. Foundrymen, it is said, like the hard dense fuel which furnacemen destest.

There are, however, a number of things which are almost always desirable in blast-furnace fuel, most of which were mentioned by the authors but to which I would add low percentage of moisture and phosphorus. A high moisture content generally indicates poor physical structure or improper quenching and, somewhat aside from the subject perhaps, has an important bearing on the monetary value of the fuel. The water that gets into the coke before it passes to the railroad scales is particularly obnoxious to furnacemen. The matter of phosphorus content also becomes one of importance when the production of bessemer pig iron is involved. A bessemer ore mixture may sometimes be materially cheapened by the use of low phosphorus coke.

On the subject of combustibility I am obliged to disagree with the conclusion that this distinctly variable characteristic of coke plays a negligible part in the operation of the furnace. Furnacemen have observed for years that the rate of driving is strongly influenced by a certain quality in the fuel which they have designated, reasonably enough, as its combustibility. Exceptional work in securing good tonnages with low coke consumption has time and again been attributed by the operators in charge to the fast burning quality of their coke. The results obtained from Pocahontas Beehive, Benham Beehive, and Granite City coke from Roberts ovens are outstanding examples. **Mr. Haven**

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[1] Superintendent Northern Furnaces, Republic Iron and Steel Co., Youngstown, Ohio.

In 1915, I was operating a small furnace in Southern Ohio. Our coke contract with the Semet Solvay people expired at the end of the year and while negotiations for its renewal were going on we were obliged to buy spot coke wherever obtainable. We first tried some Beehive coke from the Toms Creek district of West Virginia. This coke was very dense and hard and when it reached the tuyeres the blast pressure started up immediately. Within a short time the pressure capacity of our blowing engines had been reached, but the furnace steadfastly refused to take the wind. The usual list of remedial measures, such as coke blanks, tuyere changes, changes in filling and so on, was gone through from beginning to end, but to no avail. After a big loss in tonnage and the disabling of a blowing engine the use of Toms Creek coke was given up and a supply of Pocahontas Beehive was bought. To this wonderful fuel the response of the furnace was amazing. The blast pressure receded and driving was resumed at so fast a rate that only prompt charging of extra coke prevented the furnace running cold. After getting the furnace properly adjusted to this fuel we proceeded to boost our tonnage record from 313 to 356 tons daily for a month's period. If the combustibility of coke at the tuyeres of the furnace is a constant, how can such an experience be explained?

Every furnaceman knows that large blocky coke slows up a furnace and at by-product plants the large pieces are generally broken down by rough handling during screening, the reasoning, of course, being that a larger percentage of coke will be exposed to the blast. For the same reason a light fuel should burn faster than a heavy one if the pieces are of the same average size. Open cell structure should also promote fast burning. By a simple laboratory test which we developed during 1921,[1] we confirmed such a relationship between specific gravity, cell structure and rate of combustion. Our results indicate also that combustibility is probably affected by the amount of ash in the coke and perhaps most of all by the structure of the cell

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[1] *Iron Age*, September 14, 1922.

Mr. Haven walls. The burning speed of coke will be limited by the coal mixture, but may be influenced by coking methods and by sizing.

TABLE I.—RELATIVE COMBUSTIBILITY OF FUELS

	Charcoal			86.22
94 per cent	Benham	By-Product	Chicago	41.66
	Granite City	By-Product	St. Louis	39.39
85 per cent	Elkhorn	By-Product	Portsmouth	38.96
91 per cent	Klondike	By-Product	Youngstown	35.50
91 per cent	Freeport	By-Product	Youngstown	35.00
100 per cent	Klondike	Beehive	Republic	33.13
	Retort Carbon			26.83

The five tests for coke recommended by the Bureau of Mines are well suited to research work but several of them are too complicated or laborious for routine purposes. For such work the tests should be as simple as possible and the results stated in terms that may be easily applied to blast-furnace practice. For the hardness test we have used the tumbling barrel for several years and prefer it because the apparatus is simple and duplicate determinations are not necessary. It is true that an occasional screen analysis after tumbling is essential but this itself furnishes a good index of the ability of the coke to resist abrasion.

Our laboratory combustion test has proved to be practical and inexpensive and we are confident that the results obtained will reflect any great changes in density, cell structure, chemical activity of the coke substance, or anything affecting its combustibility, except the matter of size. At the Haselton Furnaces in Youngstown the complete order of testing is as follows:

1. Sizing tests daily.
2. Proximate analysis daily, including moisture determination.
3. Complete chemical analysis, semi-weekly accumulated sample.
4. Hardness test, semi-weekly, with occasional screen analysis.
5. Combustion test, semi-weekly, accumulated sample.
6. Monthly calculation of weight per cubic foot from number of charges and car weights.

It must be remembered that different furnace conditions require different grades of coke. The requirements of each furnace or group of furnaces is a separate problem in itself. But in terms of the tests just given, for our Haselton furnaces we would like to have the following kind of coke:

1. Size to vary with combustibility—100 per cent through the 4-in. but as much on the 2-in. screen as combustibility will permit, everything under $\frac{1}{2}$-in. removed.

Mr. Haven

2 and 3. Moisture, ash, sulfur and phosphorus as low as possible.
4. Hardness between 55 and 65, with satisfactory screen analysis.
5. Combustibility about 40.
6. Weight per cubic foot as low as possible for the desired size.

Mr. Malleis

MR. O. O. MALLEIS[1] (*presented in written form by A. R. Powell*).—This paper summarizes some very interesting and useful investigations conducted by the U. S. Bureau of Mines on metallurgical coke. The attempts to standardize the methods by which the properties of metallurgical coke may be studied and compared will, in many cases, be of particular value. However, some rather positive statements regarding coke combustibility appear in this paper, and these assertions call for some comment.

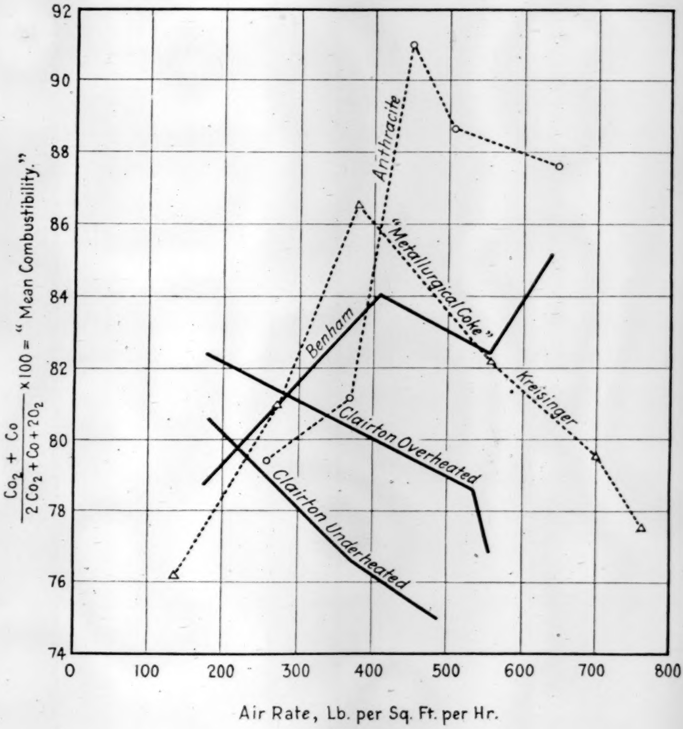

FIG. 1

The majority of American blast furnace and coke oven authorities entertain an opinion of the importance of coke combustibility quite

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[1] Assistant Chief Chemist, The Koppers Co. Laboratories, Mellon Institute, Pittsburgh, Pa.

Mr. Malleis at variance with that expressed by the authors of this paper, and while the opinion of a majority may indeed not be infallible, it has in this case certainly not been controverted by the data presented. Few will agree with the conclusion 2, "It has been shown by tests in the laboratory and at blast furnaces that combustibility of coke at the tuyeres of the furnace is practically a constant quantity, independent of the character of the coke."

Examining the laboratory tests in detail as described by Sherman and Blizard,[1] and disregarding the considerable variations in the analyses which might cast doubt on the general accuracy of the method employed, we find that the figure taken by the authors as the measure of combustibility varies with the air rate, that is, with the weight of air per square foot grate area per hour. The air rates used in the experiments range between 160 and 640 lb. per sq. ft. per hour. The air rate in ordinary blast furnace operation is from 750 to 1000 lb. per sq. ft. per hour. The diagram presented herewith (Fig. 1) has been plotted from the data given by Sherman and Blizard. The curves for "metallurgical coke" and "anthracite" being, however, drawn from Kreisinger's figures. This diagram shows that the "mean combustibility" as measured by the ratio

$$\frac{CO_2+CO}{2CO_2+CO+2O_2}\times 100$$

diverges greatly from the figures obtained at lower air rates as we approach those rates which are used in regular operation. Furthermore, the relative position of the different cokes may be altered, and there may be a much greater difference in their combustibility at the higher than at the lower rates. Now for some reason the authors in making their comparison of the different cokes did not select the high rates where differences in combustibility might be emphasized, but published only the figures calculated at the lower rates removed as far as possible from blast furnace operating conditions.

The irregularity of the data presented indicates that much more work is necessary before any definite conclusions can be drawn but the very fact that there appear to be such differences in the combustibilities of different cokes depending upon the air rate may be of some practical importance. There may be a particular rate at which each coke is most efficient, but the combustibility at this rate may be very different from the combustibility of a second coke at its most efficient rate.

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[1] "Combustion of Blast Furnace Cokes in Fuel Beds," Am. Inst. Mining and Metallurgical Engrs., February, 1923.

Mr. Malleis In regard to the tests made by the Bureau of Mines on blast furnaces, it is almost impossible to draw conclusions on account of the great variation in the operating conditions of the different furnaces and this is admitted in conclusion 4. The investigations of Howland[1] appear to be ignored. No explanation is given of the great differences in output of a blast furnace working on the same ores but with different cokes. The blast furnace man accounts for these differences by saying that one coke burns faster than another and this would seem to be reasonable. It will be hard to convince anyone who has actually watched through tuyere inspection holes the burning of coke in a blast-furnace hearth that there is not any considerable practical difference in the combustibility of different cokes.

A wrong inference might be drawn from the comparison between Connellsville beehive and by-product coke under the head of size. It has been the experience of our organization that in practically every plant where a change has been made from Connellsville beehive to by-product coke there has been an improvement in furnace working, an increase of output, and a decrease of coke consumption per ton of pig iron.

Credit should be given Mr. H. J. Rose, of the Mellon Institute Laboratories of The Koppers Company, for his suggestions in the presentation of this discussion.

Mr. Perrott Mr. G. St. J. Perrott.—I was much interested in Mr. Haven's remarks. My objection to combustibility tests of the type described by Mr. Haven is, as I discussed in our paper, that they do not measure combustibility in terms of blast-furnace conditions. In his test procedure, in which a definite weight of coke is used, the number of pieces of coke and hence the surface exposed and the amount of carbon burned in unit time will depend upon the apparent density. Furthermore, the temperature employed is much lower than that obtaining in the blast-furnace combustion zone. Variations in combustibility, not apparent under actual operating conditions, are often indicated by such a test. In the blast furnace, combustion takes place in a zone of definite dimensions and the surface exposed depends only on the size of the coke pieces. Furthermore, the temperature is around 1700° C. and the rate of oxidation of different cokes by CO_2 tends to become equal at such temperatures. Tests for combustibility should be carried out with a fuel bed of definite height and at temperatures as near as possible to those obtaining in the blast-furnace combustion zone.

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[1] *Transactions*, Am. Inst. Mining and Metallurgical Engrs., Vol. 56, p. 339 (1917).

Mr. Perrott Mr. Malleis cites Mr. Sherman's results and contends that relative combustibility of different cokes may change markedly at increased rates of burning. I agree with him that further work must be done before the question can be settled definitely. With the facts at present available, it is apparent that cokes vary much less in combustibility than was formerly supposed. Both Sherman and Kreisinger found small differences in the character of combustion at different rates of burning and with different fuels. Our work at the blast furnaces shows differences, but these are of the order of inches rather than feet. Considering that the blast-furnace hearth is from 14 to 20 ft. in diameter, it seems doubtful that such differences can have a large effect on furnace operation. The matter can not be definitely settled until work with different fuels has been carried out at one blast furnace under carefully controlled conditions.

A FUNGUS BED TEST OF WOOD PRESERVATIVES

By Cloyd M. Chapman[1]

In 1915, the writer presented a paper entitled "A Fungus Bed Test of Wood Preservatives,"[2] in which were described the methods used in the preparation of a series of test specimens in the laboratory of Westinghouse Church Kerr and Co., now Dwight P. Robinson and Co., Inc.

The treated specimens referred to in the previous paper have now been exposed in a fungus bed for about ten years and it is the purpose of this paper to give a record of the various preservatives used, and to present the results to date of their exposure.

Briefly, the procedure followed in preparing the specimens was as follows: Stakes of well air dried wood of four varieties, oak, cypress, spruce and yellow pine, were cut 1½ in. wide, ½ in. thick and 12 in. long, and pointed at one end. The preservatives were each applied by three methods, (*a*) cold dip for 15 seconds, (*b*) hot dip for one minute and (*c*) hot immersion (at about 100° C.) for one hour followed by cooling in the preservative. After treatment the specimens were hung up to drain and dry. Thirty preservatives were used, which may be classified as follows: fourteen were products of coal tar, three of water-gas tar, five of wood tar, one was mineral oil, three were vegetable or animal oils, and four were solutions of inorganic salts.

The exposure was out of doors in a fungus bed prepared by mixing decayed wood and dry pulverized sheep manure with well-sifted soil. The 12-in. stakes were inserted about 9 in. in the prepared soil. The intention was to provide an exposure comparable with that to which fence posts, telegraph and telephone poles, etc., are commonly subjected in ordinary service. At first, examinations were made at six months intervals, later one each year and finally, at two-year periods.

Preservatives

The record of the preservatives used, together with some of their physical and chemical characteristics, is given in Table I. It will be noted that there is quite a variety of coal, water-gas and wood-tar products ranging from very light to very heavy distillates. For

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[1] Consulting Engineer, New York City.

[2] *Proceedings*, Am. Soc. Testing Mats., Vol. XV, Part II, p. 267 (1915).

TABLE I.—PHYSICAL AND CHEMICAL CHARACTERISTICS OF PRESERVATIVES USED

Name of Preservative	Furnished by	Specific Gravity at 15.5° C.	Flash Point, deg. Cent.	Burning Point, deg. Cent.	Fractionation										Residue, including Loss, per cent and character	Tar Acids	Remarks
					Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent			
COAL-TAR CREOSOTES																	
English Creosote Oil........	Lemboke von Bermuth Co.	1.067	....	....	210	0.7	235	6.5	315	65.5	....	....	....	....	0.14 Coke	(1150 Soda) 8.5 per cent	Water 2 per cent Analysis by London Method. Chlorine nil. At 90° F. oil is fluid.
German Creosote Oil........	" " " "	1.0793	....	....	210	2.6	235	10.4	....	....	....	....	....	....	1.7	....	Cooled to 0° C. while stirred oil remains liquid and homogeneous.
Shingle Stain Oil............	Barrett Manufacturing Co.	0.9966	120	....	210	5.2	235	29.	270	74.1	315	94.4	355	98.9	....	None	
Dead Oil of Coal Tar........	" " "	1.059	....	....	....	....	....	....	....	....	....	....	....	....	....	....	
Oil No. 1 Average Coal-Tar Creosote	" " "	1.057	95	....	210	5.0	235	34.0	270	62.5	315	77.5	355	92.0	8 Soft Greasy Partly	11.5	Dry Naphthaline 7.3 per cent. 15 min. at 15.5° C.
Oil No. 2 High Boiling Coal-Tar Creosote	" " "	1.0828	110	....	210	1.	235	6.	270	32.7	315	58.2	355	86.7	13.3 Soft Greasy Partly	7.	Dry Naphthaline nil. 15 min. at 15.5° C.
Oil No. 3 Very High Boiling Coal-Tar Creosote	" " "	1.1308	141	....	210	0.	235	0.	270	0.4	315	17.1	355	69.3	30.7 Soft Greasy Partly	8.	Dry Naphthaline nil. 15 min. at 15.5° C.
Oil No. 4 Half Coal Tar and Half Coal-Tar Creosote	" " "	1.1116	100	....	210	3.	235	19.5	270	36.3	315	48.3	355	59.8	40.2 Soft Adhesive Pitch	9.	Dry Naphthaline nil. 15 min. at 15.5° C.
Oil No. 5 Coal Tar used in No. 4	" " "	1.1717	110	....	210	2.5	235	6.5	270	15.0	315	23.0	355	33.	67 Pitch	7.	Dry Naphthaline trace. 15 min. at 15.5° C.
15-per-cent Creosote Oil.....	Bayway Chemical Co....	0.9960	(Open Cup) 74.5	....	200	11.64	210	27.53	235	66.27	300	94.45	....	....	5.55	16.72	Analysis by M. of Way Method Water Trace.
50-per-cent Creosote Oil.....	" " "	1.0136	76.7	....	200	33.68	210	67.27	235	89.13	300	95.66	....	....	4.34	52.87	Water 0.11 per cent Anal. by M. of W. Method.

TABLE I.—PHYSICAL AND CHEMICAL CHARACTERISTICS OF PRESERVATIVES USED—*Continued.*

Name of Preservative	Furnished by	Specific Gravity at 15.5° C.	Flash Point, deg. Cent.	Burning Point, deg. Cent.	Fractionation										Residue, including Loss, per cent and character	Tar Acids	Remarks
					Up to —deg.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent			
COAL-TAR CREOSOTES																	
Avenarius Carbolineum......	Carbolineum Wood Preserving Co.	1.1286	140	170	250	not over 2.	300	not over 17	350	52. to 77.	....	.....	.....	....	23 to 50 Soft at at 20° C.	Not over 10 per cent of total distillate	
S. P. F. Carbolineum........	Bruno Groesche and Co...	1.1177	145	168	235	0.	300	6.8	330	34.1	360	68.99	.....	....	31	0.23	Ash 0.04 per cent. Insoluble in Benzole, 0.13 per cent.
Letteney..................	Northeastern Co.........	1.1193	145	.....	300	11.07	330	45.01	360	74.53	....	.....	.....	....	25 Fluid	0.22 per cent in fraction below 300° C.	Ash 0.01 per cent. Insoluble in Benzole, none.
WATER-GAS TAR CREOSOTES																	
"A" Oil..................	United Gas Improvement Co.	1.0503	72.3	.....	200	2.1	210	3.6	235	6.6	355	84.6	.....	....	15.2 8 per cent Coke	Nil	
"B" Oil..................	" " "	1.0499	71.1	.....	200	0.8	210	2.7	235	7.9	355	84.4	.....	....	15.0 69 per cent Coke	2.2	
"C Oil..................	" " "	1.1223	82.3	.....	200	0.6	210	0.9	235	3.0	355	50.2	.....	....	49.5 33.7 per cent Coke	Nil	
WOOD TAR CREOSOTES																	
No. 244 Low Boiling........	Lyster Chemical Co......	1.097	100	105.6	210	12.	235	40	270	72	290	90	....	....	....	....	
No. 49 Medium Boiling.....	" " "	1.097	100	106.7	210	4	235	65	270	96	....	.....	....	....	....	....	
No. 134 High Boiling........	" " "	1.051	107.8	107.8	210	2	235	20	270	62	290	77	315	90	....	....	

TABLE I.—PHYSICAL AND CHEMICAL CHARACTERISTICS OF PRESERVATIVES USED—*Continued.*

Name of Preservative	Furnished by	Specific Gravity at 15.5° C.	Flash Point, deg. Cent.	Burning Point, deg. Cent.	Fractionation										Residue, including Loss, per cent and character	Tar Acids	Remarks
					Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent	Up to —deg. Cent.	Per Cent			
WOOD TAR CREOSOTES																	
Spirittine Wood Preservative	Spirittine Chemical Co....	1.0024	77.8	93.4	315.5	37.4	above 315.5	52.7	...	...	...	...	...	...	10 per cent Gas and Coke	...	
Spirittine Wood Preservative Mercury	" " " ...	1.0123	77.8	93.4	315.5	37.4	315.5	52.7	...	...	...	...	...	...	10 per cent Gas and Coke	...	See Note
MINERAL OIL																	
Petroleum Tar..............	Santa Fe R. R..........	0.9839	130	...	210	0.3	235	0.6	270	2.4	315	11.6	355	29.	...	...	Distillation by Std. Retort Method of A. R. E. A.
VEGETABLE AND ANIMAL OILS																	
Raw Linseed Oil............	National Lead Co........	0.9308	...	...	...	...	...	...	...	...	...	...	...	...	...	...	
Raw Linseed Oil and Iron Oxide Pigment	National Lead Co., Prince's	0.9671	...	...	...	...	...	...	...	...	...	...	...	...	...	...	
Light Pressed Fish Oil.......	N. B. Cook Oil Co.......	0.9267	...	...	...	...	...	...	...	...	...	...	...	...	...	...	
INORGANIC SALTS IN SOLUTION																	
2-per-cent Zinc Chloride.....	Made up in W. C. K. Laboratory	1.0203	...	...	...	...	...	...	...	...	...	...	...	...	...	...	
5-per-cent Zinc Chloride.....	" " " "	1.0449	...	...	...	...	...	...	...	...	...	...	...	...	...	...	
Sodium Zincate............	Bruno Groesche and Co..	1.0478	...	...	...	...	...	...	...	...	...	...	...	...	...	...	
"Jolanin" Fluoric Acid......	" " " ..	1.0788	...	...	...	...	...	...	...	...	...	...	...	...	...	...	

NOTE.—This sample was labeled by the manufacturers "Spirittine Wood Preservative, Mercury" in contradistinction to the other sample submitted by them marked "Spirittine Wood Preservative." A request was made for information as to the difference between the two materials, and as to the form and amount of the mercury content of this one, but the makers declined to supply further data instance, the coal-tar creosotes range from one which distilled 94.4 per cent at 315° C. up to one which distilled only 17.1 per cent at the same temperature.

Some of these were also used as mixtures of two materials, such as coal-tar and coal-tar creosote.

There was also quite a range in the distillation characteristics of the wood-tar creosotes.

QUANTITY OF PRESERVATIVE ABSORBED

The various preservatives were absorbed into the specimens in varying amounts. The differences in the porosity of the wood of the specimens probably had a very decided influence upon the amount of preservative absorbed as well as the varying penetrating power of the preservatives. To preserve a record of this variation, each specimen was weighed carefully just before and after treatment, and the weight of the preservative taken up by the specimen was computed on the basis of pounds per cubic foot of wood. The results of these determinations are given in Table II.

A study of this table is quite interesting. It gives some idea of the relative eagerness of the common woods for taking up cold and hot liquids. But probably the most striking fact illustrated is the great difference between the amount of preservative absorbed by the different specimens of the same wood. However, the averages shown at the foot of the table are quite consistent and show that with each of the three methods of treatment, cypress absorbs the greatest quantity, followed by yellow pine, spruce and oak in the order named. As to the relative amount of preservatives absorbed in each of the methods of treatments, an index is given by the grand average of the individual averages for each preservative. This shows an average absorption for all specimens treated by cold dipping of 1.41 lb. per cu. ft. For the hot dip method the grand average is 1.51 lb. per cu. ft., while for all specimens boiled and cooled in the preservative the grand average is 12.46 lb. per cu. ft. Whether this amount of preservative is economical depends upon the cost of the preservative and of its application and the added life of the timber due to the greater quantity of preservative present in it. If eight or ten times as much preservative is required for the full cell treatment, the results in added life of the timber must be considerable to justify the greater expense.

TABLE II.—QUANTITY OF PRESERVATIVE ON EACH SPECIMEN

Expressed in Pounds per Cubic Foot

Name of Preservative	Furnished by	Cold-Dip Treatment						Hot-Dip Treatment						Boiling Treatment					
		Specimen No.	Oak A	Cypress B	Spruce C	Yellow Pine D	Average	Specimen No.	Oak A	Cypress B	Spruce C	Yellow Pine D	Average	Specimen No.	Oak A	Cypress B	Spruce C	Yellow Pine D	Average
COAL TAR CREOSOTES																			
English Creosote Oil........	Lemboke von Bermuth Co.........	88	1.1	1.	1.04	0.5	0.91	89	1.4	1.2	1.2	0.9	1.18	90	16.3	18.6	5.9	21.6	15.6
German Creosote Oil........	" " " "	85	1.2	0.9	2.3	1.66	1.52	86	1.9	2.9	1.	1.3	1.78	87	21.4	9.7	20.4	12.	15.9
Shingle Stain Oil............	Barrett Manufacturing Co.........	73	0.9	1.7	0.8	1.4	1.20	74	1.2	0.8	1.9	0.9	1.20	75	14.9	28.5	11.6	11.6	16.7
Dead Oil of Coal Tar........	" " "	4	1.4	3.2	0.6	0.4	1.40	7	0.6	0.8	0.7	0.5	0.65	8	26.6	17.2	8.3	11.8	15.9
Oil No. 1 Average Coal Tar Creosote............	" " "	25	0.8	1.7	0.8	0.8	1.03	26	1.1	1.2	1.1	1.5	1.23	27	17.9	20.4	9.6	4.7	13.2
Oil No. 2 High Boiling Coal Tar Creosote............	" " "	28	1.1	2.7	1.5	0.3	1.40	29	1.1	2.6	0.9	1.3	1.48	30	12.2	40.3	3.6	22.	19.5
Oil No. 3 Very High Boiling Coal Tar Creosote........	" " "	58	1.3	1.	2.5	1.2	1.50	59	1.5	2.	1.5	1.4	1.60	60	10.6	34.7	23.5	2.9	17.9
Oil No. 4 Half Coal Tar and Half Coal Tar Creosote ...	" " "	31	1.1	2.1	1.3	0.8	1.43	32	0.7	1.4	1.4	1.3	1.20	33	2.5	9.6	9.1	10.8	8.0
Oil No. 5 Coal Tar Used in No. 4................	" " "	34	2.1	2.6	2.4	1.8	2.23	35	2.7	2.2	1.5	1.5	1.98	36	2.5	11.4	7.	11.3	8.1
15-per cent Creosote Oil.....	Bayway Chemical Co.............	43	0.5	1.4	0.4	0.7	0.75	44	1.1	1.4	0.7	0.4	0.90	45	8.4	22.	4.1	13.2	11.9
50-per cent Creosote Oil.....	" " "	46	0.8	0.7	0.6	0.3	0.60	47	0.6	1.	5.7	5.5	3.28	48	3.6	10.	5.1	23.3	10.5
Avenarius Carbolineum......	Carbolineum Wood Preserving Co..	37	1.4	3.9	1.4	4.4	2.78	38	1.9	1.8	1.	4.8	2.38	39	7.1	42.4	7.	36.6	23.3
S. P. F. Carbolineum........	Bruno Groesche and Co...........	70	1.6	3.7	5.	1.4	2.99	71	2.1	1.8	1.7	1.1	1.68	72	17.2	11.7	6.4	7.7	10.8
Letteney................	Northeastern Co................	1	1.4	0.9	0.7	3.	1.50	2	1.5	1.8	0.9	2.4	1.65	3	3.4	1.5	3.9	1.7	2.6
WATER-GAS-TAR CREOSOTES																			
"A" Oil..................	United Gas Improvement Co......	76	0.9	1.2	1.	2.5	1.40	77	2.2	4.4	1.4	1.1	2.28	78	15.6	8.5	4.8	26.5	13.8
"B" Oil..................	" " " "	79	1.1	2.9	1.1	1.6	1.68	80	1.9	1.5	1.1	1.1	1.40	81	25.1	16.4	5.3	13.9	15.2
"C" Oil..................	" " " "	82	1.6	1.9	2.8	2.5	2.20	83	2.	3.5	2.2	2.9	2.65	84	3.8	10.9	18.	2.	8.7
WOOD-TAR CREOSOTES																			
No. 244 Low Boiling........	Lyster Chemical Co..............	16	1.4	2.2	1.5	0.9	1.50	17	1.	2.1	1.2	1.	1.33	18	5.1	7.8	6.8	13.7	8.3
No. 49 Medium Boiling	" " "	22	1.	2.4	1.6	3.3	2.08	23	1.1	2.3	1.1	0.6	1.23	24	5.5	37.5	2.3	1.8	11.8
No. 134 High Boiling........	" " "	19	1.6	3.4	1.1	1.	1.78	20	1.7	1.4	1.4	1.1	1.40	21	1.6	7.2	25.2	18.6	13.1
Spirittine Wood Preservative	Spirittine Chemical Co...........	13	1.2	0.9	1.8	1.	1.23	14	0.9	1.	1.2	1.9	1.25	15	10.7	9.9	27.8	2.6	12.8
Spirittine Wood Preservative Mercury................	" " "	5	1.4	2.6	1.2	1.8	1.75	9	1.4	3.1	1.3	1.1	1.73	10	2.9	10.	25.6	16.7	13.8
MINERAL OIL																			
Petroleum Tar.............	Santa Fe R. R..................	61	2.1	1.4	1.3	2.6	1.85	62	2.	2.7	2.2	1.6	2.13	63	2.7	6.	29.6	3.3	10.4
VEGETABLE AND ANIMAL OILS																			
Raw Linseed Oil............	National Lead Co...............	40	1.4	0.9	0.9	1.2	1.10	41	1.7	2.1	1.1	2.2	1.78	42	14.7	29.8	6.4	10.	15.2
Raw Linseed Oil and Iron Oxide Pigment...........	National Lead Co., Prince's.......	55	1.1	1.7	1.3	1.	1.28	56	1.7	1.6	1.	1.6	1.48	57	10.7	7.2	10.6	13.3	10.5
Light Pressed Fish Oil.......	N. B. Cook Oil Co..............	6	1.5	1.7	1.1	0.8	1.28	11	1.	1.7	1.3	3.3	1.83	12	3.1	23.7	15.4	17.5	14.9
INORGANIC SALTS IN SOLUTION																			
2-per cent Zinc Chloride.....	Made up in W. C. K Laboratory...	49	0.4	0.3	0.5	0.3	0.37	50	0.5	0.7	0.5	0.6	0.58	51	2.	31.6	28.5	1.8	15.9
-per cent Zinc Chloride.....	" " " " " ...	52	0.4	0.4	0.5	0.5	0.45	53	0.7	0.5	0.4	0.5	0.53	54	1.7	2.3	9.5	12.3	6.5
Sodium Zincate............	Bruno Groesche and Co..........	64	0.5	0.4	0.7	0.6	0.55	65	0.9	1.	0.8	0.8	0.88	66	3.1	2.	2.5	2.3	2.5
"Jolanin".(Fluoric Acid)....	" " " "	67	0.6	0.6	0.6	0.9	0.68	68	0.6	0.6	0.7	0.8	0.68	69	6.4	12.2	7.6	16.8	10.7
Average..................	....	....	1.16	1.74	1.34	1.37	1.41	....	1.35	1.77	1.33	1.56	1.51	....	9.31	16.70	11.71	12.14	12.46

TABLE III.—RATING OF TREATED SPECIMENS AT ANNUAL EXAMINATIONS.

Name of Preservative	Furnished by	Kind of Wood	Cold Dip Treatment								Hot Dip Treatment								Boiled Treatment							
			1914	1915	1916	1917	1918	1919	1921	1923	1914	1915	1916	1917	1918	1919	1921	1923	1914	1915	1916	1917	1918	1919	1921	1923
COAL TAR CREOSOTES																										
English Creosote Oil......	Lemboke von Bermuth Co.	Oak.........	100	90	70	40	30	0	..	..	100	90	65	40	40	0	..	..	100	100	90	90	90	90	90	80
		Cypress.....	100	100	95	90	90	80	60	50	100	100	100	60	40	40	30	0	100	100	100	100	100	100	90	80
		Spruce......	100	95	90	60	40	15	0	..	100	95	80	0	0	0	..	..	100	100	90	70	50	50	50	50
		Yellow Pine.	100	95	80	30	15	0	0	..	100	100	90	30	15	0	..	..	100	100	100	95	95	95	95	90
German Creosote Oil......	" " " "	Oak.........	100	90	70	60	60	40	30	0	100	90	80	70	70	50	50	30	100	85	80	80	80	80	80	80
		Cypress.....	100	100	100	70	70	60	50	40	100	100	100	95	80	60	50	50	100	90	90	90	90	90	90	80
		Spruce......	100	100	85	60	60	15	15	0	100	100	85	60	60	15	15	0	100	90	85	90	80	80	80	60
		Yellow Pine.	100	95	95	95	95	95	90	70	100	95	90	40	40	15	15	0	100	90	90	90	90	80	45	30
Shingle Stain Oil..........	Barrett Manufacturing Co.	Oak.........	100	95	60	50	30	15	15	0	95	80	60	30	30	0	0	0	100	85	75	70	70	50	45	40
		Cypress.....	100	100	95	80	60	40	15	0	100	100	95	90	70	50	50	50	100	100	100	95	95	80	75	60
		Spruce......	100	100	65	50	30	0	..	..	95	95	80	50	40	30	15	0	100	85	80	80	70	60	60	60
		Yellow Pine.	100	95	60	40	0	..	..	..	100	95	80	30	15	0	..	..	100	95	85	80	70	70	60	50
Dead Oil of Coal Tar.....	" " "	Oak.........	100	85	70	30	30	30	15	0	100	85	65	40	30	0	..	..	100	95	80	30	15	0	0	0
		Cypress.....	100	100	100	40	30	15	15	0	100	100	100	70	50	50	30	..	100	100	100	90	80	80	70	60
		Spruce......	100	100	80	40	15	0	..	..	100	100	75	0	0	0	..	..	100	100	95	80	70	70	70	60
		Yellow Pine.	100	100	80	30	15	0	..	..	100	95	80	30	15	0	..	..	100	100	100	90	70	60	50	50
Oil No. 1 Average Coal-Tar Creosote	" " "	Oak.........	100	85	70	40	15	0	..	..	100	95	80	60	50	15	10	0	100	100	90	80	70	70	70	70
		Cypress.....	100	100	100	95	90	80	70	70	100	100	95	60	40	40	30	0	100	100	95	95	90	90	70	70
		Spruce......	100	100	80	50	40	15	15	0	100	100	80	30	30	0	..	..	100	100	90	90	80	70	70	60
		Yellow Pine.	100	100	90	60	40	40	30	30	100	100	85	50	40	15	10	0	100	100	85	60	50	50	45	40
Oil No. 2 High Boiling Coal-Tar Creosote	" " "	Oak.........	100	90	80	50	40	30	15	0	100	85	80	50	40	40	15	0	100	90	85	80	80	70	70	60
		Cypress.....	100	100	100	100	95	80	80	60	100	100	100	95	90	90	90	60	100	100	100	100	100	100	100	90
		Spruce......	100	100	85	60	40	0	..	..	100	100	85	60	60	50	40	30	100	100	95	70	60	40	30	0
		Yellow Pine.	100	100	85	60	40	30	20	0	100	100	90	60	40	0	..	..	100	100	95	90	70	70	70	60
Oil No. 3 Very High Boiling Coal-Tar Creosote	" " "	Oak.........	100	100	85	60	40	15	15	0	100	90	80	70	50	0	..	..	100	90	85	80	70	70	70	70
		Cypress.....	100	100	100	95	80	70	70	50	100	95	95	95	80	80	80	70	100	100	100	100	100	100	100	100
		Spruce	100	100	90	80	40	15	15	0	100	95	85	70	60	15	15	0	100	100	100	90	90	90	90	70
		Yellow Pine.	100	100	90	70	40	30	30	0	100	100	85	60	60	15	15	0	100	100	95	70	60	50	50	50
Oil No. 4 Half Coal Tar and Half Coal-Tar Creosote	" " "	Oak.........	100	90	80	40	30	0	..	..	100	80	70	40	40	15	15	0	100	95	85	70	70	70	70	50
		Cypress.....	100	100	100	95	80	70	70	70	100	100	100	100	100	95	70	40	100	100	100	100	100	100	100	100
		Spruce......	100	100	95	0	..	..	..	..	100	100	85	40	30	0	..	..	100	100	90	80	70	70	60	50
		Yellow Pine.	100	100	85	50	40	30	25	0	100	100	85	50	30	0	..	..	100	100	95	90	90	70	50	50
Oil No. 5 Coal Tar Used in No. 4	" " "	Oak.........	100	100	85	70	70	60	40	0	100	100	95	70	70	70	30	0	100	100	100	100	95	90	70	50
		Cypress.....	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
		Spruce......	100	100	100	90	60	15	0	..	100	100	100	90	70	40	30	0	100	100	100	90	90	70	60	40
		Yellow Pine.	100	100	100	100	80	60	50	50	100	100	100	80	60	40	35	35	100	100	100	100	100	100	90	80
15-per-cent Creosote Oil...	Bayway Chemical Co....	Oak.........	95	80	55	15	15	0	..	..	90	80	45	15	15	0	..	..	100	90	70	50	50	50	50	40
		Cypress.....	95	95	90	80	70	50	20	20	100	100	90	90	80	70	40	40	100	100	90	70	50	50	45	30
		Spruce......	95	90	65	40	30	0	..	..	95	90	65	40	15	0	..	..	100	90	75	60	40	30	15	15
		Yellow Pine.	95	95	65	40	15	0	..	..	100	95	70	50	50	50	50	40	100	95	80	60	40	40	40	40
50-per-cent Creosote Oil...	" " ".....	Oak.........	100	70	40	0	..	..	..	..	100	75	50	0	..	..	..	..	100	85	80	50	40	0	..	..
		Cypress.....	100	100	90	70	70	40	35	35	100	100	90	70	70	50	45	30	100	100	90	70	70	70	60	50
		Spruce......	100	90	75	0	..	..	..	..	100	95	70	40	30	0	..	..	100	90	80	60	30	15	15	0
		Yellow Pine.	100	90	70	40	15	0	..	..	100	85	35	0	..	..	..	..	100	95	70	40	15	0	..	..

TABLE III.—RATINGS OF TREATED SPECIMENS AT ANNUAL EXAMINATIONS—*Continued.*

Name of Preservative	Furnished by	Kind of Wood	Cold Dip Treatment								Hot Dip Treatment								Boiled Treatment							
			1914	1915	1916	1917	1918	1919	1921	1923	1914	1915	1916	1917	1918	1919	1921	1923	1914	1915	1916	1917	1918	1919	1921	1923
Avenarius Carbolineum	Carbolineum Wood Preserving Co.	Oak	100	95	90	70	60	60	40	30	100	100	90	70	70	40	15	0	100	100	85	80	70	70	70	70
		Cypress	100	100	100	95	90	80	80	50	100	100	100	95	95	95	90	80	100	100	100	100	100	100	100	100
		Spruce	100	100	85	70	50	15	15	0	100	100	95	80	70	40	15	0	100	100	100	100	95	80	80	80
		Yellow Pine	100	95	80	60	30	0	..	..	100	100	90	60	60	30	25	0	100	100	100	100	95	80	75	70
S. P. F. Carbolineum	Bruno Groesche and Co.	Oak	100	95	85	60	60	50	30	0	100	100	90	95	70	70	50	50	100	100	90	80	80	80	80	80
		Cypress	100	100	100	100	100	95	95	70	100	100	100	100	95	70	70	60	100	100	95	100	100	100	100	100
		Spruce	100	85	70	80	40	30	30	30	100	100	70	60	60	15	15	0	100	100	100	100	80	80	80	80
		Yellow Pine	100	70	55	40	30	0	..	..	100	80	70	60	60	30	30	30	100	100	100	90	80	70	70	70
Letteny	Northeastern Co.	Oak	100	90	85	70	70	60	50	0	100	100	85	70	70	70	40	30	100	100	90	90	80	80	40	20
		Cypress	100	100	100	90	90	70	60	50	100	100	95	90	90	80	80	70	100	100	100	90	70	60	50	50
		Spruce	100	100	90	60	40	40	40	40	100	100	95	70	50	40	40	40	100	100	95	70	70	60	45	40
		Yellow Pine	100	100	85	0	..	..	..	..	100	100	90	0	..	..	..	..	100	100	90	50	40	40	15	0
WATER-GAS TAR CREOSOTES																										
"A" Oil	United Gas Improvement Co.	Oak	100	100	75	60	50	50	15	0	100	85	75	60	60	50	30	0	100	90	85	90	70	70	70	70
		Cypress	100	100	100	80	80	70	40	40	100	100	100	90	70	60	40	40	100	100	100	100	100	95	90	80
		Spruce	100	100	80	70	60	40	15	0	100	100	75	90	70	60	40	40	100	100	90	90	80	60	60	60
		Yellow Pine	100	95	70	50	30	0	..	..	100	100	85	50	30	0	..	..	100	100	100	90	90	70	70	70
"B" Oil	" " "	Oak	100	90	80	70	70	50	30	0	100	90	75	70	60	50	15	0	100	95	80	90	80	80	80	80
		Cypress	100	100	100	100	90	80	70	70	100	95	95	95	80	60	60	45	100	100	100	95	80	80	60	60
		Spruce	100	100	80	50	50	30	30	0	100	95	80	70	40	30	15	0	100	95	90	80	70	60	50	50
		Yellow Pine	100	90	75	70	60	50	15	0	100	100	85	40	40	15	15	0	100	100	95	80	80	50	45	45
"C" Oil	" " "	Oak	100	95	80	40	40	40	0	..	100	90	65	40	30	0	..	..	100	100	95	95	70	60	40	30
		Cypress	100	100	100	100	100	100	100	90	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
		Spruce	100	100	100	90	80	50	15	0	100	100	100	60	40	30	30	0	100	100	100	80	60	50	50	40
		Yellow Pine	100	100	100	80	40	30	30	0	100	100	100	70	0	..	..	..	100	100	80	0	..	..	..	..
WOOD TAR CREOSOTES																										
No. 244 Low Boiling	Lyster Chemical Co.	Oak	100	80	55	15	15	0	..	..	100	75	70	40	30	15	0	..	100	100	80	60	60	60	15	0
		Cypress	100	100	100	40	40	40	15	0	100	95	95	70	15	0	..	..	100	100	100	80	70	70	60	50
		Spruce	100	90	70	0	..	..	..	..	100	95	75	0	..	..	..	..	100	100	85	30	15	0	..	..
		Yellow Pine	100	100	80	0	..	..	..	..	100	100	80	0	..	..	..	..	100	100	90	80	70	60	30	0
No. 49 Medium Boiling	" " "	Oak	100	80	60	30	30	0	..	..	100	85	55	40	30	0	..	..	100	85	70	40	40	40	15	0
		Cypress	100	100	100	70	40	0	..	..	100	100	95	60	15	0	..	..	100	100	100	90	90	80	80	80
		Spruce	100	100	75	0	..	..	..	..	100	95	70	40	30	0	..	..	100	100	80	60	40	0	..	..
		Yellow Pine	100	100	60	0	..	..	..	..	100	100	75	50	40	15	15	0	100	100	85	50	40	40	30	30
No. 134 High Boiling	" " "	Oak	100	85	55	40	30	0	..	..	100	90	70	40	30	0	..	..	100	85	70	40	15	15	15	0
		Cypress	100	100	100	90	70	60	30	0	100	100	100	60	50	50	35	30	100	100	100	90	90	80	70	70
		Spruce	100	100	80	30	30	0	..	..	100	100	70	15	0	..	..	..	100	100	90	60	50	40	15	15
		Yellow Pine	100	100	80	0	..	..	..	..	100	100	90	60	40	40	30	0	100	100	95	80	40	30	15	15
Spirittine Wood Preservative	Spirittine Chemical Co.	Oak	100	85	75	50	50	15	15	0	100	80	75	60	40	40	15	0	100	90	80	60	60	60	50	40
		Cypress	100	100	100	60	60	15	15	0	100	100	95	70	60	60	45	40	100	100	100	90	90	80	80	80
		Spruce	100	100	90	30	30	0	..	..	100	100	80	0	..	..	..	..	100	100	90	70	60	40	40	0
		Yellow Pine	100	100	75	50	15	0	..	..	100	100	80	0	..	..	..	..	100	100	90	50	40	30	15	0

TABLE III.—RATINGS OF TREATED SPECIMENS AT ANNUAL EXAMINATIONS—*Continued.*

Name of Preservative	Furnished by	Kind of Wood	Cold Dip Treatment								Hot Dip Treatment								Boiled Treatment							
			1914	1915	1916	1917	1918	1919	1921	1923	1914	1915	1916	1917	1918	1919	1921	1923	1914	1915	1916	1917	1918	1919	1921	1923
Spirittine Wood Preservative Mercury	Spirittine Chemical Co.	Oak	100	80	75	50	30	30	15	0	100	80	75	60	40	40	35	0	100	90	75	60	60	50	15	0
		Cypress	100	100	100	95	90	80	50	0	100	100	100	70	50	40	30	0	100	100	100	90	90	90	90	90
		Spruce	100	100	85	0	..	..	..	..	100	100	75	0	..	..	..	..	100	100	95	70	60	40	40	40
		Yellow Pine	100	100	80	0	..	..	..	..	100	100	90	0	..	..	..	..	100	95	95	50	50	40	15	0
MINERAL OIL																										
Petroleum Tar	Santa Fe R. R.	Oak	95	80	70	60	40	0	..	..	95	85	70	60	30	15	15	0	100	80	65	40	30	0	..	..
		Cypress	100	100	100	90	90	80	60	60	100	100	100	95	95	80	70	70	100	100	100	95	95	95	90	90
		Spruce	100	95	75	60	60	15	15	0	100	95	70	60	15	0	..	..	100	100	90	90	90	80	70	70
		Yellow Pine	95	95	75	60	60	0	..	..	100	100	80	60	40	40	30	30	100	100	95	95	90	80	60	60
VEGETABLE AND ANIMAL OILS																										
Raw Linseed Oil	National Lead Co	Oak	95	75	60	50	40	0	..	..	100	70	60	15	15	0	..	..	100	75	70	60	40	30	15	0
		Cypress	95	100	95	80	50	50	40	40	100	100	80	50	50	15	15	15	100	100	95	90	70	50	50	50
		Spruce	90	85	50	0	..	..	..	..	100	90	65	15	15	0	..	..	100	95	70	60	40	30	15	0
		Yellow Pine	100	95	75	0	..	..	..	..	100	90	55	0	..	..	..	..	100	95	65	40	40	30	25	0
Raw Linseed Oil and Iron Oxide Pigment	National Lead Co., Prince's,	Oak	100	85	70	50	40	15	15	0	100	80	50	30	30	0	..	..	100	85	70	60	40	30	25	0
		Cypress	100	100	90	50	30	0	..	..	100	100	85	50	30	30	15	0	100	100	85	80	70	60	50	50
		Spruce	100	90	65	0	..	..	..	..	100	100	75	30	15	0	..	..	100	100	80	80	60	40	30	0
		Yellow Pine	100	100	70	40	30	0	..	..	100	95	70	0	..	..	..	..	100	100	75	50	40	30	25	0
Light Pressed Fish Oil	N. B. Cook Oil Co.	Oak	100	85	50	30	30	15	0	..	100	85	60	40	40	0	..	..	100	80	80	50	40	30	20	0
		Cypress	100	100	95	70	30	0	..	..	100	95	90	70	50	50	40	40	100	100	95	90	70	70	70	60
		Spruce	100	90	75	0	..	..	..	..	100	95	75	0	..	..	..	..	100	100	85	40	30	0	..	..
		Yellow Pine	100	80	40	15	0	..	..	..	100	100	55	0	..	..	..	..	100	100	80	40	30	30	15	15
INORGANIC SALTS IN SOLUTION																										
2-per-cent Zinc Chloride	Made up in W. C. K. Laboratory	Oak	95	80	65	15	15	0	..	..	90	70	40	0	..	..	..	..	95	80	45	0	..	..	..	..
		Cypress	100	100	95	90	80	60	45	0	100	100	95	40	15	0	..	..	100	95	90	0	..	..	..	..
		Spruce	100	95	65	50	30	0	..	..	100	100	80	0	..	..	..	..	100	95	55	0	..	..	..	..
		Yellow Pine	100	100	75	40	40	0	..	..	100	95	65	0	..	..	..	..	100	100	75	50	40	15	15	0
5-per-cent Zinc Chloride	" " "	Oak	95	80	40	15	15	0	..	..	95	65	30	0	..	..	..	..	95	80	50	30	30	15	15	0
		Cypress	100	100	85	70	60	0	..	..	100	100	90	70	60	50	50	50	100	100	95	80	80	60	50	50
		Spruce	100	90	65	40	30	0	..	..	100	90	65	30	30	0	..	..	100	95	60	30	15	0	..	..
		Yellow Pine	100	100	75	40	30	0	..	..	100	100	70	50	40	0	..	..	100	100	65	15	0	..	..	..
Sodium Zincate	Bruno Groesche & Co.	Oak	90	70	40	30	15	0	..	..	80	75	55	40	30	0	..	..	75	50	10	0	..	..	..	..
		Cypress	100	100	90	90	70	50	40	40	95	95	70	70	40	30	15	0	95	95	60	40	30	0	..	..
		Spruce	95	80	60	30	15	0	..	..	90	80	60	30	30	00	..	..	95	85	30	0	..	..	..	..
		Yellow Pine	95	80	65	50	30	0	..	..	90	85	50	15	0	..	..	..	95	90	60	50	30	15	15	0
"Jolanin" Fluoric Acid	" " " "	Oak	95	75	55	30	30	0	..	..	90	70	30	0	..	..	..	..	95	85	50	30	30	0	..	..
		Cypress	100	100	95	90	50	50	40	40	100	100	90	70	40	30	30	30	100	100	90	90	40	15	15	15
		Spruce	95	90	60	40	15	0	..	..	95	90	65	50	30	0	..	..	100	95	70	60	30	0	..	..
		Yellow Pine	100	95	65	40	0	..	..	..	95	95	80	50	30	15	15	0	100	95	70	40	0	..	..	..

INSPECTION AND RATING

The results of the annual inspections of the exposed specimens are shown in Tables III and IV. Table III gives the rating of treated specimens, while Table IV gives, for comparison, ratings of untreated specimens examined at the same periods as the treated specimens. The method of rating the specimens was a purely arbitrary one, in which the condition of the specimen as regards structural strength is expressed by the rating. While this method of rating specimens under long time tests leaves much to be desired from a scientific point of view, yet there is no other method which offers equal advantages. Therefore, tests to determine ratings are limited to visual inspection with perhaps a little surface testing with the fingernail or a sharp instrument. In spite of its lack of scientific basis this method has commonly been adopted for exposure tests of many materials, including paints, metal sheets and others. One of the drawbacks of the method is the possible inaccuracy of individual readings and the consequent inability of one studying the results of comparing individual ratings with any great degree of confidence in their fairness. One reasonably certain and definite point in the rating of specimens by this method, however, is the point of total failure. The complete rotting through of the specimen to the point of losing its structural strength is rather definite and by comparing the length of time required to reach this condition we have a fairly accurate method of comparison. Intermediate readings, however, are of value as indicating whether a preservative is of a type which gradually fails over a long period or one which gives protection for a period and then rapidly fails. An inspection of Table III will show both types of preservatives.

TABLE IV.—RATINGS OF UNTREATED SPECIMENS AT ANNUAL EXAMINATIONS.

Kind of Wood	Average Ratings							
	1914	1915	1916	1917	1918	1919	1921	1923
Oak	90	75	40	20	15	0	..	..
Cypress	95	90	65	55	35	20	0	..
Spruce	90	75	45	15	0	..	..	..
Yellow Pine	90	70	30	0	..	..	..	..

The results indicate rather decidedly that coal-tar and water-gas-tar derivatives are superior to the other classes of materials used. These creosotes show a higher average degree of protection than any material tested. Among the creosotes themselves there are some

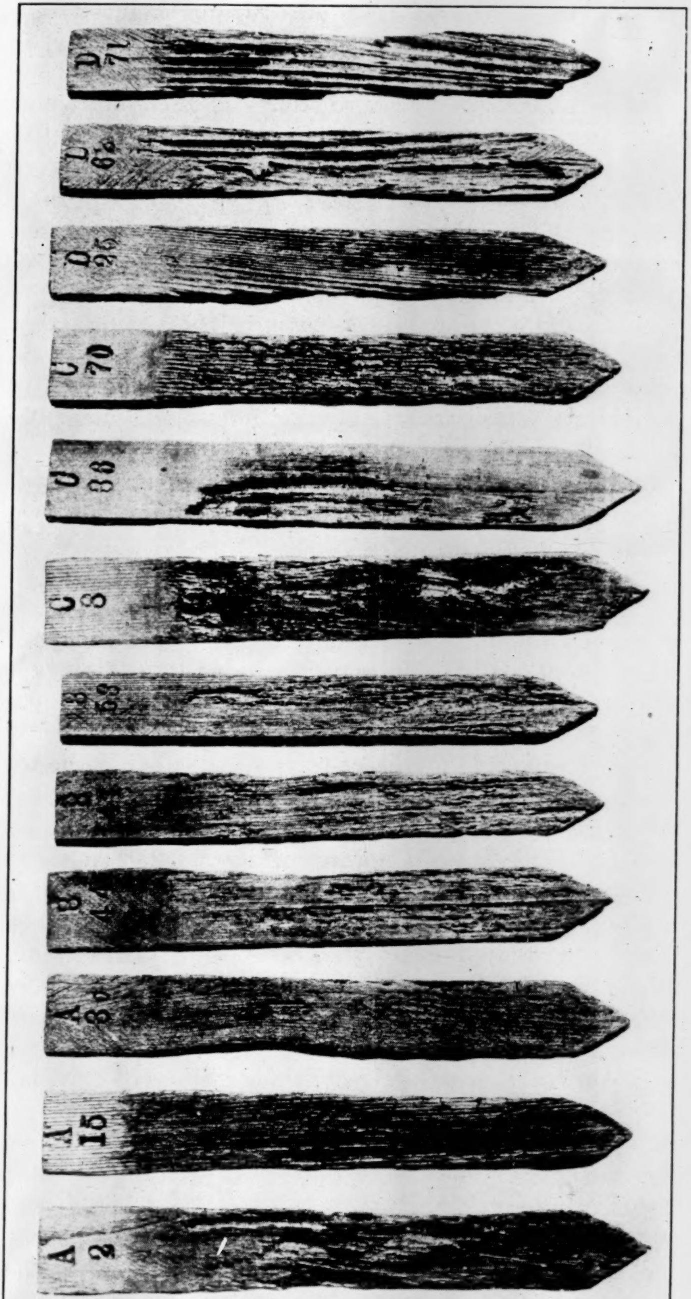
A
2
A
15
A
30
B
44
C
3
C
38
C
70
D
25
D
62
D
71

FIG. 1.—Three Typical Specimens of Each Kind of Wood

A....................Oak C..................Spruce
B....................Cypress D..................Yellow Pine

considerable variations in results. In general, the higher boiling fractions seem to be the more effective. The test is probably not sufficiently accurate in its nature to permit of close comparison between materials of nearly equal value. Fine distinctions therefore cannot be made, but the results indicate that while none of the preservatives is capable of giving permanent protection against decay under the conditions of exposure, the coal and water-gas-tar creosotes offer a better protection than any of the other treatments included in the test.

As to the relative value of the three methods of applying the preservatives, there seems to be but little difference between the cold dip and the hot dip treatments. As between these two methods and the boiling of the specimen in the preservative there is a wide difference in favor of the boiling. But even the approximate saturation which may at times be attained by the boiling treatment does not render the wood immune from decay in the course of time.

Another matter of interest in connection with the decay of wood is the variation in the relative resistance of the spring wood and summer wood in the different species.

To illustrate this there are reproduced photographs (Fig. 1) of three typical specimens of each kind of wood. These represent the characteristic appearance of these woods after decay has progressed to a considerable extent. In fact in many cases it is possible to pick out the different woods by their characteristic appearance after decay has set in. Specimens marked *A*, of oak, show checking across the grain. This is not common in the other three woods used in the test. It may be common in other woods beside oak, however. Specimens marked *B* are of cypress. This wood decays rather uniformly and without noticeable checking. In spruce, which is marked *C*, the decay progresses more rapidly in the more porous spring tissue, leaving ridges of the denser summer growth less affected. This wood shows a marked tendency to decay much more in some spots than in others. In the case of yellow pine, marked *D*, there is generally a most pronounced difference between the porous spring growth and the denser summer growth and prominent ridges result as is plainly shown in the photograph.

DISCUSSION

Mr. Blakeley

MR. A. G. BLAKELEY[1] (*presented in written form*).—I have read Mr. Chapman's paper with considerable interest. There are possibly a few wood preservatives of commercial importance not covered in Mr. Chapman's tests. The use of dilute solutions of sodium dinitrophenolate or mixtures containing this substance has been brought to our attention, but we can offer no information on this material. However, we have for some years been using sodium fluoride, and we are sorry that Mr. Chapman's tests did not include this salt.

Our work has been concerned with the preservative treatment of mine timbers, these timbers generally being of Loblolly Pine from Virginia and North Carolina. The treatment at present used consists in air drying the timbers 60 to 90 days, placing them in a treating cylinder 32 ft. long by 6 ft. in diameter and applying a pressure of 90 lb. to force the solution into the wood. We use a solution of sodium fluoride of 3 to 4-per-cent strength.

It has been estimated that in our mines (anthracite coal mines) 40 per cent of all gangway timber sent inside is used to replace decayed sets. Timber treated with creosote, gas-tar oil, zinc chloride, or sodium fluoride shows a life five to seven times the life of untreated timber.

In mine work it is thought by some that creosote and gas-tar oil may add to the fire hazard. These materials are considered objectionable also due to their oily and odorous nature, making the timber objectionable to handle while being placed in the mine.

Inspection made this year of timber which had been in one of our mines since 1908 showed that timbers treated with gas-tar oil, creosote, and zinc chloride, respectively, were all sound, whereas untreated timber in the same gangway has a life of only two to three years. Brush-treated timber did not have the life of tank-treated timber. Green-treated timber did not have the life of seasoned treated timber. This is due to a deeper penetration of the preservative in the case of the tank-treated timber and the seasoned treated timber, respectively. At this same mine, timber treated with sodium fluoride which had been in service for eight years showed no signs of deterioration, whereas untreated timber when bored showed evidence of decay.

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[1] Chemist, Philadelphia and Reading Coal and Iron Co., Pottsville, Pa.

Mr. Blakeley

At another colliery some creosoted timber which had been in service in a slope from 1909 to 1923 showed excellent condition. A fire in 1910 had charred the timber in this slope to a depth of ½ in. The life of untreated timber in this same slope is about two years.

In general, our actual service tests have shown that seasoned timber treated with any of the usual commercial wood preservatives has given quite satisfactory results. Mr. Chapman's tests show what is to be expected when wood is subjected to extremely adverse conditions, and his results are therefore of considerable value.

Mr. Church

MR. S. R. CHURCH.[1]—It is to be regretted that we have not had more accelerated service tests of the sort that Mr. Chapman has just reported. Some of us who are interested in the subject have attempted unsuccessfully to produce still more accelerated conditions of greater refinement in the so-called rotting pit or fungus pit in which maximum humidity and a temperature favorable to decay have been maintained. In some cases pure cultures have been used in an attempt to destroy the timber and know exactly what was the destructive organism. These attempts have not been successful, due to various causes. The principal cause of failure is the difficulty of keeping pure cultures alive in artificial or forced conditions.

Mr. Chapman's results emphasized the fallacy of using the terms "pounds per cubic foot" as an exclusive measure of absorption of the preservative. Some years ago Mr. Lowry Smith, of the Northern Pacific Railroad, published a series of tables in which he compared the surface area of timbers of different dimensions with the cubic content. If we take Mr. Chapman's stakes, which are 1½ by ½ by 12 in., we find that the ratio of surface area to cubical content is about 5½ to 1; whereas, if we take a railroad tie, which is the ordinary basis of integral measurement of preservatives, we have a ratio of surface area to cubical content of about ¾ to 1. In other words, there is relatively about 7½ times as much surface area on Mr. Chapman's stake as there is on the railroad tie, so that although some of his stakes by the boiling treatment showed a very high absorption expressed in pounds per cubic foot, that is not really comparable to the same absorption in ordinary dimension timber. The ratio of surface area to cubic contents probably has some bearing on the relative value of the high-boiling oils as compared with the lower boiling oils as found in these experiments. It is quite possible that a creosote of comparatively low-boiling character which failed rather rapidly on this test due to the large surface exposure might have given much better service in a timber of large dimensions.

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[1] Technical Adviser, The Barrett Co., New York City.

Mr. von Schrenk

MR. HERMANN VON SCHRENK.[1]—I am very much interested in this test of Mr. Chapman's, and particularly in Table II showing the absorption of preservative by each specimen. I believe we must be cautious in interpreting the data in this table. Mr. Chapman calls attention very strikingly to the fact of the different absorptions of different sticks for different preservatives. If I understand his tests correctly, only one stick of a given kind of wood was impregnated with each preservative by each of the three methods. Now, the absorption that takes place in the same kind of wood for the same kind of treatment is always startling to the person who sees it for the first time. We have on various railroads been treating ties the same as Mr. Chapman has treated his sticks, and in the case of the oaks, we get a variation in absorption running from 2 to 50 lb. for the same size tie, and with highly absorptive wood like pine, the variation would be even greater. Therefore, in studying a table of results in which only a small number of pieces are taken, one must always bear that factor in mind.

There is another point which is rather surprising. Mr. Chapman says the wood which absorbed the highest amount of preservative was cypress. In our experience in cylinder treatment, cypress is the most difficult wood we have to handle. We have just gone through a long experience on one of our western railroads, in which we attempted to treat cypress piling under 180-lb. pressure for 24 hours and got a penetration of only $\frac{1}{4}$ in. The cypress Mr. Chapman used must have been very porous sap wood to account for the high penetration he obtained.

I should like to add to what Mr. Church has said. It is rather fortunate that someone has finally tackled this method of experimentation. It is rather odd that for so many years there have been no more tests of this kind made.

Mr. Chapman

MR. CLOYD M. CHAPMAN.—It occurred to me, while Mr. von Schrenk was speaking about the difference between the results obtained here and with cypress in his experience, that it may be due to the relatively large surface area. Cypress is difficult to penetrate deeply, but easy to penetrate for a short distance. The specimens in my tests were only $\frac{1}{2}$ in. thick and it required only a penetration of $\frac{1}{4}$ in. on each side to give full penetration. Mr. von Schrenk mentioned also the fact that there had not been more of these tests, and that he was glad to see one of them tried. It may serve to show that this character of test is not very much more reliable than the pure culture test Mr. Church referred to. This paper is not presented with the idea that

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[1] Consulting Timber Engineer, St. Louis, Mo.

Mr. Chapman it gives a lot of valuable information of a positive nature, but it shows the results on our work. Many results are valuable because of their negative indications. This investigation, I think, shows that many factors enter into such a test which are hard to foresee. Whether a test of this kind is of real value or not, each of you can judge. My general conclusion from the whole work is that it leaves much to be desired.

AN APPARATUS AND METHOD FOR DETERMINATION OF RESISTANCE TO ABRASION OF RUBBER PRODUCTS

By W. W. Evans[1]

Rubber, in service, is nearly always subjected to some sort of abrasion—tires, belts, boots and shoes, hose, etc., all must resist abrasion—and in many cases resistance to abrasion determines the life of the article. It is important, therefore, that the manufacturer should know accurately the relative abrasion resisting properties of different compositions, and it is very desirable that these properties should be measured in the laboratory. It is not surprising then that for years persistent attempts have been made to develop a machine for this purpose.

Abrasion testing machines have been built in a variety of forms, but, in general, the sample is subjected to the action of a revolving abrasion wheel or disk. One of the objections to this type of machine is the difficulty of maintaining the surface of the abrasion wheel. The surface of the wheel or disk quickly becomes clogged with the compound under test, which changes the rate of abrasion. This difficulty has been overcome by two machines using this system, namely, that developed by the U. S. Bureau of Standards and the one used by the New Jersey Zinc Co. Comparative tests have been made between the latter machine and the one described in this paper, and it has been found that similar results can be obtained.

The machine described in this paper, developed by Mr. J. C. Sproull and co-workers in the Testing Laboratories of the B. F. Goodrich Co., is of a different type. The machine (Fig. 1) consists essentially of a train of ten identical spur gears all in mesh and arranged in circular form in a horizontal plane. The shafts of alternate gears, five in all, extend vertically downward, so that the machine somewhat resembles a multiple spindle drill-press. Fixtures carrying the test samples are attached to the bottom of the spindles, and are "buried" in loose abrasive material contained in cans as shown.

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[1] Development Manager, The B. F. Goodrich Co.

DETAILED DESCRIPTION OF APPARATUS

The body of the mandred (Fig. 2) is 1 in. in diameter and $2\frac{7}{8}$ in. long over all. This will take five samples of rubber when assembled for test. The mandrel is designed to be inverted, thus affording an equalized static pressure on all samples for the total test period. A small propeller arrangement has been made which is attached to the bottom of the mandrel, keeping the abrasive stirred up from the bottom of the can pending any tendency towards congestion of abrasive.

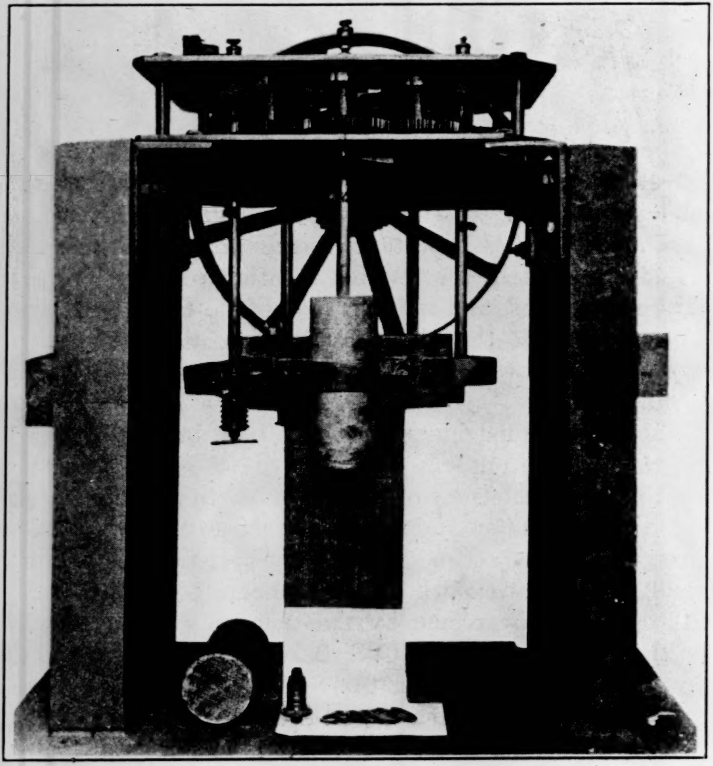

FIG. 1.—Abrasion Apparatus for Rubber Products

The spacing rings used between the samples are $\frac{3}{16}$ in. wide, $1\frac{1}{4}$ in. in outside diameter and 1 in. in inside diameter. The guard rings placed at either end of the mandrel are $1\frac{11}{16}$ in. in outside diameter, 1 in. in inside diameter and $\frac{1}{8}$ in. wide. The abrasive container is a can 3 in. in inside diameter and 9 in. long. This shape has been chosen so as to reduce the amount of abrasive required to a minimum and yet afford a static head of about 5 in. above the top of the mandrel. A 20-mesh screen is soldered to the bottom of the container, which permits the abrasive dust to separate out.

As an abrasive we have found that a carborundum grain between No. 6 and No. 8 mesh is most desirable. Crushed quartz of several varieties, steel grit, sand and alundum have been tried, but none are as satisfactory as the carborundum grain. In fact, we have found that steel grit and sand have very little abrasive actions on rubber in such a test. The carborundum grain may be used a number of times by re-screening.

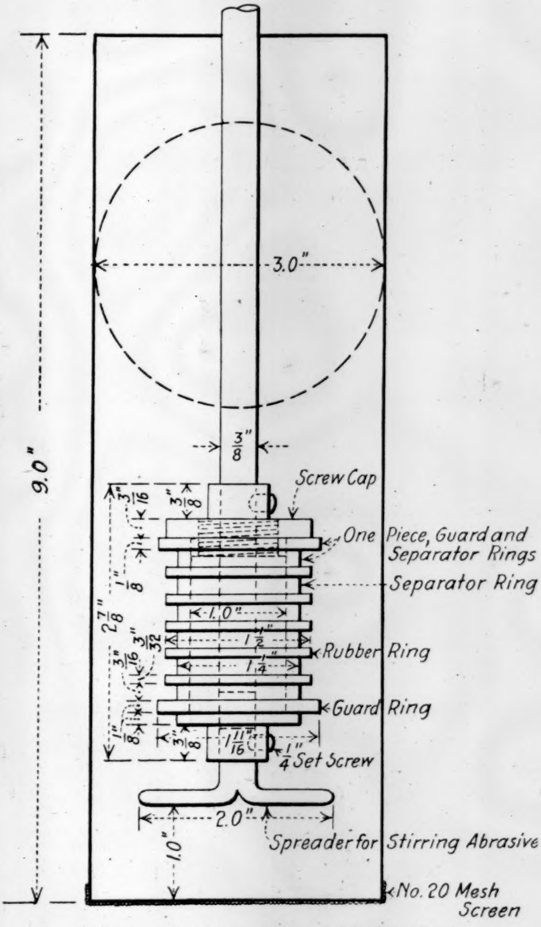
9.0"
3.0"
3/8"
Screw Cap
3/16"
3/8"
One Piece, Guard and Separator Rings
1/8"
Separator Ring
2 7/8"
1.0"
3/32"
1 1/2"
Rubber Ring
3/16"
1 1/4"
Guard Ring
1/8"
3/8"
1 11/16"
1/4" Set Screw
2.0"
1.0"
Spreader for Stirring Abrasive
No. 20 Mesh Screen

FIG. 2.—Specimens Assembled in Can

Test samples are cut from sheets $\frac{3}{32}$ in. thick with a die 1 in. in inside diameter and $1\frac{1}{2}$ in. in outside diameter. Samples fit on a mandrel, the body of which is 1 in. in diameter and the rubber is not under tension when tested. We have followed the procedure of placing the standard compound in the center and two experimental samples on either side.

METHOD OF TEST

For one mandrel, five test rings are prepared and weighed, and placed on the mandrel. As indicated above, four of these are of the unknown compositions and one standard. The mandrel is fastened to the end of the rotating shaft, the abrasive can put in place and filled to the proper level, and the machine started. This operates at the approximate speed of 130 r. p. m. The test requires about eight hours. At the end of four hours, the machine is stopped and the mandrel reversed in its position, so that the two bottom rings are now the top rings. This gives all samples equal static head for the same period of time during the test. It is also desirable to replace the abrasive with fresh material at the end of this four-hour period. A typical series of tests is recorded in Table I.

TABLE I.—SAMPLE TEST CALCULATION OF RESISTANCE TO ABRASION DETERMINATION

Length of Run, 8 hours
Room Temperature, 86° F.
Average Temperature in Cans, 150° F.
Speed of Mandrels, 130 r.p.m.

Compound	Can	Initial Weight, mg.	Loss in Weight, mg.	Loss in Weight, per cent	Index of Abrasive Resistance
A	No. 1	2481.0	531.0	21.37	759
B	No. 1	2714.0	519.0	19.12	848
C	No. 1	2707.0	509.0	18.81	862
D	No. 1	2380.0	470.0	19.79	819
Standard	No. 1	2780.0	450.0	16.22	1000
E	No. 1	2634.0	364.0	13.81	337
Standard	No. 1	1676.0	78.0	4.66	1000
E	No. 3	2680.0	385.0	14.37	349
Standard	No. 3	1637.0	82.0	5.01	1000
F	No. 2	2335.0	748.0	32.19	163
Standard	No. 2	1676.0	88.0	5.25	1000
F	No. 4	2440.0	810.0	33.21	166
Standard	No. 4	1630.0	90.0	5.52	1000

The samples are again weighed after the test, the loss determined and converted to an index number. The percentage loss of the standard compound is taken as 1000. The percentage loss of each experimental compound on the mandrel is divided into the percentage loss of the standard compound and multiplied by 1000. A compound with an index number of 1100 is, accordingly, better than the standard compound in resisting abrasion and one with an index number of

900 is not as good as the standard. It is unnecessary to obtain the specific gravity or the volume loss of each compound, since the percentage loss in weight is the same as the percentage loss in volume. Experience indicates that it is necessary to wear away 15 to 20 per cent of the original volume of the sample, in order to obtain consistent results.

One source of error is the difference in thickness of the test samples. Molded sheets used for this purpose are found to vary from 0.080 to 0.120 in. in thickness. The total area of rubber exposed to the abrasive on a sample 0.080 in. thick is 1.114 sq. in., and that for a sample 0.120 in. thick is 1.135 sq. in. The difference is 0.021 sq. in. or 1.88 per cent (based on thickness of 0.080 in.).

A test begun in one container should be completed in that container. Eccentricity of the shaft on which the mandrel is fastened will cause different degrees of wear in each container, assuming shafts and mandrels do not run perfectly true.

Some advantages claimed for this abrasion testing machine are as follows:

1. It is reasonably consistent. When a spool is loaded with samples all cut from the same sheet of rubber, the results check very closely.

2. The test samples are small enough to be cut from many finished articles.

3. The volume worn away from any given sample has little, if any, influence upon an adjacent sample.

4. Stiffness is not a factor.

5. Tests may be run in a reasonable time without excessive heating.

6. The abrasive can be screened and used repeatedly.

7. Local variations of the abrasive material, such as occur with abrasive wheels and disks, do not occur with the loose abrasive.

DISCUSSION

Mr. McBurney MR. J. W. MCBURNEY.[1]—I should like to ask the speaker if any attempt has been made to correlate this test with results in actual use. In other words, would you expect the mileage from given tire treads to be proportional to the results of this test?

Mr. Evans MR. W. W. EVANS.—I think that would be the result, in a general way. I would not say that the test results would be in exact proportion to the service results, but we would expect the rubber which had an index of 1000 in the wear test to show up better in service than one having an index of 800.

Mr. Breyer MR. F. G. BREYER.[2]—The question whether the abrasion test checks with the road test, is the most common question asked about abrasion machines. Now the necessity of an abrasion machine, the strongest argument for it, is the fact that the road tests do not check. Anybody who drives a car knows very well that he cuts his tires up on a rough country road twice as fast as he does on smooth city streets. So long as there is no method of laboratory testing designed to show the difference in abrasive resistance under constant conditions, road tests are not going to mean very much absolutely. I would say the abrasion testing machine can be taken as a standard and the road test should be compared with it, rather than the other way. Road conditions vary. If you get a standard abrasion test, the purpose is not to duplicate what can be gotten on all roads, but it is to make a test which will tell what different samples of rubber will do when a particular abrasive corresponding to a particular road is applied. You can vary the type of abrasive; you can put coral in or you can put a very much smoother, finer-grained material in corresponding to the effect of city streets. I think the abrasion testing machine is a coming machine, and whether or not it checks up with the average of all road conditions is really beside the question.

Mr. Curtis MR. E. B. CURTIS.[3]—I should like to ask Mr. Evans if one should test five samples of rubber on this machine, all cut from the same piece of stock, what variation between maximum and minimum results should be expected, on the average?

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[1] Director of Testing Laboratory, Cleveland Board of Education.

[2] Chief, Research Division, New Jersey Zinc Co., Palmerton, Pa.

[3] United States Rubber Co., General Laboratories, New York City.

Mr. Evans MR. EVANS.—In Table I of the paper, there are reported two identical samples marked E. They were run in separate cans; in one case the abrasive resistance, reported as the average of these tests, is 337; the average of three on the same material in another can is 349. In other words, you will get reasonably close results.

Mr. Breyer MR. BREYER.—We may be over enthusiastic about abrasion testing, but the result of our experience is such that we put more confidence in the amount of rubber abraded away after a standard abrading test for a certain cured stock than we do in the tensile strength that that material shows. We believe an abrasion test averages thousands of small tears, whereas the tensile test gives nothing more than the resistance to one tear. I want to emphasize the fact that no amount of work that we can do will ever do away with skilful manipulators of all the machines we devise; with a skilful and carefully trained operator, we can check our abrasion figures closer than our tensile strengths.

Mr. Jury MR. A. E. JURY.[1]—I understand that Mr. Breyer feels that the importance of abrasion testing will be in the comparison of one compound or ingredient with similar compounds or ingredients. I do not understand that the abrasion machine is being presented as a machine which will predict a service that one stock will give on the road as a tire tread or another as a friction compound or another as a sole compound, but rather as a means of checking up compound ingredients to determine whether one is as good or better than some other.

Mr. Breyer MR. BREYER.—Our experience is that wherever we have gone into other fields than tire treads we have found that we were helped materially in duplicating. Take a stock in which abrasion does not enter as a life factor at all, say we want to duplicate a certain inner tube. We can analyze it, chemically, then take that inner tube and put it through the standard tension and elongation tests, then we run an abrasion test on it, and we make another sample, curing at various stages, and if we take that cure which gives the same abrasion resistance, we get closer to duplication than through the tensile properties. From the results so far, the abrasion checks closer than the tensile.

Mr. Evans MR. EVANS.—We have checked with Mr. Breyer on a considerable number of samples, he on his type of apparatus and we on ours, and I believe that the results obtained are generally of the same order of magnitude and run in the same general direction.

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[1] Manager, Textile Section, and Director, General Laboratories, United States Rubber Co., New York City.

TOPICAL DISCUSSION ON SLATE

THE CHARACTERISTICS OF SLATE[1]

By Oliver Bowles[2]

Introduction

Before one can properly establish a system of standards and tests for any mineral commodity, he must define its uses and clearly understand the properties which fit it peculiarly for each use. It is essential also that the problems of the producer as well as those of the consumer be taken into account, for thus wise utilization is attained and economical production is safeguarded. The purpose of this paper is not to outline definitely any single specification, but rather to cover briefly the principal features of slate, and the present status of our knowledge of the subject in order to provide a background for the study of specific requirements.

Origin and History of Slate

All rocks disintegrate slowly, chiefly through the action of frost and water. Several minerals, notably the feldspars, change into clay and this clay is carried by rivers to the ocean and deposited in deep water. Such clay beds when consolidated form shales and the shales by intense regional metamorphism are converted into slates. Thus, mountain building forces have in past ages folded the shales and compacted them with terrific pressure into the form in which slate now appears.

Composition and Properties.—High temperature and intense pressure involved in the process of slate formation converted the original clay into other minerals, such as quartz, mica and chlorite. It is important to note that while slates are formed from clay rocks, the best commercial slates contain very little clay. Dale[3] gives the following approximate composition of average quality slate:

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[1] Published by permission of the Director of the U. S. Bureau of Mines.

[2] Mineral Technologist, U. S. Bureau of Mines, Washington, D. C.

[3] Dale, T. Nelson, and others, "Slate in the United States," U. S. Geological Survey *Bulletin No. 586*, p. 19 (1914).

Mineralogical Composition of Slate

Mineral	
Mica (sericite)	38 to 40 per cent
Chlorite	6 to 18 "
Quartz	31 to 45 "
Hematite	3 to 6 "
Rutile	1 to 1½ "

It may be observed that no mention is made of a clay content.

In the process of development of the flaky minerals such as mica and chlorite, pressure causes the little tabular plates to assume a parallel position and this gives to slate its most characteristic property, a remarkable fissility which permits it to be split with great ease into thin uniform sheets. This property is known as slaty cleavage.

Slate is of medium hardness, very fine grained, of low porosity, great strength and consists essentially of minerals that are very stable and resistant to weathering. The chief colors are gray, black, green, red, purple and mottled.

Uses.—A folder recently issued by the National Slate Association lists about sixty uses for slate, the chief of which are roofing, electrical switchboards, blackboards, and structural materials such as steps, toilet enclosures, shower stalls and baseboards. As the properties and adaptability of slate are better known, new uses are discovered.

Properties on Which Use Depends

ROOFING SLATE

Splitting Properties.—True and easy splitting is one of the important properties of a roofing slate. It is generally recognized that slate splits more freely when it is first quarried before the "quarry water" or "sap" has evaporated. For this reason, water is thrown over a block, or it is protected from evaporation by some other means, until splitting is completed. It is claimed, and has been successfully demonstrated, that seasoned slate may be readily split with a slate splitting machine invented by Vincent F. Lake. It would appear, therefore, that when mechanical splitters are employed, the cleavage of slate may be regarded as a more permanent and more stable property than it is now generally considered. The actual physical effect of seasoning on the cleavability is a field of study that has not yet been adequately covered.

Curvature of cleavage planes is not uncommon in slate deposits and is an undesirable feature. Slight curvature may not condemn slate for manufacture into small squares, but prevents its utilization for blackboards and similar products of large surface area. Advantage has been taken of curved cleavage for roofing round towers, but this use is exceptional.

Resistance to Weathering.—Slate consists essentially of insoluble and stable minerals that will withstand weathering for hundreds of years. However, small percentages of less resistant minerals may be present. Calcium, magnesium or iron carbonates are slowly soluble in water containing carbon dioxide, and their presence may hasten deterioration. As a rule, a determination of the carbonate present would indicate fairly well the probable resistance of a slate to weathering when exposed on a roof. Some slate in Pennsylvania contains ribbons which consist of narrow original beds usually containing carbon, and darker in color than the main body. There is a tendency for some ribbons to contain an excessive amount of the less resistant minerals and they should not appear on exposed surfaces.

Color Stability.—Slate colors may be fast or changing. The fast colors are due in the main to the presence of iron oxide and other stable oxides. As indicated in the preceding paragraph a change in the color may be due to the oxidation of disseminated carbonates. The terms "fading" and "unfading" are unfortunate, as fading always conveys the impression of inferiority. In some instances the change in color is quite undesirable, in others very pleasing tones of color result. The terms "fast" and "changing" are more appropriate.

Spots and blotches are of common occurrence in the slates of Vermont and New York. Careful chemical and microscopic studies as recorded by Dale[1] indicate that variations in color are due to differences in the proportion of carbonates, pyrite and hematite present. It is probable that decaying organisms embedded in the original shale have reduced the Fe_2O_3 to FeO, and the latter has been removed in solution. Usually in the centers of the spots there is an excess of carbonate which may have originated from the shells of the organisms. In general the spots are permanent features that are practically as stable and unchanging as the colors in the main body of the slate.

The oxidation of iron carbonate or of the mixed carbonates of iron, lime or magnesia may cause rusty stains. If the carbonates are evenly distributed they may on weathering change the general color of the entire surface. Stains are also caused by the oxidation of iron pyrite. However, the presence of pyrite is not a sure indication that the slate will stain upon exposure, for some forms of pyrite will remain unaltered for many years. The whole question of the oxidation of pyrite in building stones has been fully discussed by the author in another publication.[2]

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[1] *Loc. cit.*, pp. 23 to 27.

[2] Oliver Bowles, "The Technology of Marble Quarrying," U. S. Bureau of Mines ***Bulletin No. 106***, pp. 29 to 33.

Strength.—As slate consists largely of over-lapping scales firmly cemented under intense pressure, it is remarkably strong. If slates are not split excessively thin they possess sufficient strength for any roofing demands.

STRUCTURAL SLATE

Porosity.—Slate is widely used for sanitary purposes, and for such uses a low porosity is requisite. An average of results obtained by the U. S. Bureau of Standards from many slates indicates a porosity as low as 0.38 per cent. Thus, average slate is well adapted in this respect for sanitary uses, and rejection of slate on the basis of high porosity would imply the attempted use of a very inferior grade, such as slate from a weathered ledge.

Strength.—For certain uses such as steps, lintels, window sills or window and doorcaps, a high transverse strength is required. The peculiar sheet-like structure of slate gives it remarkable strength, a strength far in excess of that possessed by nearly all other building stones. Sandstones are very variable because some are firmly cemented while others tend to be loose and friable. Expressed as the modulus of rupture, the transverse strength of sandstones varies from 350 lb. per sq. in. for the friable types to 6500 lb. per sq. in. for dense quartzites, 1000 to 1500 being a fair average. A series of marbles shows an average of 2410 lb. per sq. in. across the bed. An average of 72 limestones recently tested by the U. S. Bureau of Standards indicates a modulus of rupture perpendicular to the bed of 1253 lb. per sq. in. The modulus of rupture of a series of roofing slates recently tested is 8216 lb. per sq. in., about five or six times the strength of average sandstone or limestone and three or four times that of average marble. Thus, slate more than satisfies all reasonable strength requirements for building purposes.

There has been some hesitancy on the part of architects to use ribboned slate for structural purposes, because the clearly defined borders of the ribbon suggest planes of weakness. Theoretically, there should be no weakness along the line of the ribbon, for it represents an original bed, and the complete recrystallization of the mass has developed structures that are entirely independent of the bedding. To obtain definite information on the question, slabs of ribboned slate were recently tested for transverse strength at the Bureau of Standards, and it was found that when breakage occurred the fractures were never coincident with the ribbons but crossed them at various angles. This proves conclusively that the ribbons do not constitute planes of weakness.

Abrasion.—For floor tile or steps, resistance to abrasion is an important requisite. In so far as this Bureau is informed, a limited amount of work has been done in determining the actual resistance of slate to abrasion. Where ribboned slate is used in sidewalks, some of the ribbons through solubility are worn away as grooves, while others containing an excessive amount of silica stand up as ridges. Such non-uniformity in ribboned slate is an objection to its use, where subjected either to weather conditions or to excessive wear.

BLACKBOARD SLATE

Texture.—A good blackboard slate must be fine-grained, uniform, and somewhat softer than much of the slate used for other purposes. The soft vein slate belt of Pennsylvania, 26 miles long and 2 or 3 miles wide, provides practically all the good blackboard slate in the world. It is selected from the widest bands between ribbons, and it is important that it be free from siliceous knots or veins that impair the writing quality.

Cleavage.—Only those slates that are true and free splitting may be used successfully for blackboards. The slabs are split to a thickness of $\frac{3}{8}$ to $\frac{1}{2}$ in., and when made from smooth splitting slate, except for double-faced blackboards, the back of the board needs no surface finish, though the face must of course be sand-rubbed and honed or polished.

ELECTRICAL SLATE

Insulating Value.—The first requisite of a slate for switchboard use is high insulating value. Slates vary considerably in insulating qualities, and therefore the best grades only are used for electrical purposes. Some slates are applied almost exclusively to electrical uses while slates from some other regions are better fitted for other adaptations. When slate fails under dielectric test the failure may be due to the presence of conducting minerals such as the iron oxides or iron sulfides, especially if they are present as stringers or veins that form a continuous path for the electricity. Some investigators have recently reached the conclusion that the presence of carbon is the chief cause of failure under dielectric test. It has recently been found that moisture in the form of quarry water will lower the electrical resistance, a higher test being obtained for the same material after it is seasoned.

Heat Resistance.—All slate is sufficiently resistant to heat to comply with any switchboard requirements.

Color and Texture.—Uniformity in color and texture are highly desirable. Enlargement of switchboard installations demands an accurate matching of the new panels with the old, a matter that is usually easy of accomplishment where slate is used.

Coefficient of Expansion.—Expansion and contraction of slate due to changes in temperature are practically negligible. Tests at the Watertown Arsenal have shown the coefficient of expansion to be 0.000005. Thus in mounting switchboard or in enlarging panels, no allowance need be made for expansion or contraction.

Workability.—Slate is easily shaped into panels and takes a good polish. It may be readily drilled with little danger of breakage.

Strength.—Transverse strength is not an important requirement, as switchboards are not usually subjected to severe strains. The strength of slate is far in excess of all reasonable needs.

Resistance to Shock.—For certain uses, it is important that switchboards should be capable of withstanding shock; for example, those on warships should withstand the shock of gunfire. No information is available as to the resistance of slate to sudden shock.

SUGGESTIONS ON STANDARDS AND TESTS

ROOFING SLATE STANDARDS

Standard Thickness.—In free-splitting slate there is a tendency to make slates thin in order to obtain a maximum production from each ton of rock. In consequence, the breakage loss in transit may be high and breakage on the roof may be frequent. This is an unsatisfactory condition for the user, as broken slates are not easily or satisfactorily replaced, and new slates may not match in color with the old roof. This reacts on the producer, for broken slates may prejudice the user against slate in general. It is generally regarded that $\frac{3}{16}$ in. should be the minimum thickness allowed, and if this were established as a standard it is highly probable that greater satisfaction would exist among consumers, and that the reputation of slate would thereby be enhanced. The thickness should, of course, be a factor of the transverse strength, but until full information is available on the average strength of slates in various regions it would seem advisable to establish a fixed minimum that would cover all slates of reasonable quality.

In fixing a minimum thickness, it is not presumed that every slate shall be exactly $\frac{3}{16}$ in. thick. Slates are split from natural blocks and are not milled to exact thickness. Slight irregularities may cause the split to run one way or another causing small variations which any specifications should permit. The splitter's chisel with which he measures and determines the thickness should, however, be standardized for $\frac{3}{16}$-in. slates.

Standards for Size.—Roofing slates are now made in about 29 standard sizes. It might appear at first sight that the number of sizes is too great and that a reduction in the number would be an advantage to both producer and consumer. There is a widespread movement to reduce production costs by simplifying manufacture, but in the slate industry conditions are entirely different from those that exist in industries where products are molded or built up into finished form. Roofing slates are trimmed to size, and the size is largely governed by the qualities or imperfections of the natural slabs. It may be found that a slate if perfect would trim to 10 by 20-in. size, but on account of some imperfection it may be necessary to trim it to a smaller size. It is noteworthy that if there is a small range of standard sizes, the gaps between them will be relatively large and the waste in trimming down from one size to another will be correspondingly great. A wide range of sizes is, therefore, of definite advantage to the slate manufacturer for it permits him to utilize his raw material to best advantage with a minimum of waste.

A variety of sizes is also an advantage to the roofer. Windy cities require small sizes, also large and massive structures demand larger sizes than are best adapted for small dwellings. Even for the same type of structure quite different sizes are demanded in different cities. Thus in Cincinnati, Ohio, and Galveston, Texas, the principal demand is for 10 by 12-in. slates; in Chicago, 10 by 16 in.; in Columbus, Ohio, 12 by 24 in. A large variety of sizes permits the manufacturer to cater to the demands of all localities.

It is evident, therefore, that nothing is to be gained by inaugurating any movement toward reducing the number of standard sizes of roofing slates.

Standards for Color Stability.—It is unfortunate that some of the trade names applied to slates mask the permanence of their colors. Slate dealers understand the peculiarities of each type, but slate users who are not so well informed are entitled to a set of terms that are descriptive in character and that represent actual conditions. Thus, terms should be employed that clearly distinguish between fast and changing colors.

Standards for Endurance.—The endurance of slate may be measured approximately by the percentage of carbonates present, but other factors such as porosity may influence it. Where the ribbon is less enduring than the clear slate, standards should be established on this basis. Slate from the zone of weathering near the surface of a deposit should not be used. While slate has been known to endure exposure on a roof for several hundred years, it is unreasonable to expect that material which has been more or less exposed to the ravages of weathering agencies in its natural bed for several thousand years, may give as satisfactory service on a roof as that obtained from the deep-seated deposits. Architects have lately developed great enthusiasm for variegated, highly colored, thick, heavy slates known as architectural grades. The author has observed such slates taken from the zone of weathering where the attractive red, yellow and orange colors were due to excessive oxidation of the iron bearing minerals. Thus endurance was sacrificed for an attractive color. No slate of this character would stand a chemical or microscopic test for enduring qualities, and thus the need is emphasized of establishing definite tests that will protect the user, and incidentally the producer.

Standards for Placing Slate on the Roof.—A product perfect in quality and faultless in manufacture may fail through improper application. Many instances have occurred where good slate has given unsatisfactory service through improper laying. A 3-in. head lap should be the universal standard. While this is generally recognized, some roofers cut down the lap to 2 in. in order to make a square of slate cover more than 100 sq. ft. of roof.

There is a wide difference of opinion as to the best type of nail to use with roofing slates. The nail should be as permanent as the slate, and standards should be fixed for length, diameter, and point, both for new roofs and for roofing over wooden shingles, composition or other materials.

The strength and spacing of supports, the nature of the sheathing, and the use of paper beneath the slates are all matters that have a definite bearing on the quality of the completed roof.

STRUCTURAL SLATE STANDARDS

Absorption.—Slate for sanitary uses should be graded according to its porosity, and satisfactory service would be promoted by fixing a maximum ratio of absorption.

Strength.—There is a dearth of information on the crushing strength of slate, and except for recent tests made at the Bureau of Standards there is a similar lack of data on transverse strength. As already stated the latter figure is exceptionally high. Wider publicity of this feature would benefit the slate producer, and at the same time encourage the builder to use slate with advantage as a substitute for weaker materials. It is not uncommon to see fractured stones in large structures. Where the strain is excessive it is desirable to establish a high standard which only the strongest natural products such as slate can successfully meet.

Abrasion.—More definite knowledge of the resistance of slate to abrasion is desirable. Some slate stairways long in use are greatly worn, others resist wear remarkably well. The first step in establishing abrasion standards would involve a long series of abrasion tests upon slate from various localities on the basis of the French coefficient of wear. The qualities should as far as possible be expressed in terms that represent actual wearing conditions in comparison with other standard materials used in steps and floors.

Structural Designing.—The best work that has yet been accomplished in slate standards has appeared in a series of bulletins by D. Knickerbacker Boyd of the Structural Service Bureau of Philadelphia, working in cooperation with the Structural Slate Co. of Pen Argyl, Pennsylvania. These pamphlets give detailed specifications with plans and elevations of wainscots, stairways, toilet enclosures, laundry tubs, sinks, etc. The object has been to so standardize sizes and designs that ordering may be facilitated, that manufacturing may be simplified, and that standard materials may be kept in stock. The latter feature is an advantage to both producer and consumer, for it saves time in filling orders, and enables the producer to operate and accumulate stock during slack periods. It has also the advantage of giving steady employment to mill and quarry workers.

BLACKBOARD SLATE STANDARDS

Standards of size and detail of installation of blackboards are given in a pamphlet of the Natural Slate Blackboard Co., and in Vol. 1 of the American Architect Specification Manual of 1919. The requirements of slate for blackboard use are more exacting than for most applications. The desire for perfect uniformity has prompted some architects, builders or school boards to specify that every board shall be absolutely free from blemish or imperfection. While this may seem desirable, any specifications as to quality should be formulated with due regard for the producers' problems. It is estimated that in many slate quarries only 10 per cent of gross production is commercial finished slate, and only 10 per cent of the latter is clear stock. Thus to fulfill a rigid demand for flawless slate the producer may use only 1 per cent of gross production. It is evident that such demands would work a hardship on the producer, and would inevitably result in limited production at an excessive price. In framing specifications for quality it is suggested, therefore, that provision be made for allowing inconspicuous spots, cloudiness or natural markings which do not detract from the writing quality or the readiness with which marks may be erased. It is generally admitted that even with minor variations, slate is superior to other materials for blackboard use, and reasonable flexibility in specifications would permit wide utilization at moderate cost. On the other hand, workmanship requirements should be rigid and methods of installation properly standardized.

ELECTRICAL SLATE STANDARDS

Insulating Value.—There is urgent need of the establishment of definite specifications and standard tests for electrical slate. Without such standards rejections occur, involving delay and inconvenience to the user, and possible loss to the producer. It is highly desirable that a system of tests be devised whereby a block of slate may be tested at the mill before any costly workmanship has been applied to it. I understand that apparatus for this purpose has lately been developed.

Before reliable, constructive work in standardization can be done much preliminary experimental work should be undertaken. The factors that govern insulating value of slate are not sufficiently understood. Some investigative work has been done by the larger electric companies, but the results are not, in so far as I know, in a form available to the public. Recently tests of Pennsylvania slate have been made in the Fritz Engineering Laboratory of Lehigh University, and the information thus obtained has served as a basis for tentative specifications. A great deal of experimental work should be done to determine the electrical resistance of slate containing iron oxides and sulfides disseminated regularly or in veinlets, and also the effect of carbonaceous matter or iron silicates. A series of tests should be made of freshly quarried slate and similar slate after it has been seasoned. Tests should also be made to determine the effect of subsequent resaturation.

In some slates, the ribbons are planes of low electrical resistance, and clear stock free from ribbons must under such circumstances be specified.

Physical Properties and Workmanship.—Limits should be established for tensile strength, porosity, and resistance to impact and heat. Reasonable accuracy should be required in workmanship to insure proper dimensions, bevels and angles.

Standards of Size.—Some effort should be made to simplify and standardize the various sizes of switchboards. It is customary when an order for switchboards is received to quarry the blocks, take them to the mill and cut them to the size specified, irrespective of the fact that the quarried blocks would often cut to better advantage to other sizes, possibly to sizes much more valuable than those ordered. Even under present standards much of the cutting to definite orders could be eliminated, for a sufficient number of sizes are now definitely specified to permit carrying a reasonable stock. A reduction in the number of sizes, and more uniform standards of thickness and bevel, would offer all the advantages already outlined for fixed standards of structural slate and in addition the important advantage of making it possible to order almost entirely from stock on hand, and thus deal only in seasoned slate.

SUMMARY

It is evident that the work of standardization should follow many lines, and also that a great deal of experimental work should be conducted as a basis for correct and logical specifications. The need of specific requirements is outstanding in the matter of thickness, size and color stability of roofing slates, quality of blackboard slate, strength and abrasion of structural slate, and insulating value of electrical slate.

ELECTRICAL SLATE

By Dean Harvey[1] and John J. Keyes[2]

Origin and Classification

Slate has been defined as rock which is capable of being easily split into thin plates of nearly uniform thickness and with smooth surfaces.

Most slates have originated from marine deposits of clay and sand derived from granite rocks. It is supposed that the sediment of clay and sand, subjected to heavy vertical pressure, became cemented together and hardened, forming shale from the clay and sandstone from the sand, retaining the stratified formation due to their distribution by the sea water. These deposits of shale and stone have been shifted by the motion of the earth's surface, caused by the contraction of the interior and corrugation of the outer part of the earth, and have been folded at a considerable angle to their original position. This has metamorphosed the shale into slate.

Aqueous sedimentary slate may be divided into two classes: mica slate and clay slate.

Mica Slate.—The great pressure and heat in the presence of moisture caused a chemical readjustment of the constituents of the feldspar, kaolin, and iron of the shale, forming mica. The mica was produced in thin scales, usually arranged with their flat sides parallel to each other and facing the direction of the pressure.

Clay Slate.—The clay slates have a similar history with the exception that the transformation has not proceeded far enough to produce the micaceous texture. They may have lower strength than the mica slates.

It is principally the mica slate that is used in electrical work. It consists largely of parallel scales of mica among which are held particles of quartz, feldspar, kaolin, various carbonates, pyrite, magnetite, and other minerals which were not transformed into mica. The color of the slate is largely due to these minerals. Slates vary in color, including gray, purple, green, and red; the degree of fissility (the ability to split into parallel thicknesses); in texture; in their mineral and chemical compositions; and in their physical properties. The large number of factors entering into the formation of slate has resulted in many grades and wide variation in the quality even in the same district, making careful selection at the quarry necessary in order to obtain suitable material.

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[1] Materials Engineer, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.

[2] Materials Engineer, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.

Nearly all of the slate quarried in the United States comes from five districts:

The Vermont district, in Bennington and Rutland Counties, Vermont, and Washington County, New York.

The Maine district, in Piscataquis County, Maine. (The Monson quarries.)

The Lehigh district of Pennsylvania, including parts of Northampton and Lehigh Counties.

The Peach Bottom district, from York and Fayette Counties, Pennsylvania, to Hartford County, Maryland.

The Virginia district, in Buckingham and Fluvanna Counties, Virginia.

Slate for use in electrical apparatus comes almost entirely from the Vermont, Maine, and Pennsylvania districts.

The Vermont slate varies in color from purple to green and frequently shows considerable variation in the same piece. The Monson and Pennsylvania slates are of a dark gray color and are called black slate.

The Vermont and Monson slates have been used in electrical apparatus for many years. The increase in use of slate in these applications during the past ten years made it necessary to obtain additional sources of supply and the Pennsylvania slate, which had been used principally for structural and roofing work, has recently been used.

PROPERTIES OF ELECTRICAL SLATE

The physical properties of slate cannot be determined from the chemical composition, as the slate is simply metamorphosed shale which, in turn, is compressed clay, all three being composed of the same chemical elements. The physical properties depend upon the texture and the mineral composition of the slate.

Dale[1] gives the range of composition of ordinary slates as follows, omitting the less important constituents:

Silica	55	to	67	per cent.
Alumina	11	to	23	"
Ferric oxide	0.5	to	7	"
Ferrous oxide	0.5	to	9	"
Potash	1.76	to	5.27	"
Soda	0.5	to	4	"

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[1] N. T. Dale and others, "Slate in the United States," *U. S. Geologic Bulletin No. 586.*

Magnesia	0.88 to 4.5 per cent
Lime	0.33 to 5.2 "
Water (above 110° C.)	2.8 to 4.1 "

The same writer gives the mineral composition of average mica slate as follows:

Mica	38 to 40 per cent.
Chlorite	6 to 18 "
Quartz	31 to 45 "
Hematite	3 to 6 "
Rutile	1 to 1.5 "

Mica is a hydrous silicate of potassium and aluminum and is present in minute plates which may be distinguished only with a microscope of high magnification. Chlorites are of various kinds, the most common being hydrous silicates of aluminum and iron or magnesium. Clay is present in small amounts in mica slates and may be quite abundant in clay slates.[1]

REQUIREMENTS AND METHODS OF TEST

The principal requirements for slate for electrical apparatus are mechanical strength, machinability and electrical insulation.

The strength and toughness should be as high as possible, consistent with good machining qualities. In various applications such as switchboard panels, where the slate is subjected to bending stresses due to the weight of the apparatus supported, and also to the shock caused by the operation of the apparatus such as the opening of circuit breakers, a high modulus of rupture is important.

While the slate should have the degree of fissility necessary to allow it to be split at the quarry, it must be free from laminations which would cause it to split or to flake off while being drilled.

It must be sufficiently soft to allow it to be drilled without too rapid wearing of the drill. Similarly, it should be free from hard spots which would interfere with drilling.

There should be a dense structure which will prevent any appreciable absorption of moisture.

The slate must be free from metallic spots or veins, which would reduce the electrical insulation.

Slate is used in electrical apparatus principally for panel boards, switchboards, barriers, knife-switch bases, face plates for rheostats, and similar applications, to serve as a base on which to mount current-carrying parts. It has been the practice to limit the use of slate to circuits not exceeding 600 volts, alternating or direct current. Polished marble was formerly largely used, but in recent years slate has almost entirely superseded marble for apparatus for low voltages on account of its greater strength, lower cost, and the ease with which panels can be matched in color.

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[1] Oliver Bowles, "The Technology of Slate," U. S. Bureau of Mines *Bulletin No. 218.*

Color.—The Vermont slate varies considerably in color and when matching is required it is given a coat of a dull black lacquer, known as "Black Marine Finish." Monson slate and Pennsylvania slate are often given an oil finish by rubbing with linseed oil. When a very close matching of panels is required, panels of the same shade may be selected, or the panels may be given the Black Marine Finish. A black enamel finish, in which the enamel is applied and then baked, is sometimes used.

Machineability.—Machineability is determined by drilling holes with a sharp drill and noting the number of holes which can be drilled before it is necessary to sharpen the drill. While it is a matter of opinion as to when the drill requires resharpening, fairly uniform results can be obtained. This test also shows the tendency of the slate to split or to flake off at the edge of the hole.

Electrical Properties.—Two types of tests have been used to determine the electrical insulating properties: a dielectric test to indicate the breakdown voltage and various insulation tests to determine the leakage current.

Dielectric Test.—In the dielectric test the high voltage stress causes ionization, resulting in failure, due probably to the progressive breakdown of the small voids containing air or organic material. The dielectric test is of especial advantage in locating conducting spots or veins embedded within the slate which would be opened when the slate was drilled and reduce the insulation below a safe value. The testing transformer should be of sufficient capacity to maintain the testing voltage.

Insulation Tests.—Leakage occurs at all voltages to a greater or less extent, with a loss varying with the temperature and voltage. Several methods have been used for determining insulation resistance, of which the following may be mentioned:

Portable Testing Transformer: The test box which is in general use for making insulation tests on electrical apparatus is often used for testing slate. It consists of a transformer of about 2 kva. capacity with maximum voltage from 6000 to 10,000 volts, having the secondary winding connected by taps to two dials so that the test voltage may be varied in steps of 1000 volts by one dial and in steps of 100 volts by the other dial. The testing electrodes consist of brass rods about ½ in. in diameter with insulating handles. The slate is explored by moving the electrodes over the surface, keeping them several inches apart. The quality of the slate is judged by observing the amount of sparking. The regulation of these transformers is generally not good, so if there is large leakage of current through the slate there will be a considerable reduction in the test voltage. This test requires an experienced tester to judge the quality of the slate.

Voltmeter Method: A high resistance voltmeter is connected in series with the specimen across 500 volts direct current circuit, and the insulation resistance determined from the formula

$$R = \frac{r\,(V - v)}{v}$$

where r = resistance of the voltmeter,
V = voltage of the circuit,
v = voltage drop across the voltmeter resistance.

The specimen is tested between flat brass electrodes, 2 in. in diameter. Considerable variation in results may be obtained due to difference in contact resistance.

The Megger: The megger consists of a hand-driven, 500-volt, d. c. generator and a differential galvanometer with no restoring torque. One coil of this galvanometer is connected through the unknown resistance to the terminals of the generator and the other two coils are connected through a self-contained resistance to the generator. The moving element takes up a position where the torque of the current through the unknown resistance balances that due to current through the fixed resistance. The specimen is tested between flat brass electrodes, 2 in. in diameter. With the low insulation resistance of slate the readings are below the range of accuracy of the meggers in general use. Also considerable variation in results may be obtained due to difference in contact resistance.

Leakage Current Measurement, a. c.: The leakage of current through the specimen subjected to high voltage may be determined with a good degree of accuracy by measuring the increase in exciting current in the primary circuit of the testing transformer due to the current through the specimen. The no-load exciting current will be out of phase with the primary equivalent of the secondary load current, but the testing transformer usually has a high power factor when operated at low induction.

This method may be applied to obtain a more accurate test with the portable test box mentioned above. An ammeter is inserted in the primary circuit of the testing transformer. The slate specimen is laid on a flat metal sheet. One terminal of the transformer is connected to the metal sheet and the other to the brass rod electrode. A test voltage of 5000 volts is used in order to minimize the effect of poor contact and to get on the flatter portion of the resistance voltage curve. The upper surface of the slate is explored with the electrode and the increase in current over the no-load exciting current of the transformer noted.

Power Loss Measurement, a. c.: A still more accurate method provides a wattmeter instead of the ammeter in the previous test and determines the power loss in the specimen. The specimen is placed between the two flat brass electrodes, 2 in. in diameter, connected to the high-tension side of the testing transformer. A wattmeter and voltmeter are connected to the low-tension side of the transformer. A test voltage of 5000 volts is used. The watt loss in the slate is obtained by subtracting the core loss of the transformer from the total watts. This includes slight errors due to copper loss of the transformer and loss in the meter, which are negligible. The transformer should be of sufficient capacity to maintain the test voltage under the worst conditions of leakage, and for close accuracy should have a core loss at the test voltage not greater than the maximum watt loss permissible in the slate. Five tests are made on each specimen, one at the center and one near each corner.

This test can be made rapidly and accurately but is essentially a laboratory test.

Porosity.—The Pennsylvania slate is of a more open structure than the Vermont slate and contains a much larger percentage of moisture when quarried. Some of the producers recommend that the Pennsylvania slate should not be used until three months after being quarried in order to allow for air drying.

Specimens about $\frac{3}{8}$ by 2 by $2\frac{1}{2}$ in. in size were dried to constant weight and soaked in water at room temperature to determine their absorption of water with the following results:

	INCREASE IN WEIGHT	
	AFTER 2 HOURS IN WATER	AFTER 100 HOURS IN WATER
Pennsylvania slate	0.173 per cent	0.574 per cent
Pennsylvania slate	0.214 "	0.712 "
Pennsylvania slate	0.243 "	0.744 "
Average	0.210 "	0.677 "
Vermont slate	0.020 per cent	0.151 per cent
Vermont slate	0.023 "	0.157 "
Vermont slate	0.022 "	0.152 "
Average	0.022 "	0.153 "

Three specimens of Pennsylvania slate from the Slatington district and three of Vermont slate, $\frac{3}{8}$ by 2 by $2\frac{1}{2}$ in. in size, were dried at 100° C. to constant weight, and were stored in the laboratory on edge, spaced about $\frac{1}{2}$ in. apart to allow circulation of air. The specimens were weighed once a day for ten days and the atmospheric humidity noted. Three similar specimens of each grade of slate were soaked in water for one hour and similarly stored and tested. The variation in weight is shown in Fig. 1. The absorption of moisture was much greater by the Pennsylvania slate than by the Vermont slate.

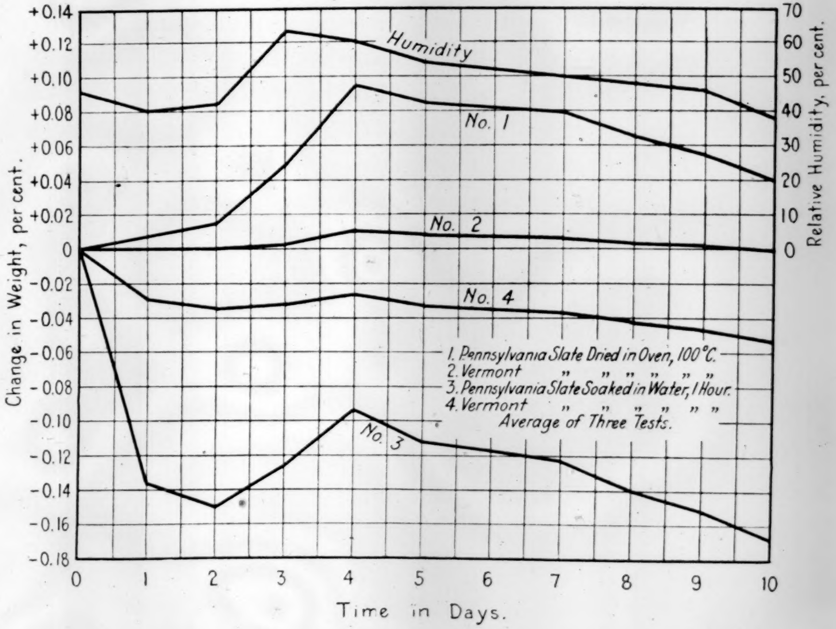

FIG. 1.—Change of Weight of Slate Exposed to Atmospheric Humidity

In order to determine the variation in the insulation of slate due to atmospheric moisture, six pieces of Vermont slate and sixteen pieces of Pennsylvania slate were tested. Ten pieces of the Pennsylvania slate had high moisture content and six were dried to low moisture content. The specimens were stored in the laboratory, stacked one above the other, with spacing strips about $\frac{3}{8}$ in. thick between them, and were tested for watt loss each day by the method previously described in this paper. The average variation in losses is shown in Fig. 2 for the Pennsylvania slate with high moisture content, and in Fig. 3 for the Vermont slate and Pennsylvania slate with low moisture content, together with the atmospheric humidity during the test period.

To determine the effect of atmospheric moisture on the insulation of slate over a considerable period of time, twenty pieces of Vermont slate (ten ¾ by 12 by 16 in. in size and ten 1 by 16 by 16 in.) and twenty pieces of Pennsylvania slate of the same dimensions were dried in vacuum for 36 hours at 140° C. to remove all moisture. They were then stacked without spacing strips and were tested for watt loss at intervals for one year by the method previously described in this paper. The average variation in losses is given in Fig. 4. Each point on the curve represents the average of 100 readings on 20 specimens.

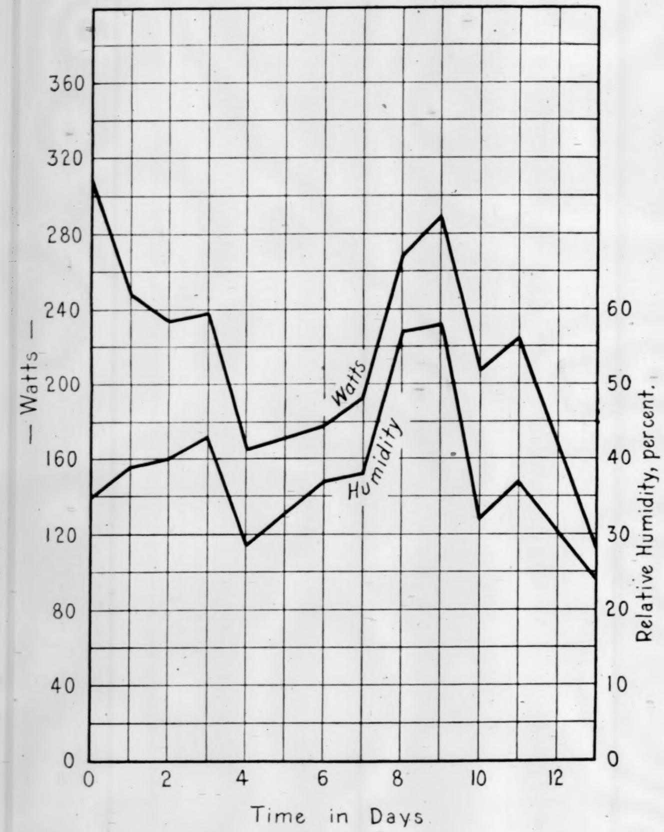

FIG. 2.—Watt Loss of Pennsylvania Slate Exposed to Atmospheric Humidity. Average of Ten Tests

The average watt loss before vacuum drying was 47 watts for the Vermont slate and 455 watts for the Pennsylvania slate. After drying, no watt loss could be detected in any of the specimens. The test was begun on January 26 and shows that the moisture content varies in accordance with the seasonal variations in atmospheric humidity. The periods of low losses toward the beginning and end of the tests correspond to the time when the building was heated.

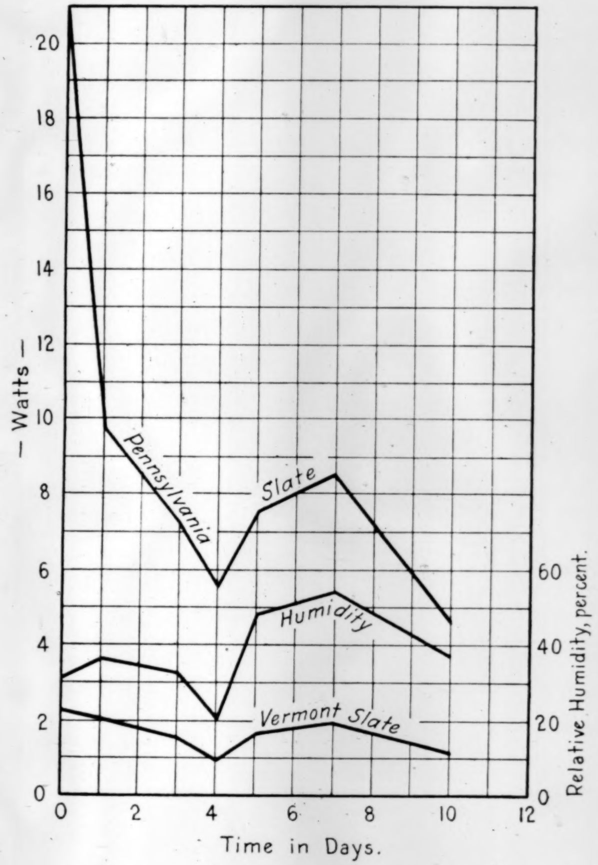

FIG. 3.—Watt Loss of Slate Exposed to Atmospheric Humidity. Average of Six Tests

The tests shown in Fig. 2 were made on specimens from the Slatington district. The source of the specimens for Figs. 3 and 4 is not known.

In general, it is of course desirable to have the moisture content of electrical slate as low as possible. The maximum moisture content which can be safely allowed is still a matter of opinion and depends somewhat upon the application.

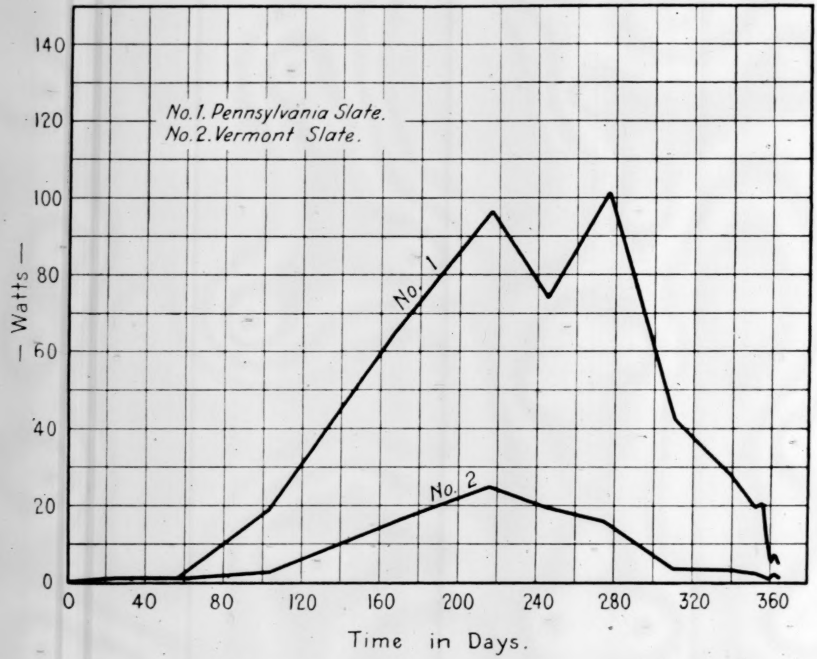

FIG. 4.—Watt Loss of Slate Panels Exposed to Atmospheric Humidity. Slate Dried in Vacuum at 140° C. Average of Twenty Tests

Porous slate can be dried and given a waterproofing treatment, but this involves an additional manufacturing operation and increased cost.

INSULATION TESTS OF SLATE FOR ELECTRICAL USE

BY R. M. SPURCK[1]

Natural black slate is now and has been used almost exclusively for a number of years as the mounting structure for meters, instruments, switches, and other apparatus forming the switchboards for the control of electric power. It is common practice on switchboards for alternating currents of voltages up to 440 volts and direct current voltages up to 600 volts and in some cases 1200 volts,[2] to mount apparatus, whenever necessary, so that the slate is the entire insulation between devices or parts of devices of opposite polarity. Inasmuch as it is obviously desirable to maintain the separation of parts of opposite polarity as small as is consistent with mechanical design and safety, it is extremely desirable to have the slate of good and reliable insulation. It is the purpose of this paper to describe the early methods of testing the insulation of slate and discuss the development, characteristics, and results obtained from a new method of making insulation tests on this material. This latter method of test is now being used and has proved satisfactory for the selection of slate for switchboards by a large manufacturer of such equipment.

The electrical characteristics of slate of most importance in determining whether or not it will be suitable for the service intended on switchboard work are:

1. The voltage required to puncture the material;
2. The leakage current that will flow at various voltages between electrodes of opposite polarity.

For a number of years it was our practice to check the electrical quality of the slate by making puncture and leakage tests as follows: The puncture test was made at 60 cycles alternating current on samples 2 in. thick selected at random. Voltage was applied at zero voltage and raised at a rate of approximately 1000 volts per second between flat disk electrodes 1 in. in diameter with rounded edges, placed directly opposite each other on the two sides of the sample. Leakage was also determined with 60-cycle alternating current between spherical electrodes 1 in. in diameter, located as described for the puncture voltage test. The voltage was gradually raised until, in a darkened room, there was a visual appearance of leakage at the terminals. Under these conditions, material having a puncture voltage of not less than 9000 volts and the appearance of leakage at not less than 1500 volts, was considered satisfactory from an electrical standpoint. Results of tests on various samples of material obtained from various widely separated quarries are given in Table I. These tests were made on samples as received from the quarries. No record of the previous history of the sample was available.

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[1] Switchboard Engineering Department, General Electric Co., Schenectady, N. Y.

[2] Material for 1200-volt service requires special selection.

TABLE I.—RESULTS OF PUNCTURE AND LEAKAGE TESTS ON SLATE

Quarry	Number of Trials	Puncture Voltage			Number of Trials	Voltage of Initial Visible Leakage		
		Maximum	Minimum	Average		Maximum	Minimum	Average
Number 1	5	13 800	5 400	10 000	36	1 550	1 000	1 350
Number 2	6	15 000	10 200	11 500	15	1 450	1 150	1 330
Number 3	18	10 200	8 000	8 900	27	1 100	900	1 000
Number 4	4	7 300	7 200	7 300	16	1 250	820	960
Number 5[a]	3	5 000	3 500	4 100	4	1 050	750	910
Number 5[b]	3	5 200	4 800	5 000	4	825	775	800
Number 6	4	6 600	6 100	6 240	51	1 700	1 000	1 300
Number 7	3	12 000	11 500	11 800	8	1 400	1 000	1 120
Number 8	10	11 000	9 800	10 200	10	2 600	1 800	2 300
Number 9	10	12 000	10 600	11 400	9	2 800	2 000	2 400
Number 10	10	12 200	10 900	10 500	10	2 300	1 560	1 700
Number 11	10	10 800	9 700	10 200	10	1 600	1 440	1 500
Number 12	10	12 400	9 000	10 840	10	2 200	800	1 670
Number 13	10	9 800	9 000	9 500	10	2 160	1 320	1 800
Number 14	10	10 900	10 000	10 500	10	1 480	1 000	1 300
Number 15	14	11 000	9 200	10 000	15	3 600	3 400	3 400
Number 16	10	11 600	9 600	10 700	10	2 040	1 500	1 700
Number 17	10	28 000	25 400	26 900	10	6 600	5 000	5 430
Number 18	4	34 000	30 000	33 000	36	3 800	2 100	3 000

[a] Sample 0.6 in. thick.
[b] Sample 1.75 in. thick.

This method involved the puncturing of the material, which obviously meant that it could not be made on all pieces used commercially. Also, it was necessary to make the leakage test in a darkened room and inasmuch as the appearance of the leakage current was a visual determination, it depended to a large degree on the experience and judgment of the observer. Even with these limitations, however, the method proved satisfactory so long as the commercial requirements were obtained from a limited number of quarries which experience had shown to be capable of producing material of reasonably consistent quality. However, as demands increased, it became necessary to resort to additional quarries to supply our requirements. It is also of interest to note that the quality of the product of a quarry may vary from that satisfactory for switchboard work to that entirely unfit. The desirability of making tests on each piece of material used commercially, therefore, became evident. A study of the problem indicated that although there was no definite relation between the appearance of leakage and the puncture voltage, samples that had a high leakage resistance could be relied upon without exception to have a puncture sufficiently high to be satisfactory. The answer accordingly seemed to be in the development of a method of determining the leakage current easily without injuring the sample or making it necessary to rely too much on the judgment or experience of the observer. Insulation resistance tests at low voltage, that is, voltages up to 500 volts, indicated that such tests did not give results from which the leakage current with alternating current at higher voltages could be predicted. The reason for this is that the leakage current increases rapidly with increase in voltage. Even though the insulation resistance characteristics determined at 500 volts direct current were comparable on various samples, the leakage currents at higher voltages alternating current were, many times, widely different. A consideration of those factors indicated the desirability of measuring the leakage currents with alternating current at some voltage sufficient to give satisfactory readings but still low enough not to injure the material.

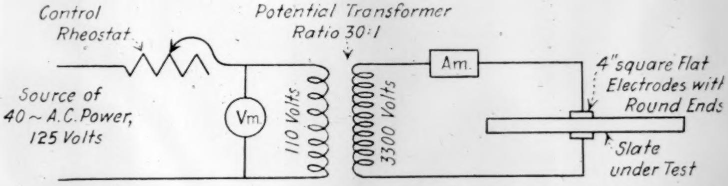

FIG. 1.—Schematic Diagram of Circuit for Measuring Leakage Current Through Slate

The first method devised for making such determination was to use a circuit shown in Fig. 1. This method involved merely the measurement of the leakage current through the sample at various places when a voltage of 3300 volts was applied. The voltage, 3300 volts, and the size of the electrodes were selected by experience. They are both purely arbitrary. The combination makes it possible to obtain satisfactory readings on commercially available ammeters.

Tests using this method showed that material having a leakage current of not in excess of 150 milliamperes was satisfactory for switchboard work and also that material having such leakage characteristics was obtainable commercially. This method, however, had the objection that it was necessary to use and take readings on a very sensitive ammeter in the high-voltage circuit. This ammeter would be damaged in the case of a short circuit of the testing circuit or puncture of the test sample. A modification that eliminated these objections was then tried. The circuit of the modified scheme is shown in Fig. 2. The principle of operation of this method is that if the leakage in the high-voltage circuit is changed, such a change will be evidenced by a change in the current in the low-tension circuit. As all current in the low-tension circuit passes through the resistor, Fig. 2, the magnitude of the current flowing in the low-tension circuit and likewise that in the high-tension circuit will be indicated by the difference in readings of V_1 and V_2.

This method gives essentially the same results as the method illustrated in Fig. 1 and has the advantage that there is no sensitive ammeter in the high-voltage circuit. The relation is shown in Fig. 3. Curve No. 1, showing the relation between readings of voltmeter V_2 and the leakage current in the high-voltage winding, was determined by actually placing samples of slate having various values of impedance between the electrodes and measuring with an ammeter the current in the high-voltage circuit. This curve is therefore a calibration curve for the particular arrangement of apparatus and takes into account, in general, the variation in the power factor of the circuit as material of varying impedance is used. It is not claimed that absolute and accurate values of impedance will be obtained but the values are sufficiently accurate for the selection of material.

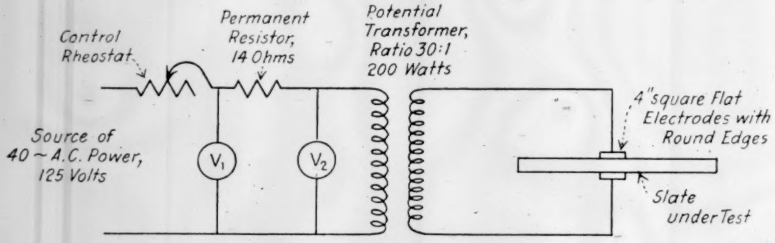
Control Rheostat
Permanent Resistor, 14 Ohms
Potential Transformer, Ratio 30:1 200 Watts
4" square Flat Electrodes with Round Edges
Source of 40 ~ A.C. Power, 125 Volts
V_1
V_2
Slate under Test

FIG. 2.—Schematic Diagram of Modified Circuit for Measuring Leakage Current Through Slate

Curve No. 2, Fig. 3, showing the apparent approximate ohmic impedance, was calculated from the voltage applied at the terminals of the slate under test and the current flowing in the high-voltage circuit as shown by an ammeter and indicated by voltmeter V_2.

Inasmuch as the voltage impressed on the test piece varies with the leakage current when the scheme of test shown in Fig. 2 is used, it is difficult to relate the actual value of leakage current as obtained with this method with the leakage obtained when the voltage across the terminals is maintained constant as shown by the method described for Fig. 1. However, samples that indicated satisfactory leakage in accordance with the method shown in Fig. 1 were retested as shown in Fig. 2, and it was found that a minimum value of 70 volts on voltmeter V_2 gave material comparable with that previously adopted as satisfactory. This 70-volt value applies to material 2 in. thick. The same value is also used for material $1\frac{1}{2}$ in. thick. In that case, slightly higher insulation material is selected but the difference is not sufficient to warrant the use of two standards of the value of V_2.

This method of testing is very simple in manipulation. It is only necessary for the tester to maintain the supply voltage constant and note that the reading of voltmeter V_2 does not fall below a specified minimum value.

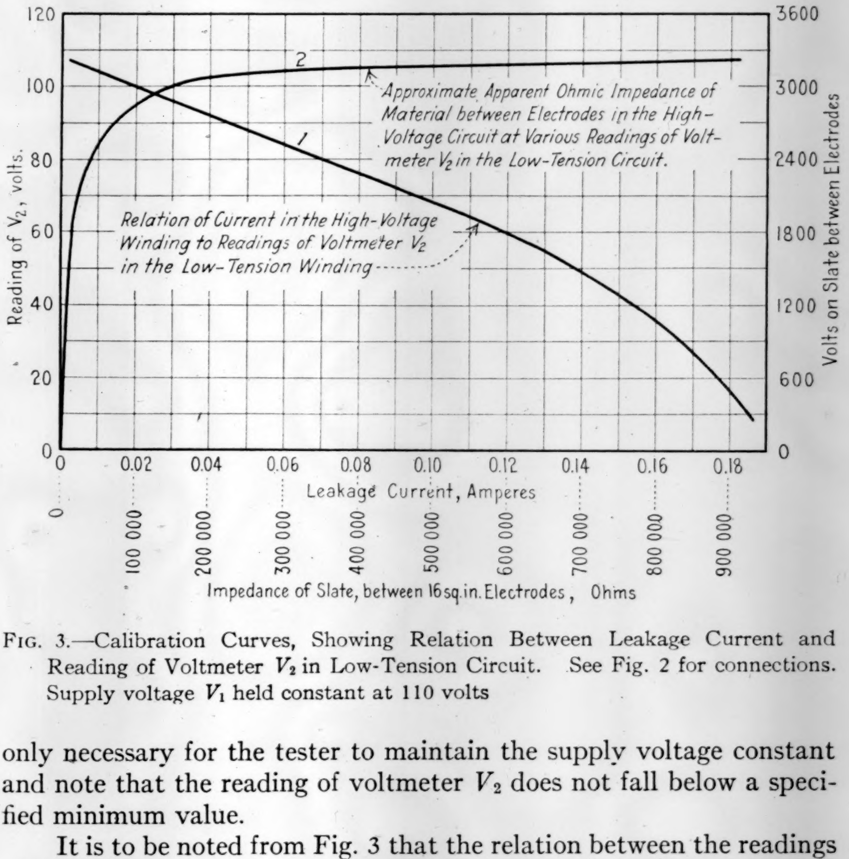

FIG. 3.—Calibration Curves, Showing Relation Between Leakage Current and Reading of Voltmeter V_2 in Low-Tension Circuit. See Fig. 2 for connections. Supply voltage V_1 held constant at 110 volts

It is to be noted from Fig. 3 that the relation between the readings of voltmeter V_2 and the leakage current through the slate under test is not a straight line function but that the sensitiveness is increased as the amount of leakage increases. The sensitiveness is, however, maximum at large leakage current and is still high enough at the value of leakage current decided upon as maximum for good material to

TABLE II.—COMPARISON OF LEAKAGE OBTAINED ON GROUPS OF SAMPLES BEFORE AND AFTER BAKING. PERCENTAGE OF PIECES SHOWING MINIMUM, MAXIMUM, AND AVERAGE READINGS OF V_2

For diagram of connections, see Fig. 2

	Before Baking															After Baking at 110° C.														
	Group 1			Group 2[b]			Group 3			Group 4			Group 5			Group 1			Group 2[b]			Group 3			Group 4			Group 5		
	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average
Number of pieces tested	15	15	15	26	26	26	20	20	20	19	19	19	20	20	20	15	15	15	26	26	26	20	20	20	19	19	19	20	20	20
Hours of Baking	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	16	16	16	24	24	24	48	48	48	72	72	72	115	115	115
In excess of 90 volts	0	7	0	0	0	0	0	5	0	0	0	0	0	0	0	20	100	67	35	92	46	80	100	90	72	100	83	95	100	100
" " " 80 "	7	78	33	0	65	15	50	80	35	5	42	0	0	5	0	67	100	73	73	96	73	90	100	100	83	100	100	100	100	100
" " " 70 "	47	100	67	35	88	54	80	90	80	50	53	47	10	25	15	73	100	80	77	96	85	90	100	100	95	100	100	100	100	100
" " " 60 "	67	100	93	65	96	92	90	100	95	68	79	58	20	65	25	73	100	100	77	96	96	90	100	100	95	100	100	100	100	100
" " " 50 "	73	100	100	77	100	96	95	100	100	90	100	84	60	100	85	93	100	100	88[a]	100	96	95	100	100	100	100	100	100	100	100
" " " 40 "	100	100	100	96	100	96	100	100	100	100	100	100	100	100	100	100	100	100	96	100	100	100	100	100	100	100	100	100	100	100
" " " 30 "	100	100	100	96	100	100	100	100	100	100	100	100	100	100	100	100	100	100	96	100	100	100	100	100	100	100	100	100	100	100
Under 30 volts	0	0	0	4	0	0	0	0	0	0	0	0	0	0	0	0	0	0	4	0	0	0	0	0	0	0	0	0	0	0

[a] Two samples at 48 volts lower this percentage.

[b] One very poor sample in group 2 before baking. This sample was very poor after an additional baking of 168 hours

insure easily readable and accurate results. The variation in sensitiveness with current changes in the high-voltage circuit, without resorting to a complex vectorial explanation, is due to the effect of the exciting current of the transformer. For small currents in the high-voltage circuit, up to currents of the magnitude of the transformer exciting current, there is little change in the low-voltage current and a corresponding small change in the value of V_2 because when the leakage current through the slate is low, the current flowing in the high-voltage winding is principally the charging current of the condenser formed by the testing terminals. This charging current leads the voltage. Then as the leakage through the slate increases, the added current does little more than change the power factor of the low-tension circuit with little change in the current in the low-voltage circuit. The variation in sensitiveness of the apparatus is emphasized by curve No. 2, Fig. 3, showing the relation between apparent impedance of the material with the readings of voltmeter V_2.

Table II gives the results of tests made on one hundred samples of slate obtained from quarries which experience has shown produce essentially comparative material. The size of the samples varied so that the number of tests on each sample was not the same. Tests made averaged 21 per piece, with a maximum of 42 and a minimum of 15 per piece.

These tests indicate that:

1. Twenty-eight per cent[1] of the product as received has an insulation equal to or greater than the established standard and may be used as received.

2. Sixty-seven per cent of the product not meeting the standard of insulation as received will be brought up to the standard of insulation and may be used if baked for 24 hours at a temperature of 110° C.

3. Eighty per cent of the product not meeting the standard of insulation as received will be brought up to the standard of insulation and may be used if baked for 48 hours at 110° C.

4. Nearly all of the slate could be brought up to standard by baking a sufficient length of time.

5. Some slate could never be brought up to standard. The indications are that this low insulation is not due to metallic veins but to some other substance in the composition of the slate.

This method of testing has been used for some time and it has been found that switchboards constructed of material tested to the standards selected will meet all insulation test requirements when completed.

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[1] It is to be noted that these percentages were obtained on material supplied commercially by a number of quarries in a limited geographical district. Although these percentages are characteristic of the regular product supplied at this time by such quarries, they may not apply to the product of some other quarry or group of quarries.

In practice, the material as received is given an insulation test as described. Those samples which do not meet the standard of insulation, namely, a leakage current of 0.095 amperes at 2100 volts between 4-in. square electrodes or a leakage of 0.0053 amperes per 1 sq. in. of section through the slate between the electrodes, are baked for 48 hours at a temperature of 110° C. The pieces after baking are again tested and those below standard re-baked as before and retested. Those pieces which will not show an insulation up to the established standard are rejected. Experience, however, has shown that it is necessary to reject only about 3 per cent of the pieces, although the percentage rejection varies somewhat but not widely in different lots from essentially the same quarries. In this connection, it is to be noted that quarries supplying slate for electrical purposes are able to select and reject by observation material that is entirely unsuited for electrical uses.

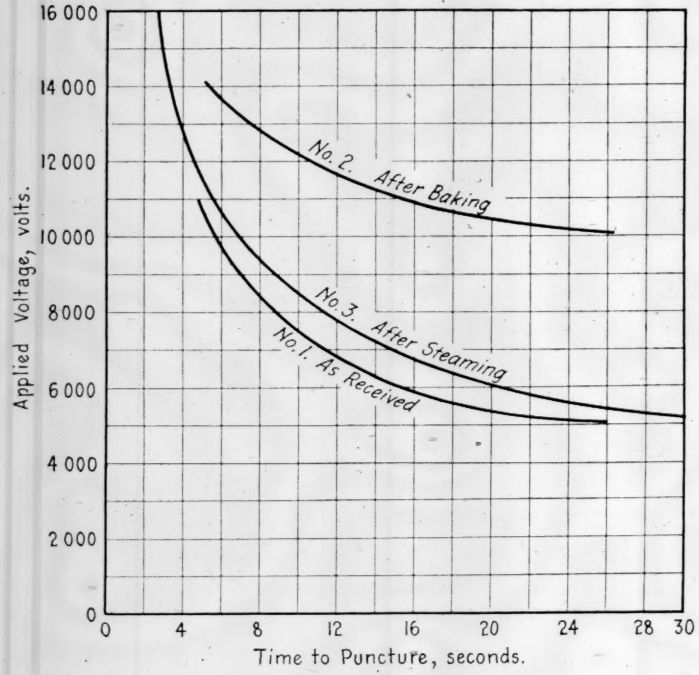

FIG. 4.—Showing Time Required to Puncture Samples of Slate at Various Applied Voltages

In addition to the results listed above, various other characteristics of slate that have been found are noted below. This summary of characteristics, although it applies in a general way to all slate, has as its basis the selected product of a limited number of quarries.

1. The puncture voltage is not affected materially by the thickness of the piece considering a range of thickness from 1 to 2 in. This characteristic is difficult to represent by actual test data due to the variation in individual pieces, but general experience in testing has indicated that such is the case.

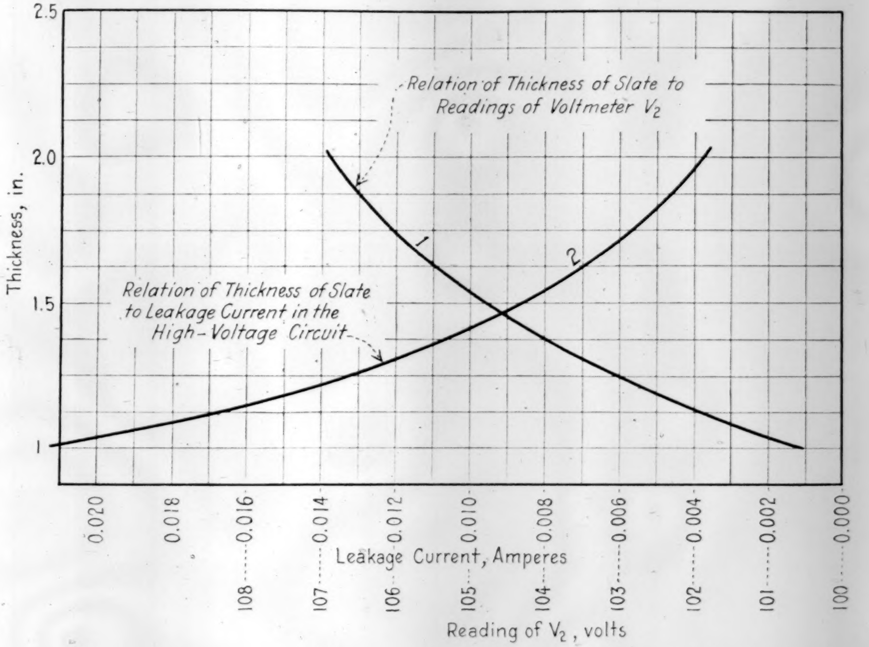

FIG. 5.—Showing the Relation Between Thickness of Slate and Insulation Resistance of Slate. See Fig. 2 for connections

2. The voltage required to puncture the material is a function of the time of application of voltage. That is, the longer the time of application of voltage, the lower the puncture voltage. This is illustrated in Fig. 4, which gives the results of tests made on samples 2 in. thick with 1-in. diameter flat disk electrodes with rounded edges.

3. The puncture voltage may, in general, be increased by baking. (See Fig. 4.) Curve No. 2 was obtained on the same sample of material used for curve No. 1, but had been baked for 48 hours at a temperature of 110° C.

4. The puncture voltage may, in general, be decreased by steaming. (See Fig. 4.) Curve No. 3 was taken after the sample used for curves Nos. 1 and 2 had been steamed at a temperature of 65° C. for 36 hours.

5. The thickness of the slate does somewhat affect the insulation resistance of the slate. Fig. 5 shows the results of tests on especially selected slate of as nearly the same character as possible and of very high insulation. This curve is, therefore, not quantitative but an indication of the relation between thickness and leakage.

Baking of slate increases its hardness and renders it more difficult to drill. The hardness will also increase naturally as the slate is aged. Although no experiments have been made, it is believed that natural aging in a dry atmosphere will produce essentially the same effect as baking.

Acknowledgment is made to Mr. C. B. Springer for assistance in the preparation of this paper and for the determination of the laboratory data.

GENERAL DISCUSSION ON SLATE

Mr. Kessler

MR. D. W. KESSLER[1] (*presented in written form*).—Slate has probably received less attention from the point of physical testing than any other structural material. This may be due partly to the lack of prescribed methods of procedure, but probably more to the fact that this material seldom gives trouble in actual use.

The physical properties and uses of slate are such that many of the laboratory tests commonly made on similar materials do not meet the requirements. For instance, the determination of the toughness has been pointed out as a valuable test on slate. The usual test for this property on stone consists in determining the resistance of the material to a series of impacts on cylindrical specimens. This test does not compare with any condition to which slate is submitted in use, hence it would be of little value. An impact test on thin slabs like shingles to determine the brittleness or resistance to hammer blows would seem to be more to the point. One experimenter made a series of tests in which he determined the amount which thin specimens would bend before breaking. He used this factor as an index of toughness. This test affords some valuable information, but his results are subject to criticism because the thickness of the specimens varied from $\frac{3}{16}$ to $\frac{1}{4}$ in., which would give rise to a variation of about 30 per cent in the results. The flexure formula for a simple beam loaded at the center is:

$$\Delta = \frac{wl^3}{4Ebd^3}$$

in which Δ = the amount of bending at the center
w = load at center
l = distance between end supports
E = modulus of elasticity
b = breadth of specimen
d = depth of specimen

One can determine the factors w and E from the transverse test and then compute the maximum deflection Δ which seems to offer a satisfactory means of comparing the toughness of different slates. This property is of value in the use of slate because a material that will bend considerably under load is less apt to break while being

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[1] Chemist, Warner-Quinlan Co., Grasselli, N. J.

Mr. Kessler handled than one that bends only slightly. From the flexure formula it is seen that a thin slab or shingle will bend more before breaking than a thicker one of the same strength and elasticity.

The determination of strength and elasticity in transverse bending offers no particular difficulties. The determination of compressive strength of slate is of doubtful value because in actual use it is seldom placed under conditions where the compressive strength is the governing factor. The absorption of slate is frequently referred to as porosity, which is wrong and misleading. Porosity refers to the volume of the pore spaces in the material, and is usually expressed as a percentage. When one says that a material has a porosity of 1 per cent he means that 1 per cent of the volume is void space while the rest is solid matter. The pores do not become filled with water in a short period of immersion such as 24 hours or even several days. Furthermore, absorption values are usually expressed as a weight ratio and have to be multiplied by the apparent specific gravity of the material before they are comparable with porosity. The actual pore space can be determined by making a specific gravity test on the original material and then grinding the sample to a very fine powder for determining the true specific gravity. From these two values the actual pore space can be computed.

Weathering tests can be made in the laboratory, as on other similar materials, by freezing and thawing or by causing an inorganic salt to crystallize in the pores which causes an action similar to frost. Freezing tests usually result in splitting the slate along the natural cleavage planes, hence tests of this kind are of little value unless the specimens are worked down to thin slabs and allowed to season. Specimens of slate recently tested at the U. S. Bureau of Standards showed very little disintegration during five hundred freezings.

Due to the generally inert nature of the minerals which form the principal constituents of slate it is not likely that acid tests will prove of much value for the determination of durability. However, it seems that tests of this nature could be developed which would simulate the change in color of the material due to weathering.

Abrasion tests are of value in considering slate for use in steps, floor tile and similar purposes. This test should be such as to give results comparable to the actual wear under such conditions. The Deval abrasion machine is not satisfactory for this test because the wear produced by it is due mostly to impact. The Dorry apparatus would be more satisfactory but this does not entirely simulate the actual conditions. There is a great need for a wear testing machine suitable for flooring materials in general.

MR. N. M. MALE.[1]—I assure you that it affords us a great deal of pleasure in being here to-day, for although the technical phases of the discussions are somewhat beyond us we nevertheless enjoyed them and were intensely interested when the physical characteristics and mode of operation were discussed. **Mr. Male**

Our industry has been somewhat backward in extolling the merits of our product, due largely to the characteristics of the people engaged in the business. They have concentrated almost their entire attention on production problems and only recently awakened to the fact that architects and engineers are anxious to learn the merits of their material and where it will serve to the best advantage. This has caused us to effect organizations and secure the services of individuals qualified to put the matter in proper form to supply architects and engineers with what I hope they want.

We have separate organizations to handle our structural slate and blackboards and have recently formed the National Slate Association, each performing its own research work and compiling the results into booklets and folders which are given wide circulation. Mr. Hays, of Philadelphia, looks after the interests of the National Slate Association; Messrs. Boyd and Abel act as architectural advisers and structural standardists; Mr. Notvest, of Cleveland, serves as electrical engineer.

Last evening the slate producers of Maine, Vermont and Pennsylvania had a round-table discussion of some of the papers to be read here today. Our attention was particularly directed to Mr. Bowles' paper because the subject matter could be more easily comprehended by us than the contents of the other papers dealing with the electrical qualities of our material.

The gentlemen from Maine, Vermont and Pennsylvania selected me as their representative to express here to-day their appreciation of the scholarly and satisfactory manner in which Mr. Bowles prepared his paper. We have no criticisms to offer but only comments of approval, realizing the thorough research and study entering into the preparation of his paper. Because of the technical terms contained in the other papers we have requested Mr. Notvest, consulting engineer of The Structural Slate Company, Pen Argyl, Pa., to comment on them. Mr. D. Knickerbacker Boyd will stress certain points on the structural work; Mr. Barker, of the F. C. Sheldon Slate Company, on roofing, and Mr. Kitto, of the Natural Slate Blackboard Company, on blackboards.

I again wish to thank you for giving us this attention and I hope

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[1] Jackson-Bangor Slate Co., Pen Argyl, Pa.

Mr. Male a committee will be appointed that will establish a more intimate relationship between the American Society for Testing Materials and the slate industry.

Recognition of the merits of our material by the user has given it the place on the market it now has, so surely with increased effort on our part in circulating our findings there will be an increased use. I have had several interviews with the General Electric Company, both the Electrical and Purchasing Departments, and find that they have decided that slate is the very best material they can use for switchboards—and they use large quantities of it. We are deeply interested in our National Slate Association and through it and our staff of engineers and professional men hope to render a service that will be a credit to our industry.

Mr. Barker MR. C. W. BARKER.[1]—There are one or two points I should like to emphasize in connection with the roofing slate feature of the industry, and one is this, that quite frequently we come in contact with the more or less prevalent but mistaken impression, that the ordinary dwelling is not strongly enough constructed to support what we term standard commercial slate. Now in a general way the industry has met this argument with the assurance that any building that is built strongly enough to support wooden shingles will support commercial slate, on the theory that a shingle roof loaded with snow, is heavier than any two slate roofs, from the fact that snow will not adhere to slate. It slides off as soon as the sun strikes it. It seems to me that we ought to have more explicit and technical facts along this line to better support our contention. There is one more disadvantage that the industry has always contended with, and that is a tendency on the part of some architects, apparently for no other reason than the fact that they may have used a certain size before, or that they like a size that is twice as long as it is wide (such for instance as 10 by 20 in. or 8 by 16 in.), to insist on slate of a certain size in all of their specifications for slate roofs. We advocate that architects be urged to use a little more latitude in the writing of their specifications as far as size goes, permitting the manufacturer, in case one size is not available, to use the same length in varying widths. Architects might specify 8 by 16 in. It might be impossible for the manufacturer to make timely delivery of an order in that size, but if varying widths could be furnished, such as 16 by 8, 9, 10 and 12 in., especially when stocks are low, it would go far toward expediting shipment. At the same time, a much more interesting roof would be the result, as the use of these varying widths relieves the monotony of equal rectangles, and breaks up the vertical and diagonal line effect so obvious in a roof laid with one size only. The use of slate in one length and random widths, as well as in random lengths and widths, in commercial as well as architectural types, is steadily increasing in popularity. **Mr. Barker**

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[1] F. C. Sheldon Slate Co., Granville, N. Y.

Mr. Boyd MR. D. KNICKERBACKER BOYD.[1]—It is my pleasure and privilege to act as consulting architect to the slate industry. In connection with our organization, the Structural Service Bureau, I, and associates like Mr. Abel, have devoted a considerable amount of attention during the last four or five years to the quarrying, production, manufacturing, testing and uses of slate. In order to keep within the time limit set for this discussion, I will refer merely to one feature of standardization which has a very important bearing upon the production and use of slate. That relates to the dimensional requirements in one large utilization of slate, namely, for what are known as toilet enclosures. We found that architects and engineers, and builders where no architects and engineers are engaged, would generally put a battery of toilet enclosures in the corner of a space or against a wall and would show four, five or six enclosures, putting them in practically without regard to the actual width or length of each, merely providing that there would be so many from a certain door to a corner or from a corner to a window, etc. Of course the designers had in mind a convenient size for the occupancy, but there would be no regard on their part to making the length or the width of the space such as to accommodate units of any regulation size—and in fact none prevailed. So our organization made measurements in buildings of all kinds—schools, hotels, apartment houses, industrial buildings and all others. We found that the widths of toilet enclosures ranged all the way from 2 ft. to 3 ft. and more—the units differing not only by inches, but quarters, half inches and all kinds of refinements, the object in each instance apparently being to get a certain number of units of exactly the same width into a given space. We found that the lengths ranged from 3 ft. to 4 ft. 6 in., also without regard to any standardized sizes of the partitions and end slabs. So we said, "Why wouldn't it be perfectly simple to adopt standardized widths of 2 ft. 8 in. and 2 ft. 10 in. and have three lengths, 3 ft. 6 in., 4 ft. and 4 ft. 6 in.?" And so, with the slate producers, we prepared to standardize on those sizes. And we found, through consultation with architects and engineers in different parts of the country, that this had never been brought to their attention before. Realizing that this natural product could be better utilized if quarried and milled and carried in stock in slabs of definite sizes, those architects approached on the subject have agreed

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[1] Structural Service Bureau, Philadelphia, Pa.

Mr. Boyd under all normal conditions to plan the spaces in their buildings to suit toilet enclosures of standard sizes, and to acquaint their draftsmen with the publications put out showing these standard sizes. On the other hand this does not mean absolute uniformity for, if any plan would show that from center to center of the partitions and ends there must be 2 ft. 8 in. in the width, it would mean that the end enclosures would have to be ½ in. smaller on the back than all the others, and that the ends would have to be one inch deeper than the partitions. So we establish as a standard that the two end enclosures would each be ½ in. less in width than all the remaining enclosures. We therefore got up a table of lengths required to accommodate three, four, five, eight enclosures and so on—and these accompany the drawings of each type enclosure in Chapter 5 of the Series on Structural Slate, which I show you as an example. That means that every back would be exactly the same size, and every partition and each end slab would be the same size, and all would be interchangeable. Close attention on the part of planners and specifiers to these standards would make it possible for the slate industry to produce during winter months in their quarries and mills, and stock for later and prompt delivery. The only thing that remains is for architects and engineers and the building public in general to be made familiar with the fact that these standards have been established and order their material accordingly, when not in conflict with any specialized requirements. If that is done, if we can get that message across to the American people, as has been advocated by Mr. Bowles himself in his fine work for the U. S. Bureau of Mines, and by Mr. Laughlin, of the U. S. Geological Survey, and will be, I trust, by the Department of Commerce, it will effect important results in the industry, in marketing and transporting its product, and make for continuity of employment for the working man and for conservation and convenience all around.

Mr. Kitto MR. W. A. KITTO.[1]—Mr. Bowles has emphasized one fact of great importance to the slate industry and that is with reference to slate blackboards. Slate blackboards are being used by practically every city in the United States in preference to other blackboard materials on the market, and one difficulty that we have is in educating the architects who are responsible for the specifications, as to what they can expect from a natural product. Mr. Bowles has mentioned the small percentage of the total volume of slate that is manufactured into slate blackboards due to the natural condition of the rock; there should be some means of conveying these facts to the architects so that they would realize the hardship they are working upon the industry when they specify material that can not be manufactured. **Mr. Kitto**

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[1] General Manager, The Structural Slate Co., Pen Argyl, Pa.

MR. H. E. FLETCHER.[1]—As I understand the situation here to-day, this Society is considering the adoption of specifications and standard tests. That being the fact, it seems to me that probably that could be handled by a committee and this is not the time nor place to go into suggestions as to what those specifications and tests should be. Therefore I will simply express my appreciation of the courtesy extended by the Society. When a committee is appointed, I will be glad to appear before it and tell it what I think the specifications and tests should include. I believe there is a necessity for such specifications and tests, for the reason that any failure that occurs in the use of slate for electrical purposes hurts the whole industry. I do not go so far as to say that you should take as a standard the slate shown by experience and test to have the greatest dielectric strength or the least loss from leakage, but determine what is a safe standard and see to it that all slate used for a given purpose comes up to that standard. **Mr. Fletcher**

MR. ROBERT NOTVEST.[2]—Notwithstanding the almost universal application of slate on industrial control apparatus and switchboard work in general, there is a deplorable lack of information in regard to the essential characteristics of this material and its peculiarities. Hardly any data can be found in textbooks on electrical engineering or the current trade literature. It is, therefore, a great satisfaction to have a meeting such as this where engineers representing the largest consumers of electrical slate have come together to discuss with the producers of this material questions and problems whose satisfactory solution will prove to be of mutual benefit. **Mr. Notvest**

Referring to the papers just presented, while I naturally may differ in some detail with some views presented in these papers, nevertheless, whatever criticism I have to make is inspired by constructive motives only, to promote an interchange of thought in order to arrive at a better understanding between consumer and producer.

Now in regard to the paper by Mr. Bowles: Mr. Bowles stated that it had recently been found that moisture in the form of quarry water will lower the electrical resistance, a higher test being obtained for the same material after it is seasoned. If you will turn to pages 19 and 20 of Chapter 12 on Electrical Slate which The Structural Slate Company publishes, you will find the data showing an increase of several hundred per cent in insulation resistance after the slate slabs were dried out.

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[1] H. E. Fletcher Co., West Chelsmford, Mass.

[2] Consulting Engineer, Structural Slate Co., Pen Argyl, Pa.

Mr. Notvest Mr. Bowles has been misinformed that there is no data available in regard to the resistance of slate against shock. On page 16 of Chapter 12 under the heading "Toughness," that is, resistance to fracture under impact, information is given pertaining to Pennsylvania slate.

In regard to the paper by Mr. Spurck of the General Electric Co.: Mr. Spurck, in accordance with good engineering practice, very correctly limits the application of slate to potentials not in excess of 440 volts for alternating current or 550 volts for direct current although if spacings are great enough and the capacity of the current-carrying metal parts are low enough in relation to the surface of the slate which they cover, 1200 volts can be applied on high-grade slate without excessive leakage.

Puncture voltage is an old obsession among electrical engineers and is disregarded more and more, especially since it has been found that there is no relation between the potential pressure required to mechanically penetrate or rupture an insulating material and its dielectric characteristics, that is, its insulation resistance and its impedance caused either by ionization or capacity condenser action. Mr. Spurck I believe will agree that there is no condition in switchboard practice or industrial control work where, through an inductive kick, sufficient voltage might be built up as to rupture the switchboard material. The only exception would be in the case of a circuit of a series or current transformer which might be opened through accident and then a potential up to 5000 volts will have to be dissipated. Fortunately, the time interval is so short (amounting to less than one hundredth of one second) and the density of the current is so low that it is not conceivable that any damage to the slate panel could be caused by such an occurrence.

In reference to the usual high-voltage test which Mr. Spurck discusses, I have on several occasions called the attention of the Standards Committee of the American Institute of Electrical Engineers that the provision of their standardization rules calling for the application of a certain high-voltage test on electrical apparatus and machinery is meaningless in so far as it does not contain a criterion as to what constitutes a dielectric breakdown or excessive sparking or heating but leaves it to the discretion of the operator performing the test.

The voltmeter differential method as developed by Mr. Spurck and Mr. Springer of the General Electric Co., actually measuring the impedance leakage, I consider to be highly accurate. A comparative calibration test made in Schenectady, under supervision of Mr. Springer, between their method and the one patented by The Struc-

Mr. Notvest tural Slate Company showed for all practical purposes sufficiently identical results and indications. The only reason why the General Electric method has not been utilized for the purpose of the slate quarries, outside of patent consideration, is that this method not only requires two operators, one to apply the contact pads, the other to keep the voltage constant by means of a variable rheostat and compute the potential difference indicated by the readings of the two voltmeters, but also because the apparatus as devised is costly, very sensitive, not portable, and unskilled labor as employed in the slate quarries cannot be depended upon to properly compute the indication of the two instruments.

In general, this method as developed by Mr. Spurck and Mr. Springer must be considered as one of the most valuable contributions in electrical science to ascertain the characteristics of a composite dielectric substance such as slate.

Another point brought out by Mr. Spurck is that the puncturing voltage is not affected by the thickness of the slate which corroborates my contention that dielectric rupture tests on slate are meaningless.

Very important also is Mr. Spurck's contention that the voltage required to puncture slate is a function of the time of application of such voltage.

I was very much interested to know that Mr. Spurck had come to the conclusion that baking of slate increases its hardness and renders it more difficult to drill; also, that the hardness will increase naturally as the slate is aged. This was confirmed by us through microscopic observation of the crystallin structure of Pennsylvania slate under a magnification of 1200 diameters which indicated that in freshly quarried slate, where the crystals still possess a molecule of H_2O, the axis of the flakes appear more unidirectional, whereas through the gradual drying out process, a change in the crystallin structure seems to take place and the flakes appear more criss-crossed like the fibers in a paper or felt.

I wish to congratulate both Mr. Springer and Mr. Spurck upon their very interesting and valuable contribution which they have given through their paper and I wish to take this opportunity to express on behalf of The Structural Slate Company and myself our sincere appreciation for their very generous assistance and cooperation in furnishing to us their data on all matters pertaining to problems connected with electrical slate.

Another very interesting contribution to the problem of the insulation qualities of slate is the paper by Mr. Harvey and Mr. Keyes.

There are only a few comments I have to make in regard to it.

Mr. Notvest I cannot agree that only Vermont and Monson slate had been used to any extent in the past for electrical purposes and that Pennsylvania slate had been used only in the past few years when an increased demand for slate forced the electrical industry to look for new sources of supply. I distinctly recollect that in 1910 the Walker Company in Philadelphia as well as the Cutter Company in South Bend used a considerable amount of very satisfactory Pennsylvania material.

Mention is made that slate should not contain laminations which would cause it to split or flake off while being drilled. This is almost entirely due to faulty manipulation of the drill press used. Either the drill itself is dull or the operator does not release the pressure of the feed handle when he feels the point of the tool breaking through.

We do not believe in a direct-current test because leakage in slate is an impedance effect. We know of a number of instances where slate showing excellent insulation resistance on 550 volts direct current might develop an appreciable impedance leakage on 440 volts of alternating current. For example, on page 6 of Chapter 12, we show the action of a test specimen under direct and alternating current.

The method of measuring the power consumption in the primary of a step-up transformer, as used by the Westinghouse Company is fairly accurate. The objections against this method from the standpoint of the slate quarry are not only the dangerous potential of 5000 volts, the large capacity of the transformer employed and the small range of the watt-meter indications in case of satisfactory material, but are mainly that unskilled labor has to use the outfit. Slate quarry operations are hazardous enough and fatal accidents occur with sufficient frequency without killing off labor as employed by us with a 6000 to 10,000-volt transformer.

Comparative data is given in regard to the factor of absorption between Vermont and Pennsylvania slate that is detrimental to the latter product. It is only fair to the producers of Pennsylvania slate to state that this data is not correct for material marketed through our organization. On page 13 of Chapter 12, you will find data in regard to the absorption of material as marketed by us, showing an average of 0.2 per cent and we can guarantee that commercial shipments will not deviate from this average by more than 10 per cent above or below. We also call particular attention to the fact that while the specimens in the Westinghouse test were only air dried, in our test made at Lehigh University very much more severe conditions were encountered in that the specimens were first baked at 120° C. for 24 hours before immersion in water for an equal period and the ascertaining of the increase in weight.

Mr. Notvest

As far as the megger is concerned, contact difficulties are so great and cause so many inaccuracies that the practical application of this instrument cannot be considered.

The slate industry appreciates the valuable work of Mr. Harvey and Mr. Keyes. No matter how much we may differ in detail, there never can be enough research and the paper contains new thoughts and outlines, new avenues of approach towards problems which urgently need solution.

Attention should also be called to an article by Mr. Miner, of the Westinghouse Company, appearing in the March, 1922, issue of the *Electrical Journal*, representing a large amount of original work.

In the past three years, The Structural Slate Company has made it a practice to investigate every instance of leakage trouble and wherever feasible to make a chemical analysis of such defective material. The moisture content of slate might cause considerable leakage. As an instance of this, we had returned to us a slab that had been worked into a 250-volt, 150-ampere, 20-circuit, 3-wire panel board and, upon completion, when tested in accordance with the Underwriters' requirements showed less than 50,000 ohms with all switches, fuses and cutouts in circuit. We had this panel returned to us and upon arrival, due to drying out, one side of the 3-wire panel tested 198,000 ohms, the other 119,000 ohms. Professor Fry, of Lehigh University, baked this panel at a temperature of 60° C. for a period of 72 hours and this increased the resistance to 1,280,000 and 1,020,000 ohms, respectively. You will also find that in Chapter 12 we have discussed this problem of moisture and give precautionary measures to prevent reabsorption of moisture, although this moisture problem is not as serious as we were at first led to assume.

For a time we attributed leakage to metallic veins, that is, strata of ferric and ferrous oxide parallel with the cleavage of the slate. But even in badly leaking slate, chemically analyzed, we found that the content of these metallic substances was the same or even below that of normal slate. We did find, however, a considerable increase of carbon in graphite form instead of the usual 0.5 per cent—sometimes as high as 2.5 to 3 per cent. All these various investigations have led us to believe that while there might be in slate under alternating-current potentials a slight hysteresis effect or even a very small leakage of the order of thousandths of milli-amperes due to the ionization of the voids between flakes, the main cause for the rapid decrease of insulation values under alternating current on slate is a capacity ohmic resistance condenser effect. Pennsylvania slate contains 1.3 per cent of ferric oxide and 2.2 per cent of ferrous oxide.

Mr. Notvest Each particle of these substances is capable of acting like a condenser, that is, assuming a capacity charge and these particles, interconnected with each other through an ohmic resistance of very high value—the 0.5 per cent of carbon in graphitic form. In other words, slate is not a homogeneous material, but must be considered as a composite dielectric—each one of the electrical inert constituents having different resistivities and different specific capacities. So leakage phenomena must be explained as actions of electrostatic charges built up in the interior of these various dielectrics and the boundaries between the molecules capable of assuming a capacity charge and the molecules acting as the insulation medium. This conclusion very satisfactorily explains not only the great discrepancy between the insulating characteristics of slate under direct current as against alternating current, but it also can be shown mathematically that the variation of either the metallic oxide content and thereby an increase of the capacity of the material or an increase of the carbon content and thereby a decrease in the ohmic resistance of the material will, as far as impedance effect under alternating current is concerned, closely correspond to the theoretical values as obtained for a circuit having a number of capacities and ohmic resistances in series. The problem of electric insulation is the most acute question confronting the electrical engineer to-day and I personally am convinced that additional research work on slate, representing such an interesting composite dielectric, will shed considerable light upon the subject.

Incidentally, Mr. Miner in his article attributes leakage in slate to ionization. No doubt the existence of voids and interstices containing carbonaceous matter might cause some ionization. The density of current, however, produced by this effect would not reach a value that could be measured with present-day instruments. As a matter of fact, the transient soaking current which is found charging the slate any time potential is applied to it is far greater. Mr. Miner also states that the losses in slate under alternating-current potentials show a remarkably uniform value and he has determined this loss at 25 cycles as varying as the 2.9 power of the voltage. There must be some error in this, as our experiments by no means show such a rapid increase in impedance effect. On page 6 of Chapter 12, it is shown that the increase in milli-amperes by no means approaches the value of the 2.9 power. In order to check this we had a test made at the Electrical Testing Laboratories, New York City, and submitted five pieces of slate about 8 in. square and 1 in. thick. The procedure was to place two brass studs $\frac{1}{4}$ in. in diameter in each piece, 2 in. apart; the incentus and the leakage current was measured with the milli-

Mr. Notvest

ammeter in series. The voltage was held about ten seconds at each step and raised to the next step in about ten seconds. On one sample a test was also made between two disk electrodes 2 in. in diameter placed on opposite faces of the sample so that the current flowed across the cleavage planes. Current was supplied by a 25-kilovolt-ampere transformer having a ratio of 22.7 to 1 and receiving energy from a 60-cycle 120-volt circuit. Voltage was measured by means of a voltmeter connected across the low-tension side in connection with the ratio of transformation. This was frequently checked with an electrostatic voltmeter placed directly across the high tension side.

The results are given in the following tables. Voltage values given are mean effective (square root of mean square) and are not maximum values.

LEAKAGE CURRENT, MILLIAMPERES, BETWEEN ½-IN. STUDS

VOLTS APPLIED	SAMPLE No. 1	SAMPLE No. 2	SAMPLE No. 3	SAMPLE No. 4	SAMPLE No. 5
500.....	7	6	5	5	7
1000.....	23 – 36[a]	22 – 30[a]	18 – 22[a]	20 – 22[a]	25 – 35[a]
1500.....	55 – 60	50 – 55	36 – 50	45 – 60	55 – 70
2000.....	100 – 135	80 – 120	60 – 90	90 – 140	100 – 150
2500.....	160 – 190	190 – 120	150 – 110	190 – 170	250

[a] The two figures give the current at the start and at the end of the ten second period.

LEAKAGE BETWEEN OPPOSITE FACES, 2-IN. DISK ELECTRODES, SAMPLE NO. 1

VOLTS APPLIED	CURRENT, MILLIAMPERES
500	..
1000	..
1500	2
2000	4
2500	10

It is readily seen that this data does not correspond with the findings of Mr. Miner. His value of increase of impedance leakage in accordance with the 2.9 power of the voltage might be correct for some slates coming from other districts.

The reasons for the peculiar design of our patented test set were as follows:

1. The outfit had to be portable.
2. It had to be direct reading so as to enable unskilled labor to make measurements therewith.
3. The indications of the instrument had to be very sensitive and give a criterion on excellent material and still plainly show up any slate which might develop leakage.

Mr. Notvest 4. The capacity of the outfit had to be held to such a limit as to preclude any fatality in case of accidental contact under short-circuit conditions.

The organization of 22 quarries, represented by The Structural Slate Company, is spending this year a considerable amount of money in additional research. In our laboratory, we are installing a high-voltage transformer where we can get steps up to ten thousand volts. We are developing a new instrument to more accurately measure the hardness, that is, the workability of slate which is a combination of the Brinell and scleroscopic method. We have ordered from Leitz in Germany a special petrographic microscope to enable us to take photographs of the structure of slate under a magnification of 1500 diameters. We are now conducting a series of experiments in order to develop a more efficient drill to bore slate, since we are convinced that especially our own Pennsylvania product does not contain sufficient abrasive ingredients to satisfactorily explain the rapid wear of the cutting edges of the tools. Our experiments, so far, have proved to us that this rapid wear is simply a temperature problem, for slate dust has such low thermal conductivity, that the energy expended by the drilling tool is not distributed over a large area for dissipation, but is held closely confined to the cutting edge of the tool. Improvements will lie in two directions: one in the development of a specially shaped drill, more easily expelling the dust, and the other in a special alloy capable of withstanding a higher temperature without losing its temper. We hope to be able to report more in regard to this in the next few months.

In general it has been the policy of The Structural Slate Company to approach the problem of electrical slate solely from the viewpoint of the use of slate. We shall be glad if the results of our research will benefit not only the consumers but also other slate producers, for we are convinced that raising the standard of quality of our own product will bring about higher standards throughout the entire industry and will in this way cause a much greater use and application of electrical slate and make this material available for new and untried purposes. The slate industry in general has been indifferent in the past, to the problems of the electrical engineer and designer, of having operated under the medieval principle of *caveat emptor*, of having shipped indiscriminately as electrical slate any material which looked like clear slate. The slate industry is responsible for the use of a number of substitutes for slate. The past ten years represent a constant search on the part of the electrical engineers to find a satisfactory substitute for slate, not because this country does not produce

Mr. Notvest

a sufficiently high quality material, but because the slate producers were not perspicacious enough to select good slate from among the various beds. I am glad to say that the organization with which I am associated has recognized the necessity for constant research and close study of the problems of the user of electrical slate to enable him, through cooperation, to secure better methods of working the material and to utilize it to best advantage.

In conclusion, on behalf of a majority of Pennsylvania producers, organized in The Structural Slate Company, I would ask for cooperation, for an interchange of thought. We request to have the benefit of the views and experience of others, so that we may furnish, through proper selection and test, slate which consumers will be glad to use on their apparatus.

Mr. Spurck

MR. R. M. SPURCK.—In general, the points brought out in Mr. Notvest's discussion of my paper agree with my experience. There are, however, some features that should perhaps be amplified. The American Institute of Electrical Engineers' dielectric test for switchboards is a moderate test of a value found by experience to be desirable for such apparatus. It serves a very useful purpose and is not too severe a test to apply to slate of the quality desirable for switchboard apparatus.

In the factory utilization of the method of test described in my paper, a definite minimum voltmeter reading of voltmeter V_2 is specified. It is therefore unnecessary for the tester to make calculations. I see no reason why the apparatus could not be arranged to be manipulated by one operator.

Mr. Harvey

MR. DEAN HARVEY (*closure by letter*).—Referring to the discussion by Mr. Notvest, our experience has been that there is a considerable variation in the quality of slate as regards laminations tending to cause it to flake off while being drilled, even though much care is taken in the drilling operation.

Mr. Notvest stated that the specimens for the moisture absorption test mentioned in the paper were only air dried, whereas his specimens were baked at 120° C. for 24 hours. Our specimens were dried in an oven until constant weight was obtained, indicating that they were absolutely dry. He does not state whether his specimens were dried to constant weight. Tests have shown that slate will sometimes require drying for more than 24 hours at 120° C. to remove all moisture.

It is unfortunate that the moisture absorption test report by Mr. Notvest cannot be compared with the tests mentioned in the paper, as the samples were not immersed for the same length of time.

TOPICAL DISCUSSION ON THE TESTING OF GLUE

INTRODUCTION

By Jerome Alexander[1]

Although there were in Rome many beautiful temples dedicated to various divinities, the Romans, fearful that they might have inadvertently neglected some god and thus incurred his displeasure, erected the magnificent Pantheon, which, as its name indicates, was dedicated to *all* the gods. In like manner, we have dedicated this meeting to every one who is interested in Glue, with the hope that all will appear, state their views, and forever after hold their peace; for the attempt will be made to consider the subject from divergent, and what have been in the past erroneously considered conflicting points of view.

This is therefore no *ex parte* proceeding, but an action *in rem*, in which all parties at interest are cited to appear, give their attention, and be heard. I refrain from calling the meeting a "pow-wow" simply because the Standard Dictionary gives the following definition for the colloquial meaning of this word: "any meeting for conference, especially when noisy, confused, and without result." I hope that we will transcend that limitation.

The discovery of glue is lost in the mists of antiquity. Its use was well known to the ancient Egyptians, who, as we are now learning, were not quite so ancient as this appellation indicates. On the walls of the tomb of Rekhmara at Thebes, dating from the time of the Exodus, there is a sculpture showing workmen using glue in veneering. Probably some one had observed that a boiled-down stew containing bones and skin is sticky, and when cool gives a jelly which could be allowed to dry out and be subsequently redissolved for use as an adhesive. In common with most other arts, the method of making glue was handed down from father to son, and was probably confined to those who afterwards made use of the product. If the glue was bad, the consumer knew who to blame. References to glue occur throughout literature, but there seems to be no detailed description of its manufacture or any realization of the fact that there are very wide differences in the quality of glues, although one Roman writer speaks of the hides of bulls as yielding strong glue. Even to this day the average person has no conception of the tremendous extremes in quality shown by commercial glues and gelatins.

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[1] Consulting Chemist and Chemical Engineer, New York City.

With the development of the glue industry and the competition for business which naturally arose among the various manufacturers, three aspects of the glue question spontaneously appeared. First, the manufacturers tried to improve their products and then to convince the consumer that they had done so. Second, the consumer, facing conflicting claims of glue manufacturers, made tests to find out the truth and to check up the tests which the glue man relied upon to demonstrate his improvement. Third (and this is only a very recent though perhaps a very valuable addition), both the glue manufacturer and the consumer began to call upon the chemist and the engineer for help in improving the product, the methods of using and testing it, and for arbitration in the case of honest dispute. The subject quite naturally divides itself, therefore, into testing as viewed by (1) the manufacturer, (2) the user, and (3) the professional chemist or engineer. If all these men work merely to discover the truth, there can never be any conflict between them, unless it be the laudable effort for giving the best service.

Let us consider the question as it presents itself to each of these groups. If any manufacturer wants to hold the trade of any consumer, he must first make a study of the requirements of that consumer, find the grade of glue best suited to those requirements, and then supply that grade in uniform quality and at a reasonable price. Above all, *uniformity* is essential, for any competent user weighs his glue and measures the water in which he dissolves it, so that any variation of quality at once becomes manifest. Contrary to the idea which some state directly and others inferentially or by innuendo, the personal interest of the glue manufacturer is to give his customer uniform, satisfactory products, and from a personal acquaintance with manufacturers extending over twenty-five years I can state that they have always tried to do so. The same also applies to the responsible dealers in glue. Barring errors, the troubles encountered in using glue may generally be traced to an inadequate conception of the requirements by the seller, or to improper handling of the glue by the consumer. Owing to the enormous diversity of uses to which glue is put, and the powerful influence that may be exerted by traces of certain constituents like grease, soluble salts, and the like, the seller must be very cautious in making any change, even if he thinks it is an improvement. Too high a grade of glue may cause trouble, as well as too low a grade, and a glue with a relatively high viscosity may make difficulty in some cases, although in other cases it would be preferred. Each glue seller has in the course of years, and often at great expense, learned just what characteristics to select and to avoid for each customer; for even in the same industry differences of working formulas and of methods necessitate different kinds of glue.

You can well understand, therefore, how the glue seller feels when some one unfamiliar with these details attempts to fix one or even a few "tests" which are supposed, at one fell swoop, to give all the information he knows that he must have. The glue seller is an eclectic. He chooses tests from all sources which will help him make proper distribution of his product, and as a result he has in the course of years accumulated a series of tests which experience has shown him to be useful. He does not sit back with smug self-satisfaction, but, as his representatives here will show you, is seeking to improve old tests, to find new ones, and to learn from others. The glue seller, confronted with a product that must be made in batches which show wide differences in grade and properties, keeps each lot separate, finds its characteristics, and then apportions it to its appropriate use, if necessary mixing several lots to obtain the desired quality and quantity.

So great is the confidence of most consumers in the honor and the ability of the manufacturer and seller, that most of them simply order glue "as last had," and use the new lot like its predecessors; and this confidence is never intentionally violated by reputable sellers. Still, since as a matter of law the buyer can hold the seller only for differences in value between goods delivered and goods paid for, and not for the much more important consequential damages which may follow the use of a wrong grade of glue, many large consumers take the very wise precaution of testing carefully each delivery. In addition, a buyer's test will enable him to select from a large offering, the most promising for practical trial in the works. Besides all this, research into the nature of glue and gelatin, and their use in his particular process or work, will often show a consumer ways of improving his product, or cutting down its cost, or both.

You will hear from consumers who have led in such research, as well as from those who have questions to ask and problems to pose. Contact of this kind is most valuable, since all are working to one common end.

The professional chemist and engineer serves both the buyer and the seller, as well as the user, and is apt to be called to aid any of them in manufacture, testing or using glue and gelatin, or in finding new uses or methods of using them. His interest is that of his client or employer.

We now come to a consideration of the details of testing. It is not my intention to enter at present into any views of my own, but rather to outline a number of points to be thrown open to consideration and discussion. Most differences of opinion about testing glue have arisen with reference to three tests: jelly strength, viscosity, and strength.

The jelly strength or consistency test, proposed by Lipowitz in 1861, needs to be standardized with respect to apparatus, and the preparation, concentration, and temperature of treatment and test of the sample. In dealing with colloids any variation may have an effect, so that all variables should be agreed upon. As a preliminary, one should first consider just how a sample for test should be taken; otherwise all one's other precautions may be set at naught.

The viscosity or running test used by Stelling and by Fels should be standardized with respect to apparatus, and the concentration, preparation, and testing temperature of the sample. Here, as in the preceding case, we should remember that differences in time and temperature of preliminary soaking and heating, may have an influence on results.

Since the joint test runs parallel with the viscosity - jelly strength figures, for the same type of glue at least, its difficulty and uncertainty will bar it from use as an ordinary routine test. Still, it will be demanded by some as a check on other indirect tests. For the busy man, a proper consideration of the viscosity - jelly strength figures will probably continue, as in the past, to be the basis of his judgment as to the strength of a glue, and can be superseded only by a test under actual working conditions.

Other questions suggested by those interested are:

1. Shall we have a water absorption test, and if so what shall it be?
2. What does foam signify and how shall a foam test be made?
3. What chemical tests are necessary, and how are they to be made?
4. Why does glue stick, and how shall we measure tackiness prior to setting?
5. Is it practical to measure the strength of a dried glue film?

These and any others that suggest themselves to you are now open for discussion.

GENERAL DISCUSSION
ON
THE TESTING OF GLUE

Mr. Williams

MR. F. S. WILLIAMS.[1]—My remarks to-night will be almost wholly in reference to the work that has been done in attempting to classify glues according to their properties. The manufacturers of glues have had an association for their mutual benefit for probably 20 or 25 years. There was a time in the history of this association when they were more or less selfish, isolated in their interests, as they supposed, and there was very little effort made toward any uniform system of classifying or grading their products; but as the years passed, it became more and more evident that something must be done. About eight or nine years ago, a real "honest-to-goodness" effort was made to devise a system of testing glues that would really tell something. Several attempts were made that were more or less failures; in fact everything that has been done up to date has been unsatisfactory in the completeness of the results that could be obtained. Some three years ago another, and up to this time, last attempt was made to get something workable for the glue manufacturer, the glue user and the glue dealer—for everyone in fact who was in any way interested in the properties of glues. Committees have been appointed to work out different aspects of this problem. The leading chemists of the leading glue manufacturers of America have been working on this problem now for about two years, and they are making progress and it is believed that they see daylight ahead. I believe I can now confidently say that before the expiration of this year, we will have formulated and published methods by which the glue chemist can determine and express in definite terms the relative values of different glues. Two of the gentlemen are present who have done quite a deal of work on this subject. While I happen to be chairman of the Committee of the association on Standardization, the real work, the laboratory work, has been done very largely, in fact chiefly, by others, and the two gentlemen who are here, I would be very glad to have explain to you what work has been done, what we hope to do and how far along we have gotten toward that result. Mr. J. R. Powell, chief chemist of Armour and Company, and Dr. W. D.

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[1] Baugh and Sons Co., Philadelphia, Pa.

Richardson, chief chemist of Swift and Company, are present, and I would be very glad to have the chairman call on these gentlemen to give you an outline of the work the association is doing, and hopes to complete in the near future. **Mr. Williams**

Mr. Richardson MR. W. D. RICHARDSON.[1]—Like Mr. Williams, all I can say concerns the work which the Glue and Gelatine Association has been conducting with reference to standardizing methods of testing. There is of course no question about the desirability of standard methods in this field as in any other field where we are dealing with materials; and why this particular work has been delayed until the present time is difficult to say. I think we have all realized the necessity for a long time, and some of us have made attempts to develop methods, not recently but a long time ago; I made such an attempt about 1909, when I asked certain chemists in the glue business if they would not consent to become members of a committee associated with some society whose purpose it would be to develop standardized methods for testing glue. There was very little response at that time; apparently the time was not yet ripe; but as a matter of fact, as a matter of history, it was the Glue Association that first took up the work actively, as Mr. Williams has told you. My connection with the work has been mainly from the side lines; I have not been an active participant in the sense of doing the laboratory work, but I have been interested in it and have been associated with it. I became actively connected with the work in this sense about a year ago last January when the committee, consisting of chemists and physicists connected with the glue industry, met in New York City. At that time no standard methods had been developed. I was not an appointed member of the committee; I was merely an observer and associate; but what I asked the committee to do at that time was to concentrate on at least two tests and develop standardized instruments for these two tests, namely, jelly strength, which is commonly considered in the trade and by users to be an important fundamental property of glues, and viscosity, which is also a test commonly applied. When I first proposed the standardization of instruments for these tests, the gentlemen on the committee who had had experience with the matter (mostly in the way of failures) rather scouted the idea that it could be done, but I requested that an active committee of three be appointed to make the attempt, at any rate, and offered the cooperation of our laboratory. I wanted to see if we could not develop two instruments, one for each purpose, which would be considered standard, which would be reproducible

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[1] Chief Chemist, Swift and Co., Chicago, Ill.

Mr. Richardson in the ordinary units of measure and with the object of being able to reproduce tests at any time and any place whether the instruments were lost or not. This committee as appointed consisted of Dr. De Benkelaer of our laboratory, chairman, Mr. Powell, of the Armour Glue Works, and Mr. Bahlmann of the U. S. Glue Company; and the work became divided into two parts, one on the gelometer and the other on the viscosimeter. Now I gave the job of making the gelometer to one of my men, Mr. Bloom. I told him what I wanted, which was an instrument with a plunger of definite diameter which would press down upon the jelly to a definite depth, which depth could be accurately measured, the load being applied within a specified time. Having given him those specifications, I left him alone, which is a very good way to do at times, and he worked the machine out. The plunger is $\frac{1}{2}$ in. (12.7 mm.) in diameter. The depth of the plunge is 4 mm., and the time of application of the load, consisting of shot, is about from 2 to 5 seconds for ordinary tests. These instruments are made and are now being sent out to the various members of the association. The instrument is reproducible and can be made by any instrument maker with ordinary skill, and the results are comparable, provided, of course, that the jellies are made up in comparable ways; and in this connection the temperature of the jelly and its history from the time of putting the glue into solution are the important factors. The temperature in particular must be controlled very closely. The instrument is too sensitive for the present methods of preparation of the jelly, and the matter of controlling the temperature at which the jelly strength is made is now under consideration by the committee and has been developed to a great degree of accuracy, the two apparatus used being a chill bath, which Mr. Williams has developed, and a chill room which is kept accurately at a certain temperature, the usual temperature being 10° C. In the chill room is placed a trough of water whose temperature is still more accurately maintained. The room may vary from 0.5 to 1° C., about a central point; the chill bath itself will not vary more than 0.1° C. In order to get the most accurate results, it is necessary to keep closely to these limits.

In regard to the viscosity pipette, we tried in our laboratory at first to make a reproducible pipette. This is an attempt which has been made many times, always with failure, and our results produced nothing but failures for a number of trials. I think we should have succeeded in the long run, but about that time Mr. Powell came forward with a method which has since been adopted and which is simpler and better, and the attempts to make a reproducible pipette were given up.

Mr. Richardson

There is another committee working on the other tests and standardizing them. Mr. Underwood is chairman of that committee, and I understand his committee is taking up such matters as melting point, foam test, grease, p.H. value, ash, chlorides and hydroscopic properties and matters of that sort.

In any event, this work has reached a point at which the ordinary tests are reasonably well standardized, and as Mr. Williams has told you, probably within the current year these methods will be available.

Mr. Powell

MR. J. R. POWELL.[1]—Of course, it is logical that there should be some standardized methods of comparing glues; what basis they should be compared on, of course, is an open question. We do not know yet whether jell and viscosity determinations are going to give what we want to know, but these have been the accepted tests for years and probably will be for years to come, so I think that it is very desirable that they should be standardized as best they can be, so that we will have some basis on which to start comparing results.

Another point is the desirability of getting some information that will assist in preparing glue specifications. I do not think there are any glue manufacturers who do not run into specifications that mean absolutely nothing to them and less to the buyer. I think one of the Government departments comes around periodically for glue made from horses' hoofs, and horses' hoofs do not happen to yield glue. The standardization of glue testing will permit the writing of specifications in terms intelligible to all.

The viscosimeter developed is nothing more than a glass pipette very similar to several that are now in use. We took several pipettes of radically different design and found they could all be standardized to give results within the accuracy required, that is, the accuracy within which the glue solution could be prepared. The method of standardization is comparatively simple. The time of discharge for two liquids of known viscosity was determined on each pipette. We happened to use 25 per cent glycerin for one of these and paraffin oil standardized by the U. S. Bureau of Standards for the other. From these data a table may be calculated or a curve plotted for each pipette from which the viscosity of any liquid in absolute units may be obtained after having ascertained its time of outflow. When dealing with glue solutions there may be a question whether we are determining viscosity or apparent viscosity, but the term has been used and will be for some time, so until we find something better we had better stick to it. Pipettes with the data furnished with each can be used for determining the absolute viscosity of other solutions besides glue.

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[1] Chief Chemist, Armour Glue Works, Chicago, Ill.

Mr. Powell If these pictures (pictures shown) are of interest, they will give the idea of the pipette. We do not claim it is an absolutely scientific instrument, there probably can be some errors found, but it is accurate within the limits required for glue solutions. Different laboratories can get check results on the same glue, something never done before.

Mr. Bogue MR. ROBERT H. BOGUE.[1]—If we went to a shop to buy glue and asked only for glue, we would be apt to get almost anything. We all know that there are many kinds and grades of glue, with a few of which we may be familiar. There is bone glue, hide glue and fleshing glue, colored glue and clear glue, strong glue and weak glue, and we may obtain it in different forms, such as in sheet form, or flake, or ground, or pulverized.

All these facts are, in general, known to us, but we cannot with accuracy make any more detailed specifications. If you are a furniture manufacturer or a man who is going to use glue in large quantity, you go to a glue manufacturer and ask him for some product that will be satisfactory for the specific uses to which you wish to put it. You are required to accept his judgment on the product, because your own information is inadequate. You cannot give him specifications; he will give you what he thinks is best suited for your purpose. I heartily grant Mr. Alexander's contention that glue manufacturers intend to be honest; that in pursuance of their business it is necessary that they be honest, but that is not sufficient, for glue sales managers, and particularly the salesmen whom they send out, are not always competent engineers or chemists who understand everything there is to be understood about glue. They cannot in all cases state definitely that a given glue will fulfill a given service to the best advantage.

Now any system that makes it possible, or rather that makes it imperative for a consumer to accept the judgment of a manufacturer through his salesmen, granting that the manufacturer and the salesmen are honest, is not a system that we, as scientists, can accept. It is indeed a challenge to our professional competency. We should, if possible, discover some basis upon which we may make definite specifications of the product, whereby a purchaser is enabled to state his wants with exactness, and then, after a purchase, may see to it that he gets exactly that which he has ordered. I think that such specifications should be possible eventually; they are not yet possible, but I am convinced that they may be if the subject is studied exhaustively.

That is the situation which confronts us, and our problem is:

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[1] Lafayette College, Easton, Pa.

Mr. Bogue

What are we going to do about it? I think the keynote of our policy should be cooperation. Mr. Williams, Mr. Richardson and Mr. Powell have all spoken of the work that has been done by the Glue Manufacturers' Association. I am heartily in sympathy with and in favor of the continuance of that work, but that is only one part of a program which I think should be much larger and more comprehensive. As has been said, they had to begin somewhere, and of course that was the logical place for a committee to begin, namely, upon the improvement of apparatus for the measurement of jelly strength and viscosity. But it is necessary for us to remember that we do not yet know positively that the thing we are after in glue is defined by or is parallel to jelly strength; we do not know that the thing we are after is parallel to or is defined by viscosity. These are the two measurements that are used and have been used for a long time, and, as Mr. Powell says, will probably continue to be used; but we should find out, we should make a study to ascertain definitely if these are the things that are parallel to the fundamental properties we wish to measure.

I do not know whether there is one fundamental property of glue or not. I am sure that most of the gentlemen who have spoken, especially Mr. Alexander, would say, "absolutely no, there is no one property." Perhaps there is not; there has been some work done, however, and I have reported on previous occasions work which has shown that the content of gelatine and gelatose in any glue is proportional to the adhesive strength of that glue. That is one step in advance, for glue is used very largely as an adhesive. It may be, therefore, that the gelatine content or gelatose content or a combination of the two, is the actual thing, the fundamental thing, that we are after in the evaluation of glue. I do not say that it is, but it looks as if it might be so. If it is possible to get at one property that we can say is the thing which we wish to have manifested in a glue, the fundamental thing, we should by all means do it. If it is not possible, then by research we should find that out definitely. For example, in sulfuric acid we are interested in the actual percentage of H_2SO_4 present, but we do not usually actually measure the content of that substance. Instead, we measure the density, because we know that the density parallels exactly the acid concentration. In the case of a syrup, we wish to know the sugar content. We do not make a chemical analysis to find out, but we measure the optical rotation, because we know that the optical rotation parallels exactly the sugar content. So with glue; it is not practicable in the plant laboratory actually to determine the content of protein and proteose, even

Mr. Bogue provided we have found this to be the basic thing we are after. Instead, we want some means for measuring that property which will result in a parallel evaluation, but which will more readily adapt itself to the usual laboratory methodology.

Our first task should be to find out definitely, once for all, if jelly strength and viscosity are properties which are actually parallel to the fundamental thing we are after in a glue. If they are, fine; if not, we should not hesitate to discard them. We must then find some property that does evaluate, and use that. In some work which I have reported, I found that jelly strength alone is insufficient; I found that viscosity at 60° C. alone is insufficient. The melting point, or the viscosity at 35° C., was found to evaluate more accurately than either of these two alone. It may be that this would be a better test to use; let us find out. You may know that the jelly strength of a glue is usually measured at about 10° C., and if you take two glues (two glues can easily be selected that will exhibit this particular property) one of these glues may have a stronger jelly than the other at 10° C., and the jelly consistency may be reversed in these same two glues, at the same concentration, just by warming them up to 15° C. Is there a fundamental reason why 10° should be selected as the temperature of test instead of 15°? Perhaps there is. If there is, let us find it out.

I am pointing out that there is a great deal of work to be done. I have shown, first, that we should find a fundamental property, if possible; second, that we should study the means by which these properties which we are after may be most satisfactorily measured; third, that the technique of measurement should be standardized, as is being undertaken by the Glue Association at the present time.

A fourth subject that should receive attention is the system for the specification of grades. That is a subject that has been more or less avoided. The glue manufacturers do not differ from manufacturers of other commodities in the reticence with which they acknowledge the advantages that would accrue to them by cooperation with the consumer to such an extent that the consumer may duplicate his tests and buy on specification. Mr. J. A. Wilson, of the Gallan Tannery Co. at Milwaukee, was telling me the other day that in the leather industry, as he knew it, everything that could be done to improve leather by one manufacturer would, if used generally, increase the sale of leather for all of them. In other words, it would be a distinct disadvantage to any one manufacturer of leather if, on finding something which would improve his product, he kept it to himself; it would be a distinct advantage in actual

Mr. Bogue

sales to him, as well as to his competitors, if he told them all about it. Exactly that same thing was told me with respect to candy manufacture; improvements in the manufacture of candy made accessible to all producers increased the sales of all of them, not just one. The glue manufacturers, I say, are not different from others in the slowness with which they are acknowledging that if they can cooperate with the consumer so that the latter may order on definite specification of product, so that on receiving the material he may apply these tests and know with exactness how his specifications have been met, then it will be to the material advantage of the manufacturer to do so. In the actual designation of grade, a distinct step in advance has been taken in the work which Mr. Powell for the Glue Manufacturers' Association has done with viscosity, because he has recommended, as I have urged in several published papers, that viscosity be expressed in poises or centipoises which is a standard unit. Actual grades of glue on the market are expressed in about a hundred different ways, as X 3, BB, $1\frac{1}{4}$, and all the rest. They do not mean a thing to anybody unless he happens to have been initiated into that particular code. A salesman told me recently that he himself did not know what they meant, how they were obtained, nor for what they stood, except that a certain symbol designated the best glue, so he was told by the home office, to use for a certain purpose. So I want to urge this in addition to my first three recommendations: that when any committee shall be appointed, and I wish that it may be so appointed to consider all of these things in their broadest aspect, they do not neglect to consider a simplification and standardization in the designation of grade.

Mr. Kelley

MR. G. L. KELLEY.[1]—You have so far heard from the producers of glue, and I am speaking on the case of the consumer. I have not been interested in glue very long, but I was astonished some time ago to find that it was difficult to buy glue on a genuine specification. Glues were recommended for certain purposes by each of the salesmen who came to us. Our success in using the product would depend of course upon the skill of the doctor as represented by the salesmen. It has seemed to me that is a condition we should overcome, and I hope that this meeting may help to that end. One thing I think we ought to bear in mind is to try to make any tests which we do devise ultimately for such purpose, so simple if possible, that they can be carried out without the use of too elaborate apparatus. One of the speakers mentioned the use of a cold room. Even the largest consumers of glue are generally using glue only as

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[1] Chemist and Metallurgist, Edward G. Budd Manufacturing Co., Philadelphia, Pa.

Mr. Kelley incident to some other business, and therefore in many instances it will be undesirable to install such extensive equipment as might be represented by a cold room. For that reason, I urge upon those of you who are working on such problems, that you try to devise means which the consumer can conveniently employ.

The literature of glue testing is not very extensive. I cannot pretend to have an extensive acquaintance even with such as has appeared. Mr. Bogue's writings are perhaps the most extensive I know of, and I have read those somewhat. I was interested in a conclusion which he apparently reached, and that is that the use of the MacMichael viscosimeter at various temperatures would give at least as fair a correspondence between tests for glue as can be obtained by other means, such as the jelly test, viscosimeter test and melting point. He pointed out at the same time that he found other viscosimeters not so satisfactory and that the use of these was characterized by error. Not dismayed by that, I tried to make use of a simple instrument which almost every laboratory has in hand, a Saybolt instrument. We tried that at 140° F., 100° F. and 85° F. I picked out 140° F. because that is very extensively used for determining the viscosity of glues. One of the glue manufacturers kindly supplied me with a set of samples which were marked with these mysterious grade symbols. Applying this crude method of test, I found that these glue samples strangely fell into exactly the same classification, even though the man who did the work did not know what the symbols meant or what the proper order of classification was. In carrying out this work, we used 5, 10, 15, 20 and 25-g. quantities with 200 g. of water. Of course with the lower concentrations we got very indefinite results, and with the higher concentrations we were not able to work with the better grades of glue at the lower temperatures. We finally settled for most of the grades of glue upon a concentration represented by 20 g. of glue and 200 g. of water. By means of that we were able to draw curves which were quite different for each grade of glue. Now, although I realize that I have established a bad practice in devising a new standard of my own, we have since been testing glue which we have received from various manufacturers by this method. I have been interested to note that certain manufacturers who have undertaken to supply us a grade of glue have supplied us with grades that corresponded almost exactly in test in lot after lot over a considerable period of time. I would hesitate to recommend this method of test, especially to those who have had more experience in testing glue than I have, but I hope that anyone who goes into it will give consideration to such a simple test.

Mr. Kelley May it not be that the difficulty in devising specifications and tests for glue is partly resident in the fact that we are trying to write a specification which covers glue for all purposes? In the case of many materials which are sold on specification, no attempt is made to draw one specification to cover all materials. Steel is my own line; not all steel is sold under one specification; we have steel for this purpose and steel for that, steel for structural purposes, steel for tool purposes, and we have different specifications for each. May it not be that glue to be used for the building up, say, of abrasive wheels, or glue to be used in furniture manufacturing requires different specifications? I am hardly able to suggest how they might be classified, but might they not be sub-divided and comparatively simple specifications drawn for each group? I know it is quite possible that traces of grease which would seriously interfere with some lines of work would have no effect upon others. That is the suggestion which I want to leave with you, that possibly you can sub-divide the applications of glue and so arrive at a better classification of glues.

Mr. Veitch MR. F. P. VEITCH.[1]—I have a problem in the use of glue that I would like to have some light on. It is very well known that glue is extensively used in the interior coating of barrels and drums containing oils or liquids of various kinds. The particular thing I am interested in is the gluing of the interior of turpentine barrels and drums. Now glue used for this purpose is not bought on any specification or attempted specification or even anything like a specification. It varies from probably the highest to the lowest grades—chiefly the lowest grades, I should say. What I would like to know from those who know something about glue, is what grades of glue are best; what characteristics should the glue have for this purpose? Have you anything concerning the application of glue? And finally, what simple test can be used by the purchaser to determine the suitability of this glue or of deliveries of glue for his purposes? That is a pretty large order, but I would really like to have some information on it.

Mr. Jones MR. WILBUR L. JONES[2] (*presented in written form*).—Of all the methods that have been devised for the testing of glues, the breaking of a glued joint has generally been considered, by woodworkers at least, to be the most practical and the most significant. Woodworkers who have never heard of viscosity or jelly strength usually make what is called a chisel and hammer test. They do this by gluing two pieces of wood together and then, after the joint is dry, inserting the point

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[1] Chemist in Charge, Leather and Paper Laboratory, U. S. Bureau of Chemistry, Washington, D. C.

[2] Forest Products Laboratory, Madison, Wis.

Mr. Jones of a chisel at the glue line and driving it in. The joint breaks apart, of course, and the failure takes place in the wood, or glue, or both. If it takes place in the glue, it is considered an indication that the glue is too weak; if it takes place in the wood, that the glue is strong enough. It is a crude test obviously, and open to criticism on many grounds; nevertheless, it is almost impossible to convince the user of the chisel and hammer test that a glue that allows the joint to separate without pulling any wood fiber may possibly be strong enough, and conversely it is hard to undermine the confidence that a large amount of wood failure in the break inspires.

Various refinements of the primitive chisel and hammer test have been worked out from time to time. Karmarsch,[1] who worked out the Spandau test, Bauschinger,[1] Rudeloff,[2] Kahrs,[2] Gill,[2] Welmers,[2] and others have developed various kinds of strength tests. The most recent of these is the shear block test devised by the Forest Products Laboratory, and described in various publications.[3] In this test a measured shearing force is brought to bear on the plane of glue between the wooden members of a hard-maple joint.

It was realized when this test was introduced that it was by no means a rigorous and perfectly satisfactory test. Nevertheless, for the lack of a better joint test it was used regularly at the laboratory and eventually incorporated in the specification for glue for airplane propellers for the purpose of supplementing the customary physical tests such as viscosity and jelly strength. It was included because need was felt for concrete evidence that the glue under examination would give a strong joint in wood. The shear test had not been in use long, however, before serious doubts about its value began to crop up. It gave results that were erratic and difficult to understand. Some high-grade glues appeared to have a low shearing strength, and some low-grade glues a high shearing strength. The strength of any single glue was likely to vary all over the range, and results could not be duplicated. Instead of confirming and substantiating the results of the other tests, it contradicted them almost as often as not.

These peculiarities of the shear block test led us in the course of time to make a careful study of the gluing process itself. This study proved that in the gluing of wood with animal glue there are a great many factors at work, all influencing the strength of the joint to a greater or lesser degree, some working independently, others conjointly. Some of the factors are controllable, others are not. It is perfectly clear, though, that the grade of the glue is one of the least influential of all. Almost anything mentioned has a more important **Mr. Jones** effect on the strength of the joint than the grade of the glue. When with a slight and almost imperceptible change in technique one may use a low-grade glue so as to obtain strong joints, and when some obscure factor, hidden or beyond control, may cause a high-grade glue to give weak joints, it is perfectly obvious that the joint test is not suitable for use in the grading of animal glue. Furthermore, when one appreciates the powerful effect of such factors as the amount of spread, direction of grain, relative amount of springwood and summerwood—factors that it is difficult, if not impossible, to control—one realizes that uniform conditions, the *sine qua non* of any test method, are absolutely unattainable in the joint test. The results of the joint test are illusionary and without significance so far as the ultimate strength of the glue is concerned.

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[1] Rideal, "Glue and Glue Testing," pp. 160–161.

[2] Bogue, "The Chemistry and Technology of Gelatin and Glue," pp. 527, 528, 529.

[3] Bogue, *loc. cit.*, and National Advisory Committee for Aeronautics, *Report 66* (1920), p. 21.

It is impossible to differentiate between high-grade glues by means of the shear test on account of the comparative weakness of the wood. Furthermore, it is impossible to correlate shear strength with viscosity and jelly strength, because low-test glues often give surprisingly good mechanical strength. The thing that is most important is to put and maintain a sufficient quantity of not-too-much hydrolyzed gelatin in the joint. The quantity thus kept and maintained will depend on its viscosity, all other things being equal, and the rate at which it thickens, solidifies, and takes hold on the wood will depend on its jelly melting point, which in turn is closely connected with its jelly strength. Thus according to the new view, the ability of a glue to cover wood and maintain a film in a joint against adverse conditions is indicated by the same things that have always been considered to indicate the degree of hydrolysis or the grade of the glue, namely, viscosity, jelly melting point, and jelly strength. In other words tests for viscosity, jelly melting point, jelly strength, foam, grease, hydrogen ion concentration and keeping qualities tell us all we need to know about an animal glue for woodworking purposes.

If it is felt some other guarantee is needed, the only recourse is to develop some sort of a film test, following the outcome of the suggestions made by Bateman and Towne in a recent article.[1] This article does not describe a perfected and practical method, but it does show how the tensile strength of strips of glue can be measured in a slow and rather limited way. It is almost certain, though, that there is a greater likelihood of obtaining information of value about

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[1] Bateman and Towne, "The Hygroscopicity of Hide Glues and the Relation of Tensile Strength of Glue to its Moisture Content," *Journal Industrial and Engineering Chemistry*, Vol. 15, pp. 371–375 (1923).

Mr. Jones animal glue by attempting to establish a film test than by continuing to use a method that is unworkable and untrustworthy.

Mr. Alexander MR. JEROME ALEXANDER.—I should like to say a few words because in my opening remarks I failed to go into any detail at all; I wanted all the others to have their say first. For those who have very little familiarity with glue, I want to say that about twenty-five years ago, when I first entered the glue business, I found out that some manufacturers and dealers were using, for comparison, an old set of standards originally put out by Peter Cooper. Now Peter Cooper was a philanthropist, as you all know, but he was not so much of a philanthropist as to tell his competitors just how he made his glue and how he tested it; and as to just what the meaning of the Peter Cooper standard was, nobody knew. But he sold his glue on definite grades that ran as follows: No. 2 was his lowest grade; then came Nos. $1\frac{7}{8}$, $1\frac{3}{4}$, $1\frac{5}{8}$, $1\frac{1}{2}$ and so on up.

I gathered what experience I could in the glue business, and then I set to work to try and define these grades of Cooper, which were not by any means invented by me, and the exact meaning of which I did not know; but I felt that there ought to be some way of defining the common grades of glue that were in general use. Other manufacturers did not use the same marks as Cooper, but they had grades that corresponded roughly to Cooper's grades, and any glue manufacturer or dealer, if asked for a glue to equal Cooper's $1\frac{1}{4}$, would know just what was meant, because he had found out by practical test how to match them up. So when I hear that nothing was done about standardizing glue, I simply would refer those interested to a paper I read in 1905, published in the *Journal* of the Society of Chemical Industry in 1906, in which I carefully described the Cooper grades and how those grades could be determined in a very simple way, and also many other tests. I think you will find reference to that work in Mr. Bogue's book and also in *Monograph No. 11*, of the American Chemical Society, on Glue and Gelatine, recently published. I mention this to show you that although Cooper was not sufficient of a philanthropist to publish his test methods and the other glue manufacturers were not philanthropic enough to publish their test methods, they all had them.

On the other hand, there is a big literature on glue, as you can find from the indices of the *Journal* of the Society of Chemical Industry, Chemical Abstracts, etc. You will find a big bibliography, and there is a large literature on the testing of glue; and recently there has been a large number of papers published in reference to it. I want to show you now a simple instrument for testing the jelly strength

Mr. Alexander

of glue. It has never yet been tried. The glue is dissolved in a rubber stoppered bottle, under conditions laid down by the National Association of Glue Manufacturers, and then after the test bottle has been placed in a thermostat and brought to the proper temperature, the frame of this little standard plunger of the same type used in the Bloom gelometer described by Mr. Richardson, is allowed to rest upon the top of the bottle until the plunger just touches the glue jelly. On top of this plunger support is a little dial which, by its rotation, shows when the plunger has been depressed into the glue jelly to any desired distance, say 1 cm. or 4 mm., or whatever you will. The bottle is placed on this stand, and then moved up by means of this little milled-head screw on the side. You see now a pressure of about 50 g. is being exerted upon this plunger. If that 50 g. would have made the proper rotation of the dial, say a rotation corresponding to a depression of 1 cm., then 50 g. would be the jelly strength of the glue. Here is a little pawl which keeps the pointer from falling backward. Now the more weight or the more pressure necessary to cause that depression, the higher the registration here, and as soon as you cease moving the bottle upward, which you can do within any desired period of time, your jelly strength is registered in grams on the dial. It is a very simple instrument. If it works consistently, as I think it will, it is perhaps less expensive and easier to construct than the very beautiful gelometer worked out in Mr. Richardson's laboratory. That instrument automatically dumps a certain stream of shot on to a pan above the plunger, and when the weight of the shot dumped on the plunger pan causes a depression of 4 mm., an electric circuit is made which instantly cuts off the supply of shot. Then you have to take your plunger pan and shot and weigh them to find the jelly strength. The instrument here has the advantage of showing the weight in grams directly on this scale, and I think it will work quicker.

I may say that while all these refinements of testing are very good and very desirable, one of the speakers brought out the important point that the differences which may occur in the using of glue are so tremendous that very slight differences, even a difference of 5 per cent in the test, are relatively not so important as would first appear. Even the MacMichael viscosimeter, which was heralded as being a wonderful boon to all testers of viscosity, is only claimed by the manufacturers to be accurate within 5 per cent. That is what their circulars say relative to it.

Two men in different plants may be doing the same work with the same glue, but doing it a little differently. One may get perfect results out of a certain glue; another man will get miserable results

Mr. Alexander with that glue, but better results with another glue that would not suit the first man.

You must also take into consideration not only the conditions demanded by the work, but by the workmen. In the case of sizing a barrel, for example, you would have to look into all the conditions that exist and make a balance between the price of low-grade glue and the efficiency of high-grade glue. Generally speaking, high-grade glue goes a great deal further than low-grade. It is a question of taking the particular glues and balancing increased carrying power against low price. In some cases low-grade glues will not do the work under any circumstances, whereas in others, high-grade glues will not do the work under any circumstances. Mr. R. E. Wilson has shown how, by the addition of minute quantities of chrome alum to glues, their efficiency in sizing barrels is greatly increased, owing to the fact that the glue jellies up better and does not run down the side of the barrel in hot weather. There are a lot of tricks of that kind that make a glue work, where a man who did not have the knowledge and experience that Mr. Wilson has published, would not get the results.

There are all sorts of tricks; I do not mean secrets, furtive things, but methods of common sense and ingenuity. There is no substitute for common sense in any operation, especially in a large scale operation. I was once called into a factory where the manufacturer said that the barrel of glue was mixed, that half of it gave perfect results and all of a sudden everything was wrong. The trouble seemed to be in certain joints. I walked through the factory; it was in the northern part of New York State. It was quite cold out, well down below zero, and the fellow doing that joint work had a window pane broken near him, and of course the icy air chilled the glue before the joint could make up. I told them what they needed was not a glue expert but a glazier. Ninety-nine times out of a hundred, if a consumer makes a complaint, you can trace it down to some fault in his method of operation. I think Mr. Kelley gave you good evidence of the fact that the glue manufacturers knew their job; they had to know it.

Mr. Baxter MR. F. R. BAXTER.[1]—I am connected with a company that uses large quantities of glue for the interior finish of wood barrels in which all kinds of petroleum products are shipped and stored, and the question of the selection of the proper grade is always an important one. A number of years ago we concluded to try every known test of glue to find if several of these could not be coordinated and worked into a method that would enable us to differentiate suitable grades.

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[1] Chief, Testing Laboratory, Vacuum Oil Co., Rochester, N. Y.

Mr. Baxter

The surface was examined to learn whether or not there had been any bacterial decomposition during the process of manufacture; the fracture noted to find out whether it splintered or what form it did take when broken; the odor dry and in solution; and the viscosity. The viscosity is taken on a definite concentration of the glue; 60 g. as received is added to 360 cc. of water in a beaker which is placed in an ice box over night at a temperature of about 40° F. Next morning the beaker is placed in a water bath, heated to 140° F. (not over 150°) and stirred occasionally, but not briskly, to solution. When completely dissolved, the glue is strained and the viscosity measured in the usual way at 140° F. in a Saybolt Universal viscosimeter. The odor wet is observed; the foam test made; the sour test noted to determine its keeping qualities; the grease test, the reaction, acid or alkaline, and the test for adhesive strength are made. This latter you may call a rule of thumb, but it works out fairly well. The same strength solution that is used for the jelly strength test is taken and in it is immersed a piece of barrel stave of the ordinary kind for a definite length of time and allowed to cool for a standard length of time. It is then sprinkled with sawdust and allowed to set. The strength with which the glue holds the sawdust is observed—a rather rough-and-ready test, but it is an indication of the property desired. The moisture in the glue as received is also determined.

The jelly strength is a most important test. A solution of 60 g. of glue in 360 g. of water is carefully made at a temperature between 140 and 150° F., portions filled into heavy glass tumblers (such as used in milk-shake machines) and allowed to cool 15 hours at 45° F. In the morning the samples are tested immediately upon removal from the cold chamber. The frame carrying the $\frac{1}{2}$-in. brass tube to the bottom of which is attached a $\frac{5}{8}$-in. disk and the top a cup for receiving shot, is placed on the tumblers, the disk lightly resting on the jelly. (The lower edge of the disk must be rounded to a perfect curve.) Shot is dropped gently and continuously from a controlled container until the disk just breaks the surface. The sum of the weight of shot added, plus the weight of plunger in grams, is the jelly strength. An average of 800 to 900 g. is a good barrel sizing glue.

Besides the jelly strength, the jell point, viscosity, keeping qualities and adhesion are of much service in evaluating glue. Each one of the tests named is given an arbitrary number, varying with the grade under examination, and their total for any given sample is called its glue value number.

Mr. Powell

MR. POWELL.—Just a word about specifications, since they came up. If any of you do have occasion to write specifications, keep out the irrelevant tests, so that the man to whom grease means nothing, will not specify the amount of grease to be in the glue, and so that the man to whom foam means nothing, will not specify the foam, and the man to whom ash means nothing will not insist on a critical ash determination. All that puts a burden on the glue producer, because he has four or five tests to match exactly. Remember he cannot make his glue to these specifications, he has to select it and he goes to his cabinet of hundreds of lots of glue and takes out glue after glue and tests it until he gets one that meets the exact specification, and you pay for it. If you can say what you want, the glue manufacturer will give it to you as nearly as he can, but don't ask him to give you something you do not need.

As to simplicity of testing: I do not think there is any glue laboratory in the country that has not an apparatus case that is not filled with apparatus with which they have tried to make simple glue tests, and as has been said, they have all been failures. We do not know whether we are going to get what we want now; we think we are; we are going to get near enough together so that two people from different laboratories can agree about one sample, at least. But it requires a certain amount of technique and exact handling to get it. It will be made as simple as possible, surely.

The shot test just described by Mr. Baxter, was used in the glue industry years ago a great deal, but it is very erratic, due to the formation of a skin on top of the jell, due to surface drying. You are not testing the resiliency of your jell, but the strength of the skin on top of the glue, and the results vary greatly, depending on the conditions under which the jelly has been seasoned.

Mr. Richardson

MR. RICHARDSON.—I do not think I have anything that can be added to this discussion. It seems to me to have been brought out clearly that heretofore the various laboratories have not been able to make their tests in such a way that they could obtain comparative results, one with another; each laboratory has had its own methods and it own instruments, and within a given works the various grades were well established and duplicate lots could be matched; and any user of glue products could obtain what he wanted from a given producer. That has been the situation, and until the Association of Glue Manufacturers took hold of the matter, no attempt had been made to establish uniform methods of testing which were comparable in all laboratories. It seems that that situation is being remedied, and we have every assurance that before long standard methods of

Mr. Richardson

testing will be available. I think the point brought out by Mr. Powell is a very important one, taken especially in conjunction with the remarks made by Mr. Alexander and others, that in all likelihood we shall not be able to find one test, or perhaps even two or three tests, which will be sufficiently fundamental and general so that they will establish standard grades of glues for all purposes and all manufacturers. While the single method of testing which appears to be uppermost in Mr. Bogue's mind is very desirable, I question that it can ever be attained owing to the fact that different manufacturers require different qualities and properties in the glue they use; and if they were to ask for a glue based on a single specification, in the first place, they would probably not get what they want, or if they got a grade under that single specification which was what they wanted, it would probably be a better glue than they needed and would cost them more money than they could afford to pay, everything considered, or than they ought to be asked to pay. It seems to me that the outcome of the whole matter will be that those various tests, perhaps five or six or seven in number, will be standardized; the manufacturers will select the fundamental properties of glues important to their particular work, whether it be, for example, the chlorides to the match manufacturer or the hygroscopic properties or jelly strength or viscosity for some other use, or grease for the wall paper manufacturer, or foam for the one who uses machines in spreading his glue—they will select from the number of standardized tests those which are of primary importance for their use, and making use of those tests in their specifications and giving them to the manufacturer, they will undoubtedly obtain what they want for the lowest cost.

NEW DEVELOPMENTS IN ELECTRIC TELEMETERS[1]

By O. S. Peters[2] and R. S. Johnston[3]

Development work has been in progress for a number of years at the U. S. Bureau of Standards on a line of strain, stress, and pressure gages with the view of making them remote reading and remote recording, and at the same time adapting them to the measurement of displacements and forces that are rapidly varying in value. Instruments capable of accomplishing these purposes would have numerous advantages over those now in use. The latter, in general, are suitable only for measuring values that are relatively steady, and instead of being remote reading or recording must be read at the point of measurement, which in many cases, as in field work, may be inaccessible. Satisfactory progress has been made and instruments for several purposes developed with sufficient accuracy and stability for many kinds of engineering measurements.

For their operation these instruments depend upon the well-known pressure-resistance, or corresponding displacement-resistance, characteristic of a stack of carbon plates. Heretofore, attempts have been made to utilize this characteristic of carbon for making engineering measurements, but without success because of erratic behavior of the resistance of the carbon stack, hysteresis effects, and also the non-linear relationship existing between displacement and resistance. In the present work, the resistance of the stack has been stabilized, and changes of resistance with changes of pressure made reproducible, by mounting the stack in a frame in such a way that it is always under a considerable pressure, and confining the range of change of pressure within comparatively narrow limits. The pressure on the stack is also applied axially and great care is exercised in assembling in order to make transverse forces on the carbon contacts as small as practicable.

Hysteresis effects, or those effects which cause the resistance of the stack corresponding to a given pressure to be higher or lower, depending upon whether the pressure has been approached from a higher or lower value, have been made negligible by using a mounting frame without mechanical joints, it having been found by experiment that the cause of the hysteresis lay in these joints rather than in the stack itself. Care is also exercised to prevent any part of the frame from being stressed beyond the elastic limit of the material by the pressure applied.

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[1] Published by permission of the Director of the U. S. Bureau of Standards.

[2] Electrical Engineer, U. S. Bureau of Standards, Washington, D. C.

[3] Engineer Physicist, U. S. Bureau of Standards, Washington, D. C.

The non-linear characteristic has been overcome by mounting two stacks of plates in each instrument in such a way that the resistance of one is decreased and the other increased by the force or displacement under measurement. By experiment, the cumulative change of resistance of the two stacks has been found to be proportional to the change of displacement or pressure, with the ultimate result that an instrument with substantially a linear calibration curve is obtained.

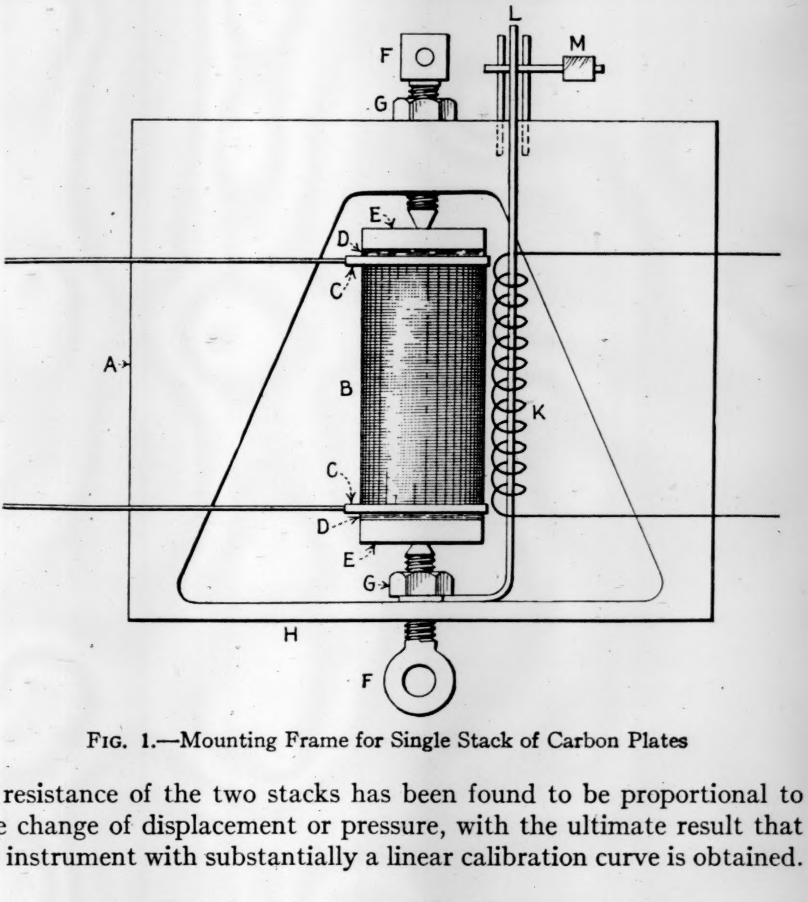

FIG. 1.—Mounting Frame for Single Stack of Carbon Plates

THE APPARATUS AND ITS OPERATION

General Form of Mounting for Single Stack.—In Fig. 1 is shown a mounting frame for a single stack of plates which embodies the essential elements necessary to secure the stability of resistance and freedom from hysteresis effects described above, and a form which has been used extensively for testing different kinds of carbon. *A*, is a steel frame cut from a solid piece of metal; *B*, a stack of carbon plates, or rather annular rings about fifty in number; *C*, a pair of brass contact disks for making electrical contact with the carbon rings; *D*, a pair of mica disks insulating the carbon from the frame; and *E*, a pair of tempered steel end pieces cupped axially to receive the round-pointed tempered steel set-screws, *F*, which are threaded into the frame and held rigidly by the lock nuts, *G*. With a stack of rings assembled between the set-screws, the pressure is adjusted to a pre-determined value and the lock-nuts drawn up to prevent any further movement of the screws in the thread. Part *H* functions as a stiff spring which is deflected a small amount by the force or displacement under measurement, the resistance being measured by means of the contacts, *C*. *K*, *L* and *M* are not a part of the resistor, but an auxiliary reflecting device for calibrating purposes, the mirror, *M*, being rotated by deflection of spring, *H*, through rod *L*, *K* being a magnetizing coil to make the roller stick.

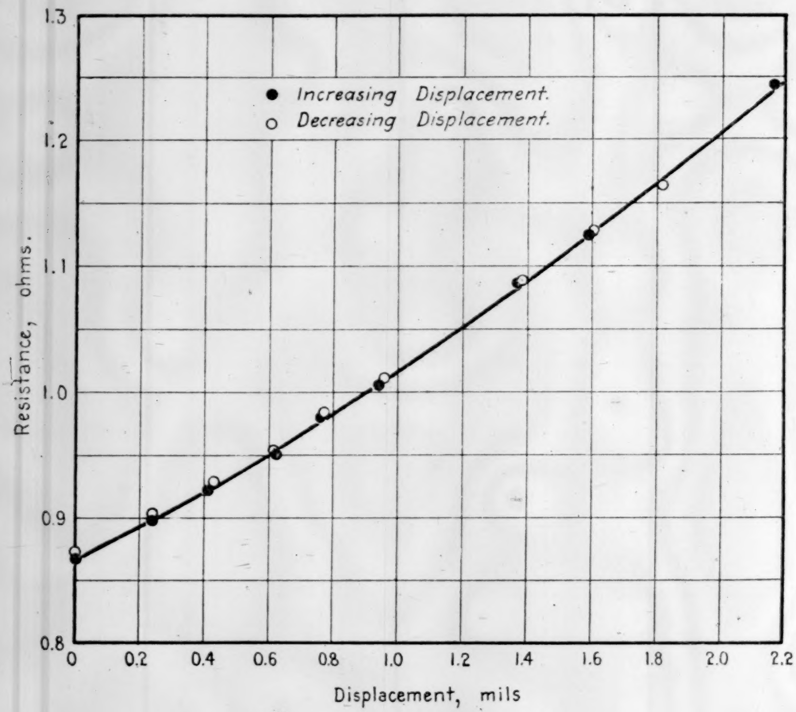

FIG. 2.—Displacement-Resistance Curve for Single Stack of Carbon Plates (Carbon-Resistor No. 5)

In Fig. 2 is shown a typical displacement-resistance curve obtained from a single stack resistor of the type described above. The magnitude of the change of resistance is of particular interest, it being approximately 46 per cent for a change of length of the stack of 0.00217 in. Attention is directed to the coincidence of increasing and decreasing values; that is, the hysteresis effect in the stack itself is negligible, and it may also be stated that after the stacks are properly aged they will retain their values of resistance for long periods under ordinary handling with changes not exceeding one per cent.

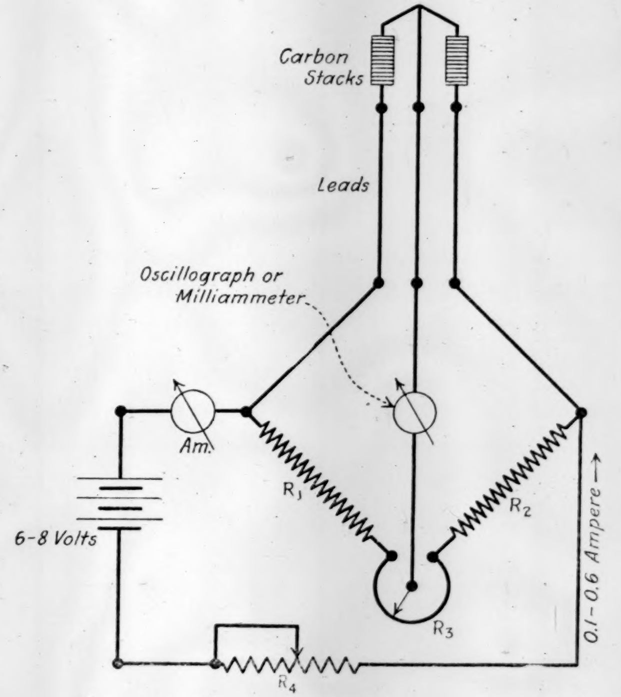

FIG. 3.—Two Stacks of Carbon Plates in Wheatstone Bridge Circuit

Electrical Circuits for Indicating or Recording Measurements.—In order to utilize the change of resistance of the two stacks of rings, which, as stated above, are incorporated in each instrument, they are connected in a Wheatstone bridge circuit as shown in Fig. 3, the two branches of which consist of the carbon stacks and leads in series on the one hand, and the resistances R_1, R_2 and R_3 on the other, R_1 and R_2 being fixed resistances and R_3 a slide wire resistance by means of which a fine degree of balance of the bridge is obtainable. The bridging instrument may be a milliammeter, galvanometer or oscillograph element, and is connected between the midpoint of the carbon stacks and the movable contact on R_3. The bridge is energized from the battery at the left, the current being kept at some suitable constant value by means of the variable resistance R_4.

In use, the bridge is first balanced. Then as change takes place in the resistance of the carbon stacks the balance is upset and the bridging instrument deflected. As long as the current taken by the bridging instrument is small in comparison with the total current in the bridge circuit these deflections will be proportional to the total change of resistance of the stacks, and the latter being proportional to the force or displacement under test, the ultimate result is a uniform scale reading of the bridging instrument. This enables calibration directly in thousandths of an inch in the case of strain gages, or total load or pounds per square inch in the case of stress or pressure gages.

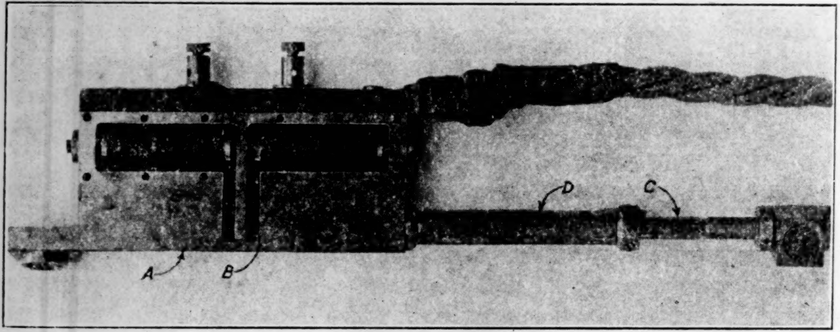

FIG. 4.—Strain Indicator for Rigid Members

The leads from the carbon stacks to the other portion of the bridge circuit may be of almost any length, some of No. 16 copper wire 100 ft. long having been used successfully. This enables simultaneous readings or records at a number of widely separated points by a single operator, the fixed resistances and bridging instruments being combined in one suitable panel. Groups of as many as twelve have been used, and larger groups could be assembled if necessary.

Strain Indicator for Rigid Members.—In order to measure strains in rigid structural members, a pair of carbon resistors is incorporated in a device the details of which are shown in Fig. 4. Here, *A* is a frame formed from a solid piece, preferably of nickel steel, of which *B* is a cantilever integral with the frame and having a relief cut near its fixed end. Part *C* is a rod which is threaded into the cantilever and drawn up with a lock-nut, the rod being free to move in the guide *D* which fits closely enough to prevent appreciable lateral movement. The stacks of rings are assembled on each side of the cantilever, with

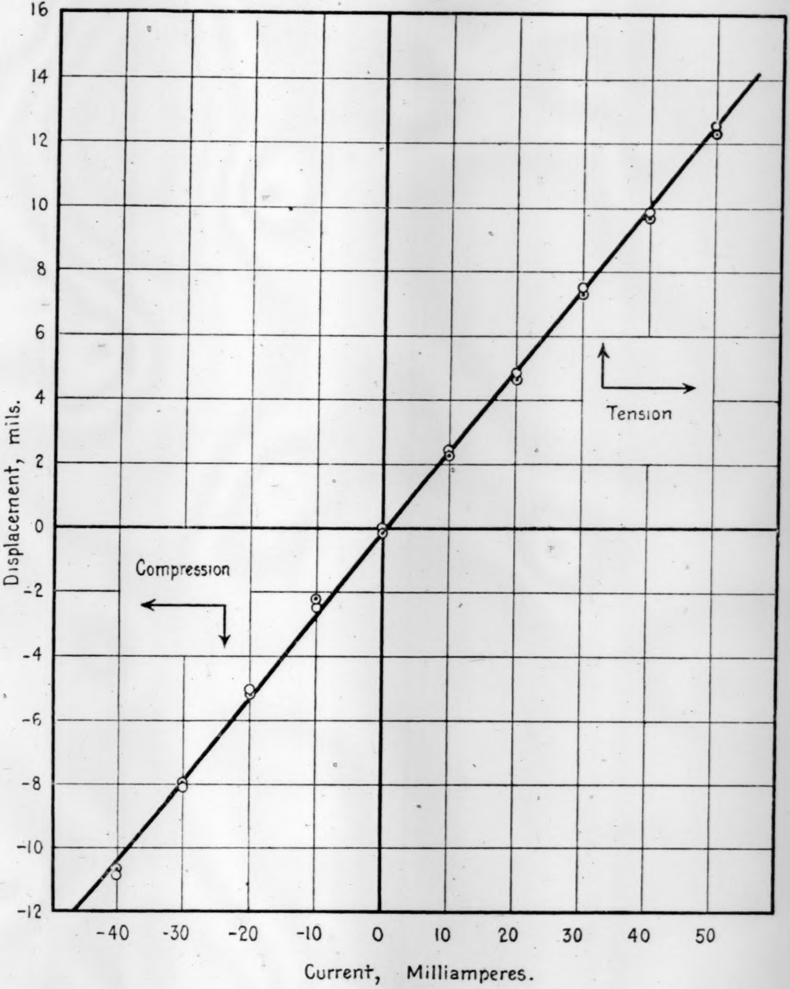

FIG. 5.—Calibration Curve for Carbon-Resistor Type of Strain Indicator

a three-conductor wire lead connecting the stacks to the indicating device. At the outer end of the rod is a block with two nuts which serve to adjust the position of the cantilever, and also clamp the block to the rod. The span covered by the instrument shown is

8 in., although others have been made ranging in span from 2 to 24 in., and having full scale readings from 0.002 to 0.024 in.

In operation, the indicator is attached to a member by means of clamps, the sharp steel points shown at either end in Fig. 4 being forced into punch marks in the metal. The clamping is done with the nuts at the outer end of the rod loose, after which these are drawn up in such a way as to bring the cantilever to its initial position as shown by a zero reading of the indicating instrument. As stress comes on the member, the length changes and the rod and cantilever move relatively to the frame. Part of this displacement is communicated to the stacks of rings, changing their resistance and causing deflection of the bridging instrument, which as indicated above is proportional to the strain.

The force required to operate the instrument depends largely upon the resistance of the cantilever to bending, but this need not exceed 10 lb. as a maximum which is a small portion of the total stress usually involved.

A typical calibration curve is shown in Fig. 5, which indicates the performance in both tension and compression with reversal of stress, and also the linear characteristic resulting from the use of two stacks of plates as compared with Fig. 2, where a single stack was used. The hysteresis effects found in present equipment do not in any case exceed 4 per cent of the full range of the instrument, and in the majority of cases are less than 2 per cent. At the same time they are of such a nature that correction can be made for them if it is desirable.

The method of calibration consists in clamping the gage to a base having one fixed block and one movable by a screw, with a micrometer microscope to measure the displacement. For any given displacement the bridging instrument in Fig. 3 can be adjusted to read an even number of divisions, thus doing away with multiplying factors, and making the readings direct in whatever units are chosen.

As to the accuracy obtainable, it may be stated that with great care, and where uni-directional strains are involved, the errors need not exceed 0.5 per cent. With ordinary care and where there may be a reversal of stress, the errors need not exceed 2 per cent, and in work involving vibrations or unfavorable conditions 5 per cent.

PRACTICAL APPLICATION IN LABORATORY TESTS

A set of twelve of the instruments described above originally intended for use on the U. S. Navy Fleet Airship No. 1 now under construction at Lakehurst, N. J., has been in use at the U. S. Bureau of Standards for some time. They have been used in the tests of 143 lattice girder members which were full-size duplicate parts of the duralumin frame construction of Fleet Airship No. 1, and on fourteen column test specimens of the investigational tests for the Delaware River Bridge Joint Commission. These instruments were designed to have a maximum range equivalent to a total deformation in 8 in. of 0.012 in. In both the test series they have consistently been used considerably beyond their designed range with no noticeable effect upon their calibration.

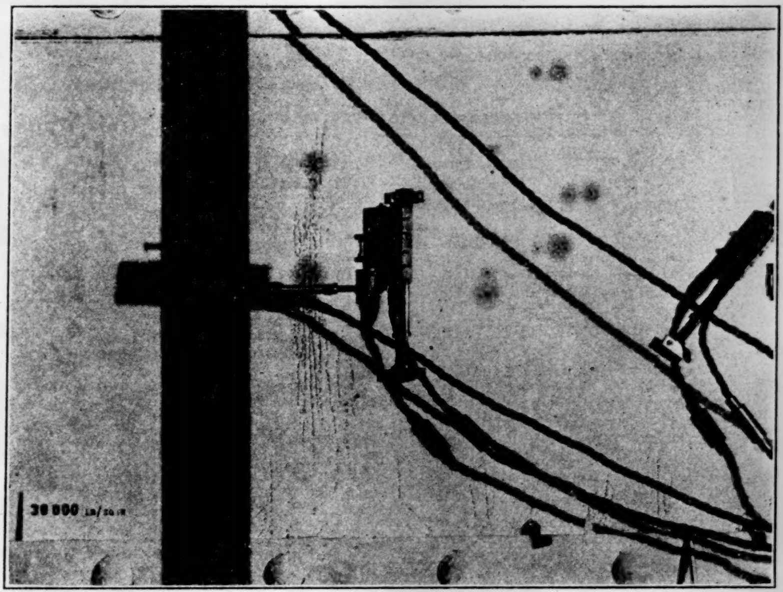

FIG. 6.—Telemeter Gages Clamped to Web of Plate Girder

In the Delaware River Bridge tests, a most interesting comparison as the result of their use is that which shows the relation between longitudinal and transverse strains on the web of the column section tested. The location of the instruments on the 1-in. web plate at the mid-length of the specimen is shown in Fig. 6, and the log sheet readings are given in Table I. The consistency of the operation of the instruments is nicely shown by these readings. A study of the data of the tests indicates that the load in the column section was not uniformly distributed throughout the length of the member until a stress of 15,000 to 20,000 lb. per sq. in. had been applied—the strain measurements at the ends and the early development of

Hartman lines at the ends being duplicating evidence of this fact. The strains measured at mid-length, therefore, would not be in agreement with what might be expected for the assumed uniform distribution of stress. The modulus would therefore be high. As loading is continued and buckling of the web proceeds, the longitudinal strains would be somewhat excessive and modulus computations show a lower modulus. This phenomenon is indicated in the deformation shown for instrument No. 2. It is of interest to note the uniformity of the relations of transverse to longitudinal strain throughout the test, as exhibited by two similar instruments in simultaneous operation.

TABLE I.—TESTS ON PLATE GIRDER WEB. DELAWARE RIVER BRIDGE JOINT COMMISSION

Applied Load, lb. per sq. in.	Total Deformation in 8 in., in.		Ratio, Transverse Strain to Longitudinal Strain
	Instrument No. 2 (Longitudinal Strain-Compression)[1]	Instrument No. 10 (Transverse Strain-Tension)[1]	
0	0.0000	0.0000	0.000
5 000	0.0012	0.0002	0.167
10 000	0.0027	0.0005	0.185
15 000	0.0042	0.0009	0.214
20 000	0.0056	0.0012	0.214
22 000	0.0061	0.0014	0.229
24 000	0.0068	0.0015	0.220
26 000	0.0073	0.0017	0.233
28 000	0.0080	0.0018	0.228
30 000	0.0088	0.0020	0.227
32 000	0.0096	0.0022	0.229
34 000	0.0102	0.00225	0.220
36 000	0.0117	0.0025	0.214
38 000	0.0120	0.0027	0.225

[1] Last figure estimated.

OTHER APPLICATIONS

Field Tests on Bridges.—Field tests on bridges have been limited to testing the apparatus in field service rather than analyzing the distribution of stresses in structures, but enough has been done, and the design sufficiently advanced, to show that good results can be obtained. In Fig. 7 are reproduced two records of the strains occurring in some of the tension members of a steel truss bridge while trains were passing. The six traces were taken simultaneously with an oscillographic recorder, four being gage records, one a time record, and one inactive. The outfit is equipped for six gages, but only four were available at the time of taking the records. The width of the film is 6 in., and the calibration is in pounds per square inch per millimeter of deflection.

The vibrations recorded range between 2 cycles and 100 cycles per second, the amplitude of the higher frequencies being insignificant, of course, as compared with that of the lower frequencies. With vibrations of this character, repeated tests have shown very little effect on the calibration, and as the free period of the different parts of the apparatus which could introduce sympathetic vibrations was adjusted above 300 cycles per second, or three times the highest frequency recorded, it appears that the deflections must be proportional to the strains.

The entire equipment for the field outfit may easily be carried in a touring car or light truck.

Airplanes and Airships.—One of the first applications was to the measurement of stresses in airplane stay-cables during flight, a recorder having been developed that enables twelve records to be taken simultaneously during maneuvers of various kinds. A similar application has been made to airships, although no records or readings during flight have so far been taken, the tests having been confined to work during construction.

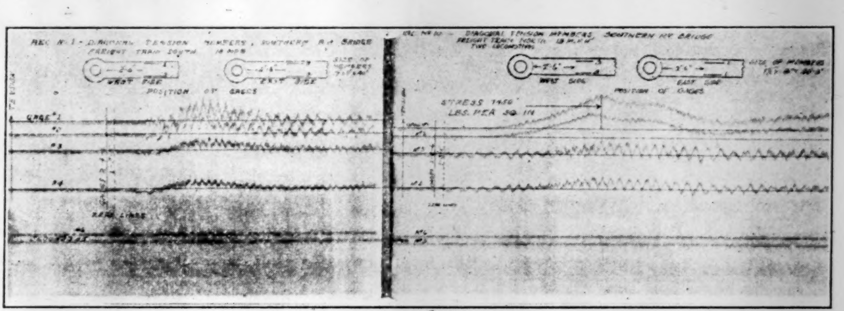

FIG. 7.—Vibratory Strains in Railroad Bridge Due to Passing Trains

Dynamometers.—A carbon resistor device for recording the pull on the pressure arm of a machine for testing brake band material has been in service for some months with good results, giving steady readings, and permitting one operator to care for several machines instead of one as would be the case if a spring balance were used.

Pressure Gages.—Experimental applications have been made to the measurement of pressures ranging from a few millimeters of mercury to 40,000 lb. per sq. in. More work remains to be done in this as well as other directions, but the indications are that pressure gages can be made that will give satisfactory results, and that will be especially valuable where records are desired of rapidly changing pressures.

OPTICAL STRAIN GAGES AND EXTENSOMETERS[1]

By L. B. TUCKERMAN[2]

For many years optical methods have furnished the most sensitive and most accurate means of measuring small distances, including small displacements or deformations. For this reason they have been chosen where extreme sensibility or extreme accuracy was desired. The Martens extensometer[3] was heretofore the most precise extensometer in common use and the interference method of Peters[4] has been the most accurate method of checking precision of gages.

Unfortunately, optical devices, although precise and sensitive, have been cumbersome and difficult to maintain in adjustment. This has limited their usefulness in great measure, confining them largely to well-equipped laboratories with a highly trained personnel.

A study of optical methods of length measurement for the purpose of making them conveniently usable under ordinary conditions has led to the development of the new optical devices here described, which are free from many of the disadvantages of the older types.

Two optical methods are conveniently available, interference methods and the optical lever. The use of a microscope, as in the Ewing extensometer, might also be called an optical method, but the principal of the instrument is mechanical, the microscope being used solely to make the displacements conveniently readable. Possibly the accidental double refraction of transparent materials under stress, discovered by Brewster,[5] applied by Maxwell[6] to the study of plane stress, and recently brought again into prominence in "optical stress analysis," might also be used, but there seems to be no likelihood that it will ever be used in a strain gage or extensometer.

INTERFERENCE METHODS

Interference methods have the advantage that they carry their own calibration with them. This makes them useful in standardization. Except in very limited fields, however, the tediousness of counting bands excludes them from common use. Fig. 1 shows the principle of the Fabry-Perot[1] interferometer. The incoming beam of light is reflected at two nearly parallel surfaces. In Fig. 2 is shown a Fabry-Perot interferometer in the form of an interferometer dynamometer of rugged construction developed in collaboration with Mr. C. G. Peters of the U. S. Bureau of Standards. It is so designed that it will maintain its adjustment unchanged under very rough handling. The instrument can be pounded with a hammer or even dropped a foot or so onto a wooden floor with no perceptible change of its reading. This ruggedness has been obtained by machining the dynamometer from a single solid piece, eliminating all screw adjustments and supporting the interferometer plates in intimate contact with sturdy three-point bearings. The central symmetrical position of the plates makes the reading practically independent of eccentricities of loading. It is hoped to produce more convenient designs by introducing wrench-fitting screw joints in strain-free positions. This possibility has not yet been tried out.

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[1] Published by permission of the Director of the U. S. Bureau of Standards.

[2] Engineer Physicist, U. S. Bureau of Standards.

[3] A. Martens, "Handbook of Testing Materials" (Translation by G. C. Henning, 1899), p. 67.

[4] C. G. Peters and H. S. Boyd, U. S. Bureau of Standards *Scientific Paper No. 936;* A popular account in *American Machinist*, September 30 and October 7, 1920.

[5] D. Brewster, Phil. *Transactions* Royal Soc., Vol. 105, p. 60 (1815).

[6] J. Clark Maxwell, *Transactions* Royal Soc., Edition 20, Part 1, 1850; Scientific Papers, Vol. 1, p. 67.

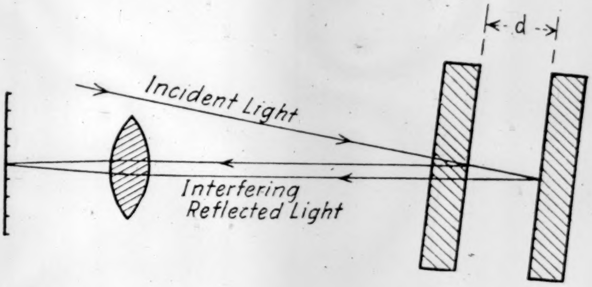

FIG. 1.—Fabry-Perot Interferometer

Measurement easily possible to $\frac{1}{40}$ wave length of the light used, approximately to $\frac{1}{2{,}000{,}000}$ in.

If the light from a helium lamp reflected from this instrument is viewed through a Pulfrich spectral system,[2] the field is crossed by interference bands as seen in the typical fields, Fig. 3. The reading consists in counting the bands as they pass the fiduciary mark (in the form of a cross). During this passage the bands narrow and widen and shift through all points of the compass, due to unavoidable small changes in the eccentricity of the applied load, but the symmetrical arrangement of the plates and the fiduciary mark makes the number of the band on the mark an accurate measure of the average stretch of the dynamometer. As the total range of this instrument is approximately 1000 bands and readings can readily be made to $\frac{1}{10}$ of a band, the instrument is obviously extremely sensitive. Its constancy, however, has not been fully checked. It is more constant in reading than the testing machines with which it has been compared.

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[1] Fabry et Perot, *Annales de Chem. et Phys.*, Vol. 22, p. 564 (1901).

[2] Pulfrich, *Zeitschrift*, f. Inst. k., Vol. 18, p. 261 (1898).

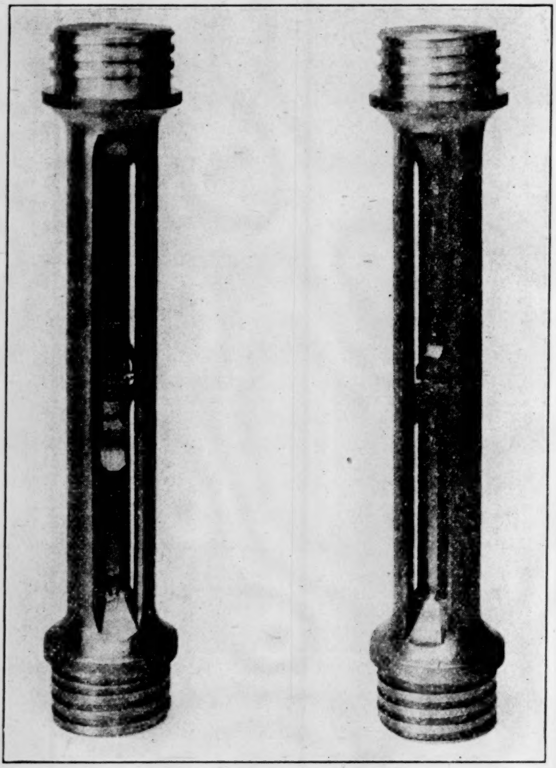

FIG. 2.—Interferometer Dynamometer

It is planned to make a complete check on the constancy in a dead weight machine now under construction.

The usefulness of this instrument is much limited by its sensitiveness to vibration. Heavy running machinery in the neighborhood sets up vibration sufficient to completely obliterate the bands.

OPTICAL LEVER

Observing Systems.—In Fig. 4 are shown three types of observing systems for the optical lever. The first is the uncollimated system in common use to-day—either as telescope and scale or as lamp and scale. Its disadvantages are well known, any relative shift of fiduciary mark, scale, lens or mirror may change not only the reading of the instrument, but also its calibration. Any one who has had the misfortune to have a partially completed series of readings made valueless by an inadvertent disturbance of such a system will realize fully how great a disadvantage this is.

The second is a collimated system. Here the calibration of the instrument depends solely on the focal lengths of the two lenses and their proper collimation. These can easily be made of rugged construction so that only extreme abuse will alter their adjustment. The reading will depend only on the relative direction of the two collimators (or collimator and telescope) and the mirror.

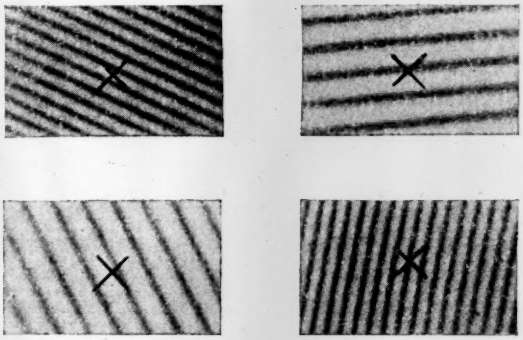

FIG. 3.—Interference Bands as Seen in Pulfrich Spectral System

The third is an auto-collimated system. Here the possibilities of disturbance are still further reduced. The calibration depends only on the focal length of the auto-collimator and its proper collimation. The reading will depend only on the relative direction of the auto-collimator and mirror.

Both collimated and auto-collimated systems have been used for a long time in many fields of optics. So far as I knew they had not been applied to optical levers, but since this work was begun, Gehlhoff[1] has published a description of an auto-collimator adapted for use with an optical lever.

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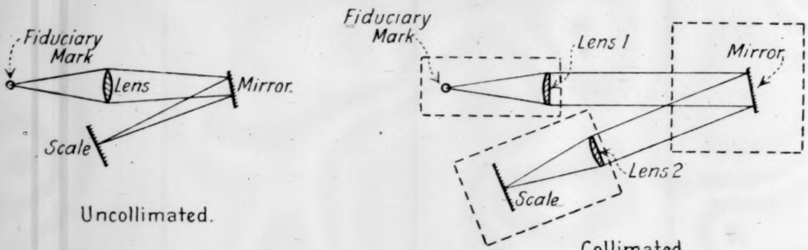
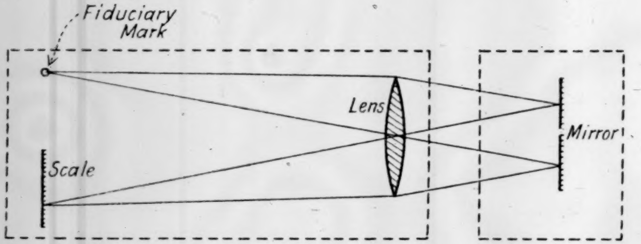
[1] G. Gehlhoff, *Zeitschrift*, f. Tech. Phys., Vol. 3, p. 225 (1922).


Fiduciary Mark
Lens
Mirror
Scale
Fiduciary Mark
Lens 1
Mirror
Lens 2
Scale

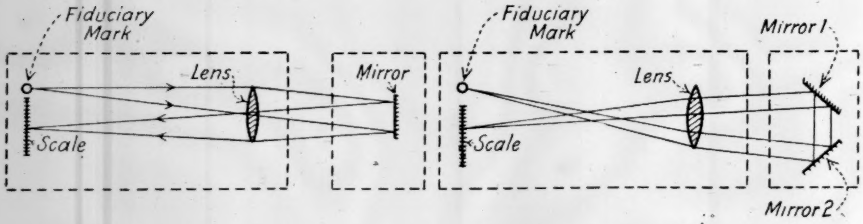
Uncollimated.

Collimated.


Fiduciary Mark
Lens
Mirror
Scale

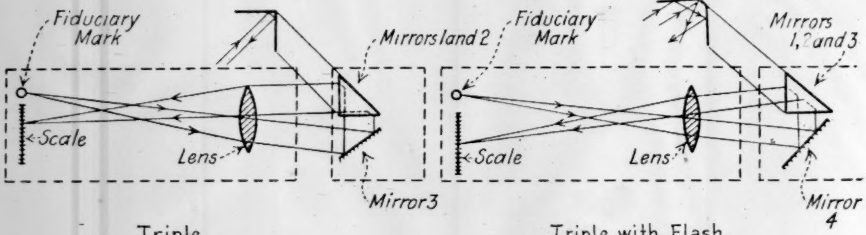
Autocollimated.

FIG. 4.—Three Types of Observing Systems for the Optical Lever


Fiduciary Mark
Lens
Mirror
Scale
Fiduciary Mark
Lens
Mirror 1
Scale
Mirror 2

Single.

Double


Fiduciary Mark
Mirrors 1 and 2
Scale
Lens
Mirror 3
Fiduciary Mark
Mirrors 1, 2 and 3
Scale
Lens
Mirror 4

Triple.

Triple with Flash.

FIG. 5.—Four Mirror Systems

Mirror Systems.—The advantage of the auto-collimated observing system is so great that the mirror systems are all discussed in combination with an auto-collimated observing system. In Fig. 5 are shown four different mirror systems. The first is the common single mirror system as used on the Martens extensometer. The reading is intended to measure the extension of the test specimen, but every rotation of the observing system or of the specimen as a whole will alter and therefore vitiate the readings.

The second is a double mirror system. (This system was used by Gehlhoff). A reading taken in any direction perpendicular to the line of intersection of the two mirrors depends solely on the angle between the two mirrors. Deviation from the perpendicular introduces a cosine error which is negligible for small deviations.

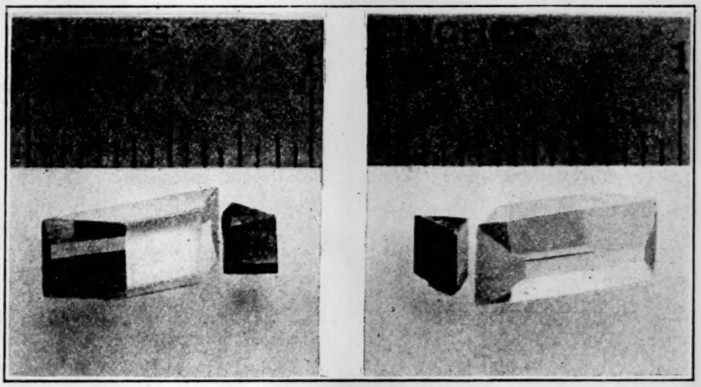


FIG. 6.—Prism System

The advantage of this system has long been known. Its most familiar application is in the sextant. Used with an auto-collimator it has a disadvantage not found in the sextant. Any deviation of the auto-collimator axis from the plane perpendicular to the mirror intersection moves the reflected light at right angles to the scale through double the angle. This does not appreciably alter the reading (the cosine error is small), but makes the reading more difficult.

The third is the new triple mirror system, finally adopted. Two mirrors are fixed together practically at right angles, and a third, relatively movable, is nearly at right angles to these two, its axis of rotation being practically perpendicular to the line of intersection of these fixed pairs. A reading can be taken from practically any position in front of the three mirrors, and the image of the fiduciary mark in the observing field is steady, even with a vibrating or rotating mirror system. There is, of course, a cosine error. This could be eliminated as in the use of the sextant by swinging the instrument to a maximum reading, but the fourth system was found more convenient, a small auxiliary mirror being used to convert it effectively into a combination of double and triple mirror systems thus producing a "flash" as the collimator passes through the perpendicular position. Whenever the "flash" is within the field of view, the deviation from the perpendicular is so small that the cosine error is negligible.

For use with an auto-collimator the mirrors must have very accurately plane surfaces. These are more easily obtained in prisms than in ordinary plane mirrors. Fig. 6 shows two views of the prism system as constructed. It consists of an ordinary 45-deg. prism and a roof prism modified to give the "flash." Fig. 7 is a diagram of the auto-collimator, showing the illuminating lamp and prism, the reticule carrying fiduciary mark vernier and scale, and the auxiliary "focal adjuster." The rulings on the reticule face the objective. The instrument is permanently adjusted to a standard focal length (25 cm.) and the bearing ring of the reticule is accurately machined to focus. By this means the reticules are made interchangeable without the necessity of re-collimation or re-calibration.[1]

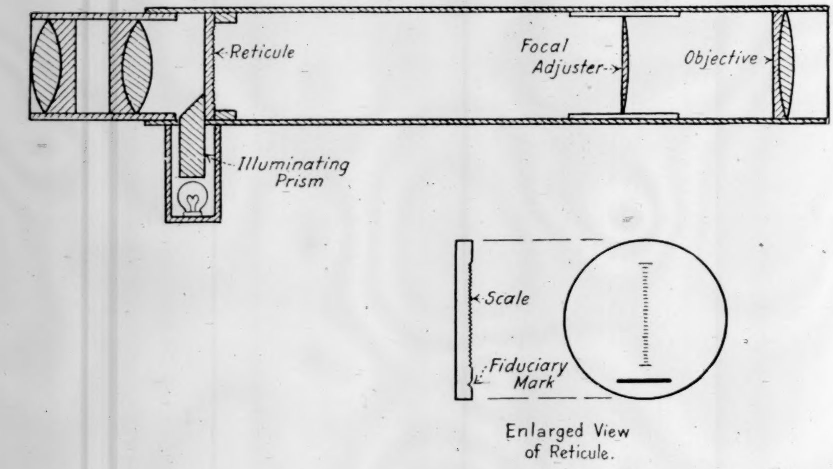


FIG. 7.—Diagrammatic Sketch of Auto-Collimator

Fig. 8 is a photograph of the observing field, showing the "flash," fiduciary mark, vernier[2] and scale. The shape of the illuminated area is designed to make alignment of the vernier easy. In connection with a 0.2-in. Martens lozenge, 1 scale division represents 0.00004-in. deformation. Tenths of a division are easily read. The reticule is graduated to read elongations directly in inches per inch on a 2-in. gage length.

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[1] The reticules for these instruments were ruled by Mr. W. Souder of the U. S. Bureau of Standards.

[2] The use of the vernier was suggested by Lieut. E. L. Gayhard of the Construction Division of the United States Navy.

The present instruments were built by the Naval Gun Factory for the Bureau of Construction and Repair of the United States Navy. An extended investigation of the stresses in riveted joints required the use of a large number of accurate strain gages. For this purpose the prisms are mounted in 2-in. strain gages, using the Martens lozenge. It is planned to place thirty of these strain gages at various points on the riveted specimens. These will be read by two auto-collimators. Fig. 9 is a working drawing of a strain gage. *A* is the knife edge, *B* the lozenge carrying the 45-deg. prism, *C*. *D* is the roof prism, which is adjusted by the double cam, *E*, with coarse and fine adjustment, to bring the fiduciary mark to zero on the scale. Fig. 10 is a photograph of the strain gage as constructed.

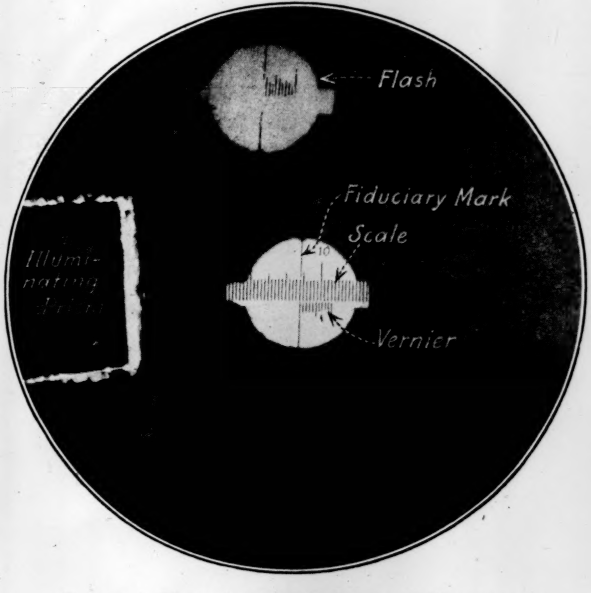

FIG. 8.—Observing Field of Auto-Collimator

The sensibility of the system as constructed (1/250,000 in.) is well above its possible limit. It may ultimately be possible by its use to measure strain conveniently and accurately on $\frac{1}{4}$-in. gage lengths.

The optical system described, obviously has many possibilities of application. It is directly applicable to a wide variety of strain gages and extensometers. On light specimens, such as thin sheet materials or wires where the weight of the extensometer is a controlling factor, it has obvious advantages. It is at present being adapted in the dynamometer laboratory of the U. S. Bureau of Standards to a torsion dynamometer for rotating shafts, and a number of other applications are contemplated.

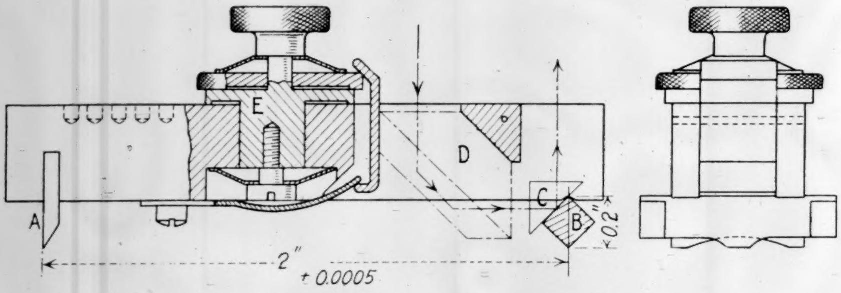

FIG. 9.—Diagram of Optical Strain Gage

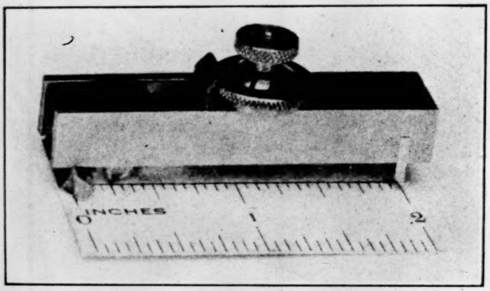

FIG. 10.—Optical Strain Gage

A NEW METHOD OF MAGNETIC INSPECTION

By A. V. de Forest[1]

The object of inspection is to discover before use whether an article is suited for the purpose intended. In general, this is done by comparing the sample with a standard known to have given good service. There is seldom any direct measurement of a given physical property which can be used to estimate the real value of a specimen. Physical measurements usually serve only as a basis of comparison. The magnetic inspection method here described is to be placed in the same category. It is based upon a comparison of an unknown with a known specimen, the performance of the latter having been determined under service conditions.

The outstanding advantage of the magnetic method of test lies, of course, in its non-destructiveness. But this is by no means its only advantage. In magnetic testing, the effects of what appear to be the internal stresses concommitant to structural changes are evaluated. Physical testing, by which is meant such mechanical testing as is commonly used to-day, involves deformation with consequent modification of internal stress. When we consider the part that such stresses must ultimately play in determining the performance of a material, particularly when such material has been subjected to a process of heat treatment, magnetic testing as a test procedure becomes of great promise of usefulness.

In the examination of material which in its finished form represents the summation of more than one characteristic or quality, or is the result of more than one thermal process, it is important to be able to effect at least a partial separation of the different factors, all of which combined produce a finished article. Let us consider a cutting tool. This tool has been subjected to a heat treatment, which embraced both a quench and a draw. A physical test, such as a hardness measurement, will give a single factor result and one which is not able to differentiate as between, for example, an overquench followed by an overdraw, and an underquench followed by an underdraw. It is, however, a characteristic of magnetic data that intermediate processes in thermal treatment leave a definite stamp on the material; that is, subsequent treatment does not entirely efface the effects of an earlier step.

While chemical composition and initial grain structure influence the cutting performance of a given tool, the most important variable entering into its manufacture rests upon its final thermal treatment. It is the thermal treatment, more than any other process, that causes the wide variation found in tools of commercially similar composition. An error of omission or commission in the quench or draw, or both, means decreased uniformity of production. A test method which serves to identify even in part that particular step in a thermal process in which departure from routine procedure occurs, is of great value. Those who have considered deeply as to the possible reasons for the performance of what proves to be an exceptionally good tool, will appreciate this.

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[1] Research Engineer, American Chain Co., Bridgeport, Conn.

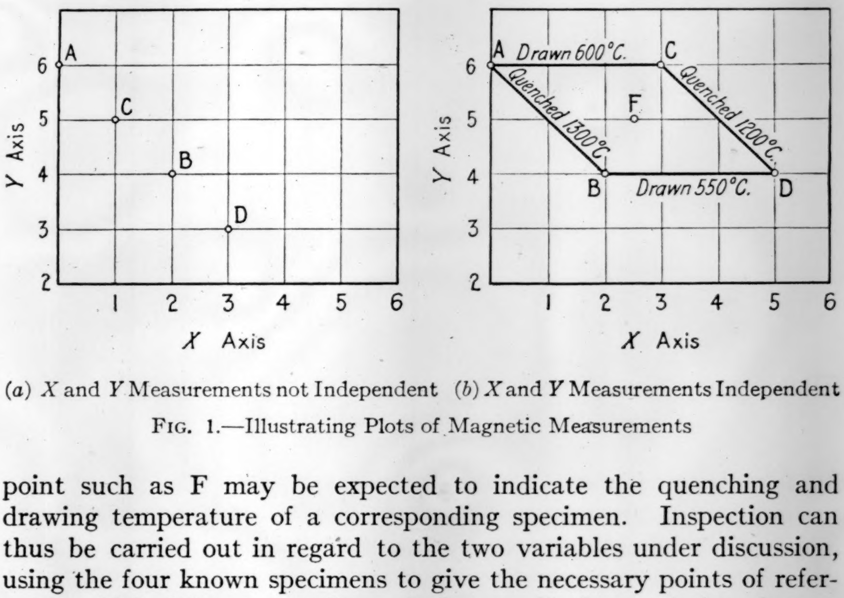
In describing a method of magnetic test procedure which effects a desired differentiation with respect to more than one variable, I wish to point out that this paper does not pretend to cover a completed research or to offer a full solution. Since, however, the results presented are obtained by a method believed to be entirely new and one which has found successful commercial application during the past two years, it is felt that publication at this time will stimulate others in the field of magnetic inspection and facilitate the gathering of additional magnetic data.

For the purpose of the discussion below, we shall consider that the chemical composition and initial grain structure of the specimens tested are fixed, and the factors which are our immediate concern are found in the quenching and drawing process. We shall not attempt at this time to explain the exact nature of the particular factors which influence the magnetic results. It is sufficient for the argument that they exist and that they serve to a degree to define the steps through which a tool has passed to its final condition.

To illustrate the general principle employed in differentiating between two or more quantities or properties, the following remarks may be made: We learn in elementary algebra that two independent equations must be set up to represent the known effects of two variables. If one equation is a simple multiple of the other, the solution is indefinite; a combination of the two unknowns can be evaluated, but not the quantities themselves. Similarly, the effects of two heat treatments can be separated only if two measurements can be made involving different combinations of the separate effects. As an instance, let us consider four specimens of high-speed steel, alike except as to heat treatment:

A is quenched from 1300° C., and drawn to 550° C.
B " " " 1300° C., " " " 600° C.
C " " " 1200° C., " " " 550° C.
D " " " 1200° C., " " " 600° C.

If we plot a magnetic quantity, x, of each piece, against a magnetic quantity, y, of each piece, it will be seen at once whether these quantities are interdependent or not. Suppose for A, $x=0$, $y=6$; for B, $x=2$, $y=4$; for C, $x=1$, $y=5$; and for D, $x=3$, $y=3$. Plotting these, as in Fig. 1 (*a*), it will be found that all the points lie on a straight line; therefore the measurements are not independent. If the values for A are, $x=0$, $y=6$; for B, $x=2$, $y=4$; for C, $x=3$, $y=6$; and for D, $x=5$, $y=4$, it will be found, as shown in Fig. 1 (*b*), that C and D are in a different category from A and B. If it were then shown that all specimens quenched from 1300° C. and drawn between 550 and 600° C. lie along the line AB, and all quenched from 1200° C. and similarly drawn lie along the line CD, and that the value of y varies with the drawing temperature, the intermediate position of any point such as F may be expected to indicate the quenching and drawing temperature of a corresponding specimen. Inspection can thus be carried out in regard to the two variables under discussion, using the four known specimens to give the necessary points of reference. This follows regardless of the actual shape of the plot, or whether the lines of equal quench and equal drawing temperature are straight or curved.



(*a*) *X* and *Y* Measurements not Independent (*b*) *X* and *Y* Measurements Independent

FIG. 1.—Illustrating Plots of Magnetic Measurements

THE TESTING APPARATUS

In Fig. 2 is shown diagrammatically the wiring diagram for one form of apparatus employed in the tests. *T* represents a test solenoid arranged to receive the specimen under examination. This solenoid is energized from a source of variable electromotive force *E*, through the switch S, variable inductance I, variable resistances R and R_1 and switch S_1. As shown, switch S_1 is arranged to allow the successive use of two independent sliders L and L_1, both of which coact with the resistance R_1. Ratio coils B and B_1 are shunted across solenoid T through switch S_1. Bridging the two parallel circuits formed by T and R_1 and B and B_1 is the moving coil M of a galvanometer G, which for the present we will consider as a pointer instrument. In series with M is a condenser C which may be short-circuited by the key K, and bridged across the terminals of M are resistances r and r_1, so connected with respect to a switch as to permit the adjustment of the galvanometer sensitivity under two conditions. The field winding F of the galvanometer is wound upon a magnetic core H and is excited from the source E through the variable inductance I_1 and variable resistance R_2. A and A_1 are ammeters located in the test coil and galvanometer field circuits, respectively. As will be seen, the test solenoid forms one arm of a bridge circuit, the other three arms of

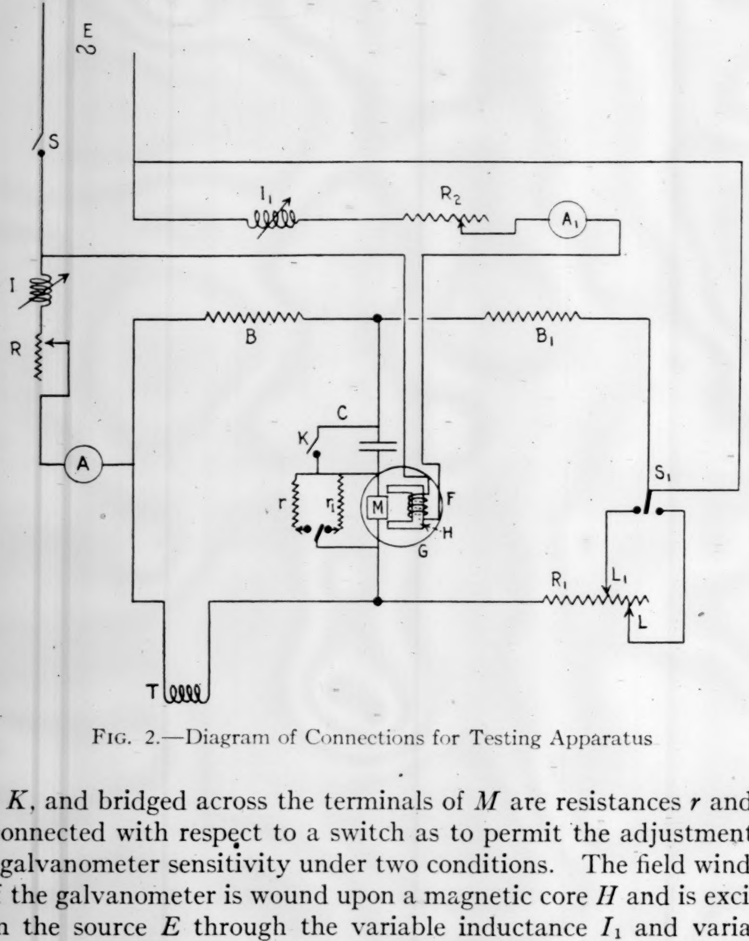


FIG. 2.—Diagram of Connections for Testing Apparatus

which are B, B_1 and R_1. All switches are operated from the same handle, and make the proper connections for the X or the Y circuits, that is, the circuit with and without the condenser.

Manipulation of Apparatus.—In the use of the apparatus shown in Fig. 2, a standard specimen of known heat treatment is inserted within the solenoid T and switch S_1 closed so as to connect with slider L. Key K is now closed so that condenser C is short-circuited. Switch S is closed and the currents in the test solenoid and galvanometer field circuits adjusted to desired values by means of resistances R and R_2. The deflection of the galvanometer G, which is normally observed, may be brought to any desired value on the scale by combined manipulation of slider L and resistance R. The final value as adjusted may be defined for the present as the X reading. Switch S_1 is now closed so as to connect with slider L_1, key K is opened so as to place C in series with M and the galvanometer deflection brought to its desired value by the combined manipulation of slider L_1 and resistance R_1. This second galvanometer deflection is designated as the Y reading. These two galvanometer deflections are indicative of different combinations of two different magnetic properties of the standard specimen and subsequent readings with test specimens are to be referred to these values as standard.

The use of condenser C, as when placed in series with M or short-circuited, operates to cause a variation in phase relation between the current in the moving coil circuit and that in the galvanometer exciting field. This variation in phase relation and the modification in wave form which accompanies it, allows a desired discrimination between two different magnetic properties, and hence structural properties, of the material in T.

While, as described, a single galvanometer may be used to obtain both the X and Y readings, two similar galvanometers, one of which is in series with a condenser, may be employed and thus the necessity of taking two successive readings avoided. A method utilizing two galvanometers has been successful in the testing of continuous lengths of rods and wires. It may be remarked in this connection that variations in speed of feed do not affect the results. Indeed, trial shows that the method may be used in connection with automatic forming machines where the feed is intermittent.

The use of a single reflecting galvanometer, which permits reading from a spot of light, offers many practical advantages and in Fig. 3 is shown an arrangement that has been found very useful. D is a light source which is deflected by the mirror O to the galvanometer mirror M' and thence, by means of the mirror P, to the coordinate paper *W*. *P* may be rotated by hand about its horizontal axis, by means of the knob *U*, so as to adjust the light spot along the *Y* axis of the coordinate paper. If then the circuit be closed, with the condenser in, that is to say, in obtaining the *Y* reading, a deflection is observed indicating the condition of balance in the circuit under these conditions. The position of the spot of light may be at 1 on the plotting paper in Fig. 3. This measures a distance *y* which, however,

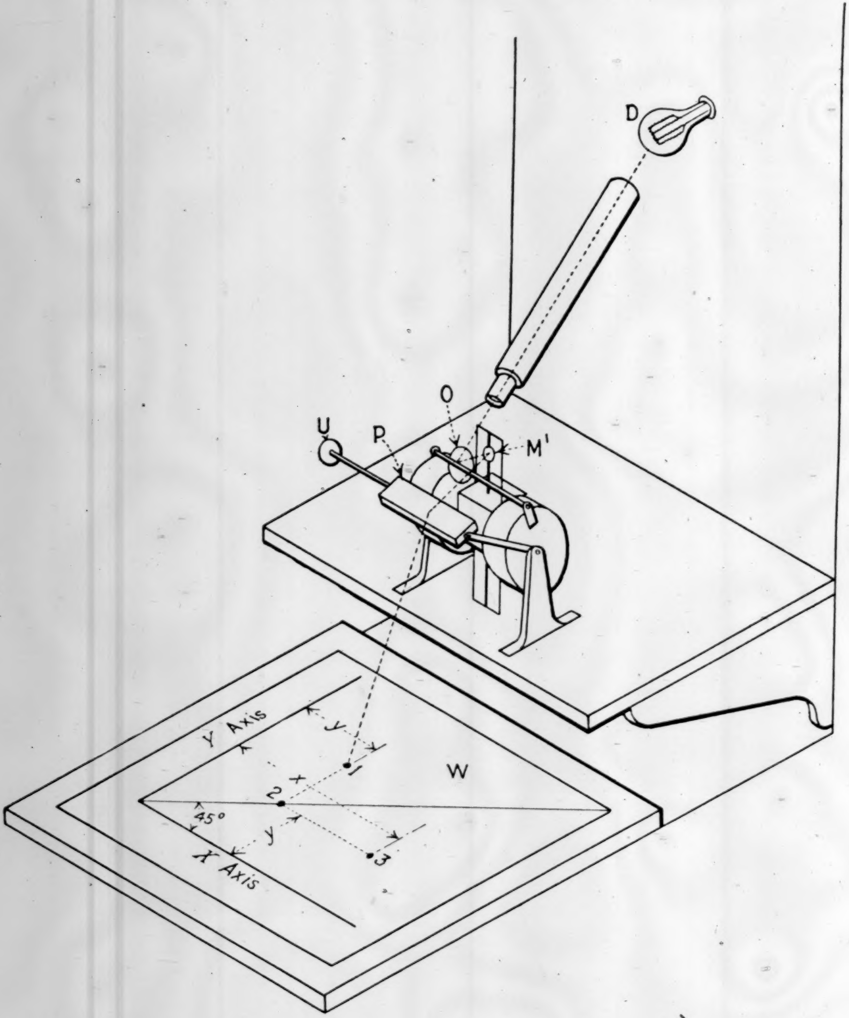


FIG. 3.—Illustrating Use of a Reflecting Galvanometer

being traced parallel to the *X* axis, is in the wrong direction for measuring the *Y* coordinate. By the simple device of moving by hand the mirror *P*, the spot of light is made to travel parallel to the *Y* axis from 1 to 2, the position at which it intersects the 45-deg. line bisecting the angle formed by the *X-Y* axes. The distance from 2 to the *X* axis is now equal to *y*, measured as is necessary along the *Y* axis. Without moving the mirror *P*, the *X* circuit is then connected, short-circuiting the condenser, and the resulting deflection which carries the spot of light to 3 is the *X* reading, along its proper axis. The position of the spot of light may now be recorded by a pencil mark on the paper which constitutes the plot. The coordinates of this spot, *x* and *y*, are the measurements which it is desired to plot against each other, in their proper relation.

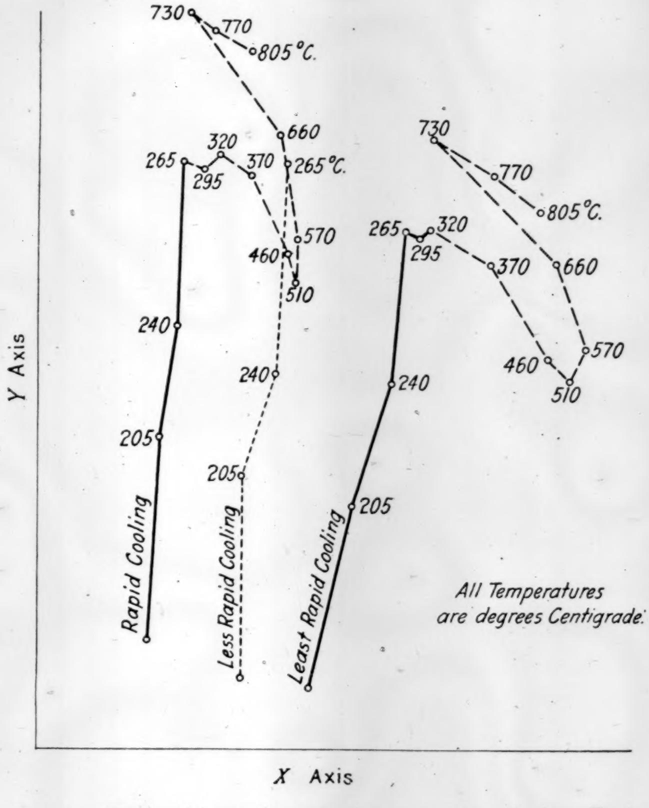


FIG. 4.—Plot of Magnetic Data for 1-per-cent Carbon Tool Steel, Quenched from 810° C. at Three Rates of Cooling, and Drawn to Temperatures Indicated. Specimens $\frac{1}{2}$ by $\frac{3}{8}$ by 4 in.

Applications of the Method

Carbon Tool Steel.—In Fig. 4 are shown the changes in a 1-per-cent carbon tool steel, plotted according to the method described, accompanying three different rates of cooling from 810° C., with similar successive drawing temperatures. The dimensions of the specimen are $\frac{1}{2}$ by $\frac{3}{8}$ by 4 in. The drawing temperatures for the intermediate rate of cooling are not shown above 265° C., but for the most rapid and least rapid rate the drawing temperatures are plotted in dotted lines up to 805° C. The plot shown is intended for actual use only below the drawing temperature of 265° C., at which point a new metallographic constituent of the steel begins to exert a predominating influence. If inspection is to be carried out above this point, a readjustment of the apparatus is necessary to prevent confusion between the different effect of rate of cooling and temperature of draw. The method is most applicable to steels in the same metallographic state, that is, martensitic, troostitic, sorbitic, or pearlitic, corresponding to the four branches of the curves as drawn for the highest and lowest cooling rates in Fig. 4. A hardness test will readily distinguish between the states.

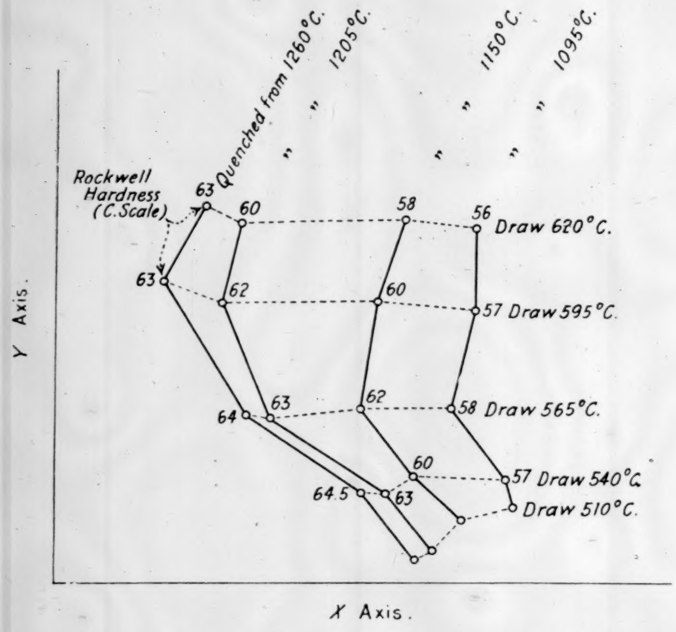


Fig. 5.—Plot of Magnetic Data from 18-per-cent Tungsten High Speed Steel, Showing Effects of Quenching and Drawing. Specimens $\frac{3}{8}$ by $\frac{3}{8}$ by 3 in.

Regarding the steel of most rapid quenching and drawn within the range 265 to 510° C., it may be remarked that the use of the device shown in Fig. 3 permits us to coordinate any particular drawing stage so that, for example, a line drawn so as to join these points shall be in general parallel to the *Y* axis. It is possible further to manipulate the variable inductances I and I_1, and variable resistances R and R_2 of Fig. 2, so that specimens alike as to one variable, as quenching, shall have the same *X* reading regardless of the drawing temperature. The *Y* reading then indicates variation in the drawing temperature.

High-Speed Steel.—The plot shown in Fig. 5 illustrates *X* and *Y* readings for high-speed steel bits, $\frac{3}{8}$ by $\frac{3}{8}$ by 3 in., quenched from different temperatures and drawn as indicated. Figures of Rockwell hardness are also given. In this case the specimens were quenched from the liquid bath used by A. E. Bellis. An attempt at the usual method of hardening led to discordant results. All drawing was done in air, and the specimens held for 30 minutes. The time element in drawing appears to be very important, especially below 595° C. In this case, the plot is prepared for the region between 565 and 620° C., drawing temperature. By a proper adjustment the area from 510 to 565° C. could have been made to approximate rectangular coordinates.

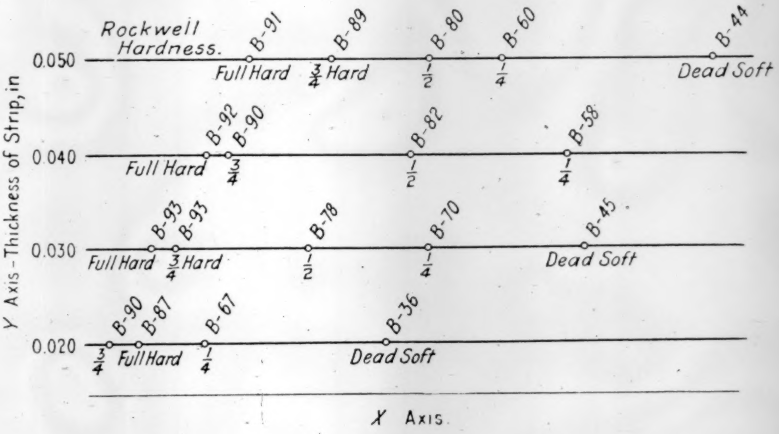


FIG. 6.—Showing Plot of Magnetic Data from Cold-Rolled Strip Steel. Carbon, 0.10; Manganese, 0.45; Phosphorus, 0.02; Sulfur, 0.05 per cent

Cold-Rolled Strip Steel.—A different use of the apparatus is illustrated by its application to cold-rolled strip steel. In this case, the chief variables are the thickness of the stock and the degree of cold rolling, or the percentage reduction since the last anneal. This is not very accurately designated as full hard (corresponding to the maximum cold work which it is practical to give the steel without injuring it), three-quarter, half and quarter hard, and dead soft, or annealed. The tensile strength, and hardness, as measured by Rockwell or scleroscope, increase with cold working and the elongation decreases. The Rockwell measurement is found to indicate pretty closely the tensile strength.

In Fig. 6, the *Y* axis is used for the thickness variation, and the adjustable mirror is set by hand so that the spot of light lies on a line parallel to the *X* axis, corresponding with the thickness of the stock. A single magnetic reading is adjusted by means of the variable inductances and the use of a suitable test coil so that the magnetic effect of changes in cold rolling is a maximum. In other words, the adjustment of the phase relation of galvanometer field to moving coil is such as to give the greatest differences between specimens varying in cold work. In the case illustrated, the thickness then had a relatively small effect between 0.030 and 0.050 in. Below 0.030 there is a loss in sensitivity. This can easily be readjusted if desired. The magnetic reading along the *X* axis is shown for the different degrees of rolling and the corresponding Rockwell hardness numbers are given. While the correspondence is not exact on account of the small number of specimens represented, it indicates a close relationship. The 0.020-in. strip is interesting in that the "¾ hard" on both Rockwell and magnetic is "harder" than the full hard. In the tension test, ¾ hard gave 104,700 lb. per sq. in. tensile strength and full hard 96,200 lb. per sq. in. Here the magnetic and physical properties agree in showing that the amount of cold work does not always give a correct idea of the corresponding physical properties.

A similar situation is encountered in testing small case-hardened chain as used for automobile skid chain. Here the factors to be measured and guarded against are brittleness at one end of the scale and softness at the other. It is immaterial from the point of view of acceptability whether the chain is soft because of insufficient depth of case or improper quench. Both will read soft. At the hard end, a coarse structure with a moderate case reads higher than a similar case of normal structure. Hence, the factors which make for brittleness read in the same direction and make a single measurement sufficient. If all other factors are constant, the magnetic reading is proportional to the depth of case. In this machine the sensitivity is adjusted and the zero shifted by changes in the circuit to give the same acceptance limits for all the different sizes. The use of the inspection method has enabled the manufacture of a much more uniformly acceptable product with practically no change in the production equipment.

CONCLUSIONS

A rapid test can be carried out with portable apparatus and commercial alternating current, which can be used for commercial inspection of suitable objects.

A separation can be made between the effects of different variables which enter into the quality of the finished product.

A single measurement can, under favorable circumstances, be adjusted to read the desired combination of several factors influencing a complex property such as strength, hardness or brittleness.

A simple and sensitive method of test is brought to the attention of those interested in studying the metallography and physical properties of steel.

Acknowledgments.—Thanks are publicly due Mr. F. P. Fahy for encouragement and assistance at all stages of this development. To Messrs. A. E. Bellis, A. M. Grossman and C. F. Lewis, I am indebted for their encouragement, heat treatment, and samples.

DISCUSSION

Mr. Burrows

Mr. C. W. Burrows.[1]—I think Mr. de Forest and his collaborators are to be congratulated for this contribution to magnetic analysis. They are emphasizing one point which I believe is vital, and that is that magnetic analysis, to mean the most to the steel industry, is going to make use of more than one magnetic variable. We have sometimes felt that the criterion of magnetic analysis is a single magnetic characteristic. This is true in a particular case, but there is more than one magnetic characteristic. We may even consider that there are a great many and that magnetic analysis is an art and also a science comparable in its magnitude with chemistry. We have been several hundred years in bringing chemistry to the state of perfection it is to-day, and we must expect magnetic analysis to take a few years to place itself upon the map. I believe that when we appreciate the meanings of the various magnetic characteristics, recognizing that there are several characteristics that may be used, just as there are several elements that may enter into a chemical analysis, then we may expect to reach the best there is in magnetic analysis. I believe that eventually we will have magnetic analysis advancing in its use to the steel industry beyond what the microscope is able to give us, just as the microscope has told us the story of steel in a way that even chemistry could not. I wish to express my personal appreciation of the work that Mr. de Forest has been doing. I feel in a way, that magnetic analysis is one of my own children and I have a very kindly feeling toward anyone who pushes forward in the art as Mr. de Forest and his collaborators are doing.

Mr. Fahy

Mr. F. P. Fahy.[2]—I have had the privilege of following from its inception the development of this device of Mr. de Forest. The value of magnetic analysis, as he shows, lies in its ability not only to interpret the final condition of the material under examination, but also to tell in some degree by what stages it has reached that condition. Up to now we have been limited in the number of factors that may be definitely identified by magnetic analysis, but already from Mr. de Forest's work there appears the possibility of greatly increasing the number which can be evaluated. Along the line of Mr. Burrows' remarks I would like to say this in regard to magnetic testing in general:

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[2] Consulting Engineer, 50 Church St., New York City.

Mr. Fahy

I am greatly in sympathy with Mr. de Forest's idea of bringing the apparatus here that interest might be further stimulated. Talks with a number of people in regard to the possibility of magnetic analysis being of real use to them, have shown the prevalence of the opinion that it is a very special subject and involves great difficulties in application. This is not altogether true. While some applications may offer complications, I have no hesitation in saying that very frequently the simplest form of magnetic testing will be of great informational value. There are many organizations represented here which are equipped to start work of this sort both as to personnel and laboratory facilities.

As chairman of Committee A-8 on Magnetic Analysis, I ask the members present, as far as they can and as far as their interests bear on the point, to examine the apparatus which Mr. de Forest has upon view, to see it in operation and to discuss with him some of these matters. I think he will bear me out that, basically at least, there is nothing very complicated about the application of magnetic analysis. If magnetic analysis lags at all, it lags because there are not enough people working upon it. There should be a great many more than at present.

Mr. Spooner

MR. THOMAS SPOONER.[1]—I had not intended to discuss this paper, because I know so little about the subject. I have seen Mr. de Forest's apparatus work and have set up two or three of his bridge circuits. Judging from the small amount of work which we have done, the problem is not as simple as Mr. de Forest would lead us to think. So far our results have not been altogether satisfactory, but in the hands of an expert like Mr. de Forest, the apparatus is capable of yielding very remarkable results. As he says, the matter of checking drawing temperatures is very simple, since almost any kind of set up will do that. It is much more difficult, however, to check quenching temperatures accurately and it is there that we have had our difficulties so far. I am sure, nevertheless, that when we know more about Mr. de Forest's methods, they will find many valuable applications.

Mr. Outerbridge

MR. A. E. OUTERBRIDGE, JR.[2]—I merely wish to express my appreciation of this most interesting subject which has been presented in an ideal way before us. No one could have listened to Mr. de Forest without believing that he has commenced a new method of magnetic analysis which has untold possibilities. About twenty years ago it fell to my lot to be in charge of the heat treatment of cutting tools under the Taylor-White process, long before the discovery

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[1] Research Engineer, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.

[2] Junior Metallurgist, William Sellers and Co., Inc., Philadelphia, Pa.

Mr. Outerbridge of these modern high-speed cutting tools. In those days great effort was made to have the heat treatment alike in a certain batch of tools. There were nine different formulas presented to the concern that purchased at a large price the shop rights to use the Taylor-White process. All the tools in one batch were from one steel stock, they were all treated, so far as we could tell, absolutely alike. They were heated to the "bleeding point" of the tool, which, prior to that time, was supposed to be enough to ruin it, plunged into a bath of molten lead (the temperature of which was kept at a uniform point by means of a special pyrometer system devised for that purpose), kept in the bath for a certain length of time, taken out and air-cooled and drawn, as you might say, then ground and put into work. There was so great a variation between those tools that it was impossible to form any opinion as to what the product would be when it was put into practical use. I can see that here is a new method of determining the uniformity of the heat treatment of any special lot of tools.

Mr. Styri MR. HAAKON STYRI.[1]—Perhaps Mr. De Forest's success has been due to his having very homogeneous structure in the material examined, for I think he would have inconsistent results with material that varies in structure from the surface to the center. If he had used carbon steel, for instance, of various dimensions, he might be able to get a perfectly uniform martensitic structure throughout in smaller sections, but in a heavy section, martensite at the surface and troostite in the center. All who have been dealing with heat treating know that carbon steels under certain conditions are extremely difficult to get uniform. This is important to everybody who works with magnetic analysis. We have tried it in our laboratory and I am quite sure that one reason for getting rather poor results is that we have not been able to get perfectly uniform structure throughout. On alloy steels it might be much easier.

Mr. de Forest MR. A. V. DE FOREST.[2]—In regard to uniformity from the surface toward the interior, I have not done very much. It is perfectly possible to conceive of performing this test at various frequencies of exciting current which would give various depths of magnetization and therefore analyze the different layers of the tool successively. As yet I have merely taken the 60-cycle commercial circuit and used the penetration and the distribution that that gave. That lumps the entire cross-section in a more or less conventional way. So far we have only compared tools of the same dimensions which were treated in different ways, and in doing that could tell whether the quenching temperature had been more or less than standard and whether the drawing temperature had been more or less than standard, without regard to the depth at which the martensite changed to troostite. The influence of a change of steel and size of stock is immense, but we have not gone into analyzing that as yet. It is, however, theoretically a very simple job to start at the surface and go right through the cross-section, measuring differences from one layer to another, but that as yet has not been worked out. **Mr. de Forest**

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[1] Chief of Research Laboratory, S. K. F. Industries, Inc., Philadelphia, Pa.

[2] Research Engineer, American Chain Co., Bridgeport, Conn.

ACCELEROMETER FOR MEASURING IMPACT

By Earl B. Smith[1]

The design and construction of an instrument that will indicate with reasonable accuracy the rate of change in velocity or the acceleration of a moving mass makes possible the solution of most engineering problems where the force exerted by impact is required. The force of impact in most cases can not be measured directly as can be done by weighing a static force; and any device intended to receive the impact and indicate its value in pounds may not only be cumbersome, but will introduce conditions in the force system that will change the force value from what it would be otherwise. The force resulting from the action of a moving mass or from an impact is a product of mass and acceleration, and the most convenient and direct method of determining this impact value is by means of an accelerometer that may be attached directly to the moving body or hammer and which will indicate the acceleration value. A brief description of such an accelerometer is given in this paper.

The essentials of a small accelerometer are shown diagrammatically in Fig. 1. Within a rigid frame is mounted a steel beam, *B*, supported only at the two ends. At the center of the beam is attached a mass, *W*. A toggle, *C*, is provided which will hold the beam at any maximum deflection position until the amount of deflection may be measured with the micrometer dial. In use, the accelerometer is rigidly attached to the moving body or hammer, with the plane of the beam, *B*, at right angles to the direction of motion or to the line of action of the force to be determined. When the velocity is constant there will be only the static deflection of the beam, but when the velocity is changed and the acceleration is negative the beam will be further deflected because of the inertia of the mass, *W*; that is, under the influence of a deceleration (negative acceleration) the mass, *W*, will exert a force, *F*, which will deflect the beam.

The action of the accelerometer may be analyzed by the use of the fundamental relation, $F = Ma$.

Let a = acceleration,
w = weight of W,

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[1] Senior Assistant Testing Engineer, U. S. Bureau of Public Roads, Washington, D. C.

M = mass of the acting weight $\left(M = \frac{w}{g}\right)$,
F = resulting force,
g = 32.2,
d = static load deflection of beam in inches,
P = static force required per unit deflection,

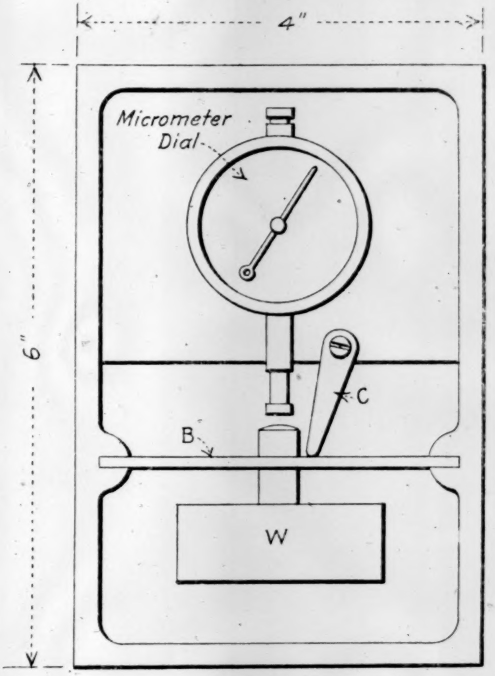

FIG. 1.—Diagrammatic Scheme of Accelerometer

D = deflection of beam under impact, and
K = coefficient of calibration.

Under the influence of a deceleration, the mass, W, will produce a force on the beam as shown by

$$F = Ma = \frac{w}{g}a\text{; transposing, } a = \frac{Fg}{w}$$

Since the value of w is easily determined by carefully weighing, and with which is also included approximately one-half of the weight of the beam, it is only necessary to determine the value of F to solve for the deceleration, a.

The deflection, d, of the beam, B, is an indication of the static force, Pd, applied; therefore, from a static calibration of the beam, obtaining the load-deflection curve, this force value may be obtained. However, the effect from the action of the mass, W, is not as a static load when it is the result of a deceleration; it is more nearly as a suddenly applied load, and the effect in causing deflection of the beam is $2d = D$. Therefore, after an impact the deflection as shown by the micrometer dial indicates an equivalent static force of

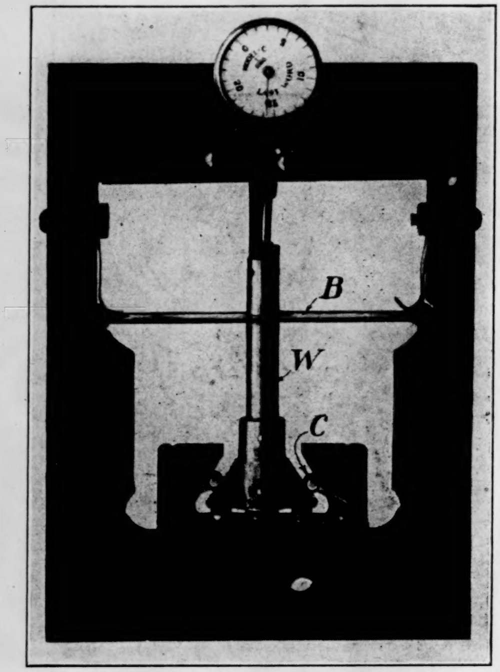

FIG. 2.—Accelerometer, with Dial Measurement

$$F = \tfrac{1}{2}PD$$

Then, as the beam deflection is an indication of the force applied under the dynamic action of the attached weight, and since all other factors are known and constant, the deflection multiplied by a coefficient is an indication of the value of the deceleration, as given by the equation

$$a = \frac{Fg}{w} = \frac{PDg}{2w} = KD$$

This shows the basic principle and the mathematical theory of the accelerometer, but the mechanical operation and the accuracy of its indications depend on additional factors. By properly proportioning the beam and its attached mass, *W*, the range of the instrument may be made suitable for most any desired condition. The deflection of the beam, however, must be restricted to a very small limit, unless the acceleration to be indicated is comparatively small and persists for a time much greater than the period of vibration of the beam. The importance of this factor may be appreciated by considering the fact that the beam must respond quickly enough to accomplish its full deflection under the action of the acceleration; and this may be accomplished only with small allowable deflections.

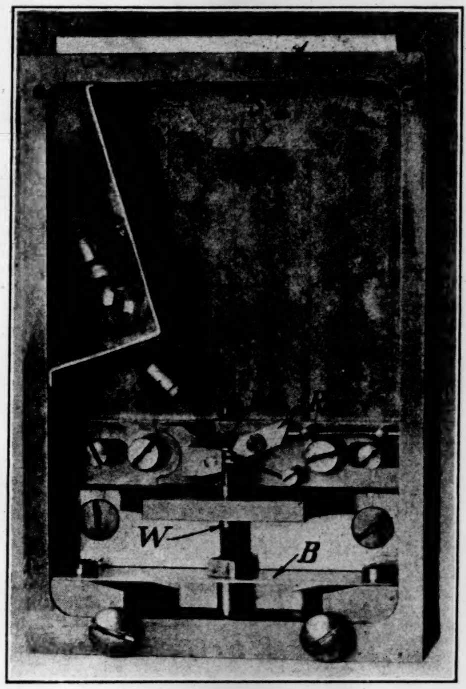

FIG. 3.—Improved Accelerometer, with "Optical Lever" Multiplication

The maximum deflection of the indicating beam is kept within about 0.015 in., and the exact amount is read from the micrometer dial, or by means of an "optical lever" system.

The accelerometer shown in Fig. 2 is one of very simple construction. The maximum deflection of the beam, *B*, is held in position by two rollers, *C*, each acting between two surfaces at slightly different angles as shown. The dial stem is pressed in contact with the upper part of *W* only at the time the indication is desired, and is not in contact and the dial does not operate during the time of deflection of the beam; this is a necessary provision to prevent damage to the dial and a change in the force conditions acting on the beam during the period of deceleration.

An improved design is shown in Fig. 3. It was found desirable to greatly reduce the maximum allowable deflection of the indicating beam. The beam, *B*, in this instrument has fixed end supports, and a maximum deflection of 0.003 in. This small deflection is measured by the movement of a beam of light giving a magnification of about 800. The stem, *W*, which is also the weight attached to the beam, operates a small oscillating mirror at *M*. The light from the small electric lamp is focused by the lens, *L*, onto the small mirror and reflected to the scale *S*. At *R* is a stop or brake which holds the small mirror in its deflected position after the deflection of the beam until the reading may be made from the scale. A small push rod allows the brake to be released and the mirror returned to the zero position.

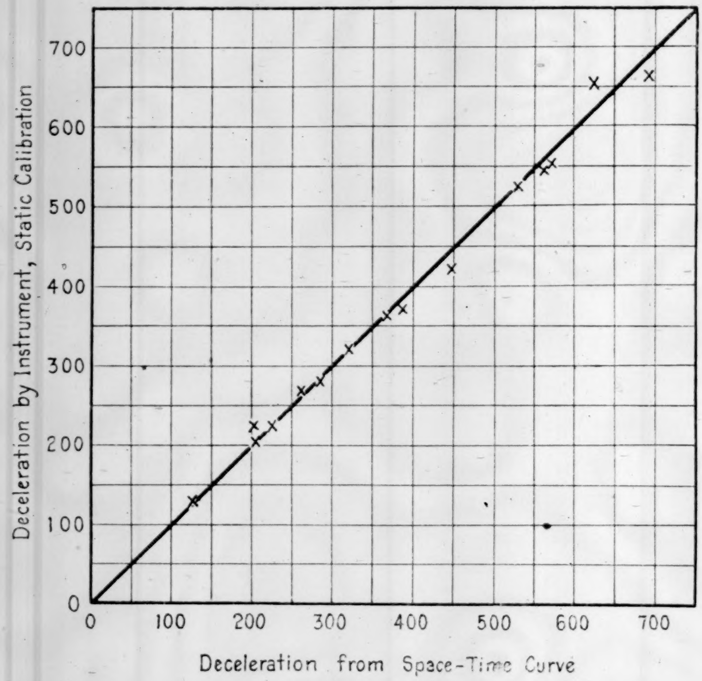

FIG. 4.—Accelerometer Calibration in Feet per Second per Second

These instruments may be calibrated by determining the deflection of the small beam per unit load as indicated by a dial or by the deflection of a ray of light. They may also be calibrated by comparison with a known deceleration as determined from the autographic space-time curve.[1] The coefficient for the accelerometer shown in Fig. 2 is obtained from the data: Weight of plunger, W, including one-half the weight of the beam, B, is 0.114 lb.; static weight necessary to deflect beam 0.001 in. is 0.575 lb. Then

$$K = \frac{Pg}{2w} = \frac{0.575 \times 32.2}{2 \times 0.114} = 81.2$$

And in use the deceleration indications for each 0.001 in. indication, D, on the dial is

$$a = KD = 81.2D$$

The most reliable range of this particular instrument is within the limits of $D = 0.0005$ and $D = 0.005$ in.

The accelerometer shown in Fig. 3, having a more reliable and greater magnification of the beam deflection, is more sensitive and of greater range. The weight of the stem and parts, W, supported on the beam, including one-half the height of the beam, is 0.0249 lb.; the static weight necessary to deflect the beam and move the light ray on scale one inch is 0.650 lb. Then for D, measured in inches on the scale of the instrument,

$$a = \frac{PgD}{2w} = \frac{0.65 \times 32.2 \times D}{2 \times 0.0249} = 420D$$

The results of a calibration of this improved instrument in comparison with the results from a space-time curve obtained at the same time is shown in Fig. 4. Such comparisons and calibrations indicate at least a reliable degree of accuracy. The range of this accelerometer is from about two to twenty-five times the acceleration of gravity.

A photographic recording attachment carrying a roll of film is also provided which may be quickly attached to the top of the accelerometer, by means of which continuous records may be obtained.

These accelerometers were originally designed for use at the U. S. Bureau of Public Roads on auto truck wheels and axles in connection with some special investigations to determine the impact of trucks on roads. With some slight changes in design and proportion of the parts this type of instrument may be adapted to a large variety of impact determinations.

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[1] Earl B. Smith, "Measurement of Impact," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1073 (1921); also, Armin Elmendorf, "Measuring Forces in Impact," *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part II, p. 117 (1922).

DISCUSSION

Mr. Lindau MR. A. E. LINDAU.[1]—I should like to ask how the instrument is applied in practice.

Mr. Smith MR. E. B. SMITH.—We have used it mostly on trucks, and have attached the instrument directly to the axle of the truck. In the case of testing wheels for cushioning effect, it is attached directly to the frame which holds the axle of the wheel. When used on pile drivers, it is simply bolted very rigidly to the hammer itself.

Mr. Lindau MR. LINDAU.—The effect of rough roads, various kinds of tires, etc., is to produce certain impact effects that may be measured as a certain percentage of increased static load. Can Mr. Smith give some indication of the range of those values?

Mr. Smith MR. SMITH.—In connection with work on trucks, the range of the impact has been probably in the low values, around 8000 to 15,000 lb., but some high values have been around 30,000 lb., and in one or two cases we had values as high as 42,000 and 45,000 lb. In comparison with the static load of the truck on which the instrument was used, those values run as an average about five times the static load or mass on which it was connected. That is, if the static load of the wheel was 1000 lb., the impact load would be easily 5000 lb. There are some instances where this ratio runs over seven.

Mr. Lindau MR. LINDAU.—In the use of this instrument, have you any idea of the effect of the road itself on the impact, such as the effect of the character of the road, the difference between a hard surface road and a bituminous macadam or some other type of road?

Mr. Smith MR. SMITH.—There is not much information along that line. We have used the instrument mostly on concrete roads and only a little over bituminous roads, but the comparison of the impact between the two is very difficult because of the uncertainty of securing the same obstruction or condition which causes the impact. If you could be sure of exactly the same condition or cause, a good comparison could be obtained. Perhaps you are thinking of the cushioning effect of bituminous surfaces in comparison with concrete. You must understand that impact forces on roads occur at extremely high rates and that the flow or cushioning effect of bituminous road compounds is very slow in comparison with the period of impact, and probably offers only a little additional cushioning effect. Moreover, in connection with rubber tires the deflection or cushioning is practically all in the rubber.

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[1] General Manager of Sales, Corrugated Bar Co., Buffalo, N. Y.

Mr. Moore MR. H. F. MOORE.[1]—In obtaining the maximum deflection of the spring, does the wear of the instrument affect the back lash of that maximum movement to an appreciable extent?

Mr. Smith MR. SMITH.—No, it does not. The instruments are so constructed that a zero adjustment corrects for any wear that might be either on the connection of the mass on the beam or at the beam supports.

The Chairman THE CHAIRMAN (PAST-PRESIDENT C. D. YOUNG).[2]—The earliest testing of railway materials was largely by the impact method, namely, by drop testing. That is still the case in the testing of rails. At one time couplers, bolts and a great deal of railroad material was tested by impact. Efforts were made to translate the results of those impact tests into force in pounds. I believe it was the consensus of opinion through twenty or thirty years' experience in that work, that it was neither wise nor safe to draw too definite conclusions in the translation of the forces applied through acceleration into pounds loading equivalent to static. A great deal depends upon the character of the apparatus as to what the ratio factor should be in that translation; a great deal depends upon the mass back of the resistance, and as I understand the paper, in this particular case it would be the mass of the vehicle carrying the instrument. In addition to that, the character of the springs would be a determining factor in the translation of those forces into pounds, in the same way that the character of the spring bed supporting the anvil of a drop-testing machine has its effect upon the translation of the falling tup to the effect upon the metal and deflection of the metal. In other words, by changing springs in the anvil or the support of the piece deflected under accelerated drop test, you can decidedly change the deflection for the same height of fall with the same material; so, I feel that where data are obtained through accelerated movement, as in this instance, the attempt to translate into pounds of force should be made very cautiously.

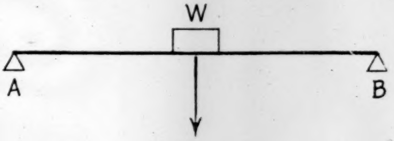

FIG. 1

Mr. Timoshenko MR. S. TIMOSHENKO[3] (*by letter*).—In the paper presented by Mr. Smith, the theory of the accelerometer and the expression for the coefficient of calibration K are given.

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[1] Professor of Engineering Materials, University of Illinois.

[2] General Supervisor of Stores, Pennsylvania Railroad System, Philadelphia, Pa.

[3] Westinghouse Research Laboratory, East Pittsburgh, Pa.

Mr. Timoshenko

It may be shown that the coefficient K must be taken twice as large as it is given in the paper.

In Fig. 1, let x denote the vertical displacement of the weight W calculated from the position of equilibrium, $\eta = \mathrm{f}\ (t)$ —the vertical displacement of supports A and B as a function of the time t, and P the static force required per unit deflection. Then the equation of motion of the weight W^a is

$$\frac{W}{g}\ \frac{d^2 x}{d t^2} + P\left(x - \mathrm{f}(t)\right) = 0 \ldots\ldots\ldots\ldots\ldots (1)$$

Suppose, for instance, the supports perform a simple harmonic motion of the amplitude b and of the period τ. Then

$$\mathrm{f}\ (t) = b \sin \frac{2\pi t}{\tau} \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

and the acceleration is

$$\mathrm{f}''\ (t) = \frac{-b\ 4\ \pi^2}{\tau^2} \sin \frac{2\ \pi\ t}{\tau t}$$

The maximum deceleration to be measured is

$$a = b\ \frac{4\ \pi^2}{\tau^2} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

The period τ_1 of free vibrations of the beam is the same as the period of a simple pendulum whose length is $\frac{W}{P}$, that is, equal to the deflection of the beam produced by the statically acting load W. Then

$$\tau_1 = 2\ \pi \sqrt{\frac{W}{P\ g}} \ldots\ldots\ldots\ldots\ldots\ldots (4)$$

and

$$\frac{W}{P\ g} = \frac{\tau_1^2}{4\pi^2}\ .$$

Substituting this in Eq. 1 and integrating the equation we obtain,

$$x = \frac{b}{1 - \frac{\tau_1^2}{\tau^2}} \sin \frac{2\ \pi\ t}{\tau}$$

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[a] W includes also approximately one-half the weight of the beam.

Mr. Timoshenko

The expression for the deflection of the beam is

$$x-\mathrm{f}\,(t)=\left(\frac{1}{1-\frac{\tau_1^2}{\tau^2}}-1\right)b\sin\frac{2\,\pi\,\mathrm{t}}{\tau}=\frac{\tau_1^2}{\tau^2-\tau_1^2}\,b\sin\frac{2\,\pi\,t}{\tau}$$

Maximum deflection

$$D=\frac{\tau_1^2}{\tau^2-\tau_1^2}\times b\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(5)$$

When the period τ_1 of free vibration of the beam is very small in comparison with τ, we can neglect in the denominator of Eq. 5 the quantity τ_1^2 in comparison with τ^2 and obtain such an approximate formula,

$$D=\frac{\tau_1^2}{\tau^2}\times b$$

or using Eqs. 3 and 4

$$D=\frac{4\,\pi^2\,W\,b}{\tau^2\,P\,g}=\frac{a\,W}{P\,g}$$

The maximum acceleration

$$a=\frac{P\,g}{W}\,D\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(6)$$

is proportional to the deflection D of the beam.

The coefficient of calibration is

$$K=\frac{P\,g}{W}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(7)$$

that is, twice as large as it is given by Mr. Smith. In general, for the motion of supports A and B can be assumed,

$$\mathrm{f}(t)=b_1\sin\left(\frac{2\pi t}{\tau}+\alpha_1\right)+b_2\sin\left(\frac{4\pi t}{\tau}+\alpha_2\right)+b_3\sin\left(\frac{6\pi t}{\tau}+\alpha_3\right)+\ldots\quad(8)$$

in which τ denotes the time of persisting acceleration and α_1, α_2, α_3, . . . phases of component harmonic motions.

The apparatus proposed will give satisfactory results only in the case when the coefficients b_1, b_2, of the series in Eq. 8 decrease very quickly and τ_1 is small in comparison with τ.

Mr. Smith

MR. SMITH (*by letter*).—The analysis presented by Mr. Timoshenko applies in general to forced vibrations which occur under the influence of a periodically varying force. His conclusions are based

Mr. Smith on an approximation of his Eq. 5, which approximation does not apply to all the conditions or limits of use.

If the hammer to which the accelerometer is attached be shod with solid rubber, the resulting deceleration at impact increases very rapidly from zero to a maximum value, and is then constant for a longer time. This is shown in the shape of the autographic curve. The period of vibration of the accelerometer spring beam is high (1500 per second, more or less), and the deceleration value attains its maximum in a time small in comparison with the time of vibration of the beam. This is the condition of "very rapid" increase of deceleration. Under this condition and limitation, the action of the spring beam has a double deflection as under a suddenly applied load. That this deduction and the method of calibration, within the limits of use, as given in my paper are proper, is shown by the close agreement with the results obtained by differentiation of the autographic space-time curves.

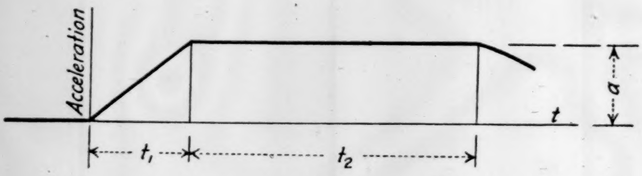

FIG. 2

Mr. Westergaard MR. H. M. WESTERGAARD[1] (*by letter*).—The remarks which follow deal with the matter of the calibration factor of the accelerometer described by Mr. Smith.

Let it be assumed that the upward acceleration whose maximum value is to be recorded varies as is indicated in Fig. 2: the acceleration is zero up to the time $t=0$; from $t=0$ to $t=t_1$ it increases uniformly from zero to the maximum value a; then it remains constant equal to a for a period t_2; and thereafter it decreases gradually. And let it be assumed that up to the time $t=0$ the deflection of the spring due to the dynamic action is zero.

If the acceleration reaches its maximum value relatively slowly, that is, if t_1 is large compared with the period τ of free vibration of the spring, the maximum deflection of the spring will be close to that caused by a static load $\frac{w}{g}a$. On the other hand, if t_1 is small compared with τ, and if the period t_2 of constant acceleration is not too short, the spring will have an overthrow of nearly 100 per cent, and

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[1] Assistant Professor, University of Illinois.

Mr. Westergaard

the deflection will be close to that caused by a static load $\frac{2w}{g}a$. The calibration factors, then, in the two cases are $K=\frac{Pg}{w}$ and $K=\frac{Pg}{2w}$, respectively (P = load causing a deflection equal to one unit; $a=KD$, where D is the maximum deflection). If a series of calibration experiments indicates consistently a factor close to the latter value of K, the explanation appears to be reasonable that under the particular conditions the acceleration varies approximately according to the diagram in Fig. 2, with a small value of t_1 and a not too small value of t_2.

In view of the possibility of a variety of uses of the instrument, and in view of the possibility of large effects of minor changes in the design of the instrument (for example, the change to a still smaller period of vibration of the spring), a discussion of what happens when t_1 is not particularly small compared with τ may be in place.

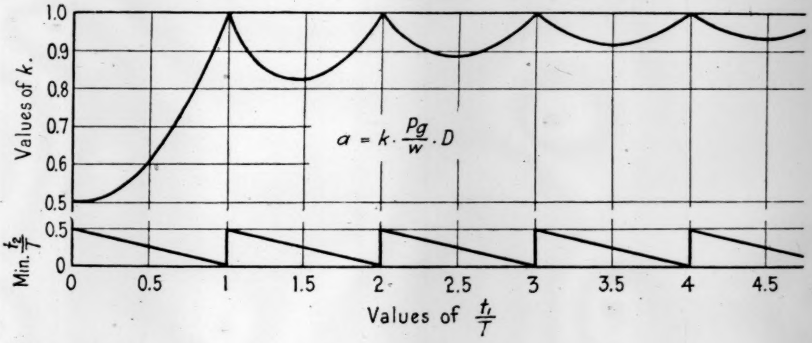

FIG. 3

The calibration factor may be stated in the form

$$K=k\,\frac{Pg}{w},$$

where k is a pure number varying between the extreme values 0.5 and 1.0, corresponding to overthrows of 100 per cent and zero, respectively. An analysis based on the assumption that t_2 is not too small gave the values of k which are shown by the curve at the top of Fig. 3. The curve starts out with a horizontal tangent; with $t_1=0.1\tau$, that is, with the period of increase of the acceleration equal to one-tenth of the period of free vibration of the spring, the value of k is 0.504, deviating less than one per cent from the extreme value 0.5.

Mr. Westergaard

The lower diagram in Fig. 3, the saw-tooth line, shows the least values of t_2 which are required if the full overthrow is to develop before the acceleration begins to decrease. These values of t_2 do not exceed one-half of the period of free vibration of the spring.

Let y be the deflection, positive downward, of the spring at the time t, and let $\mathrm{f}(t)$ be the downward displacement of the supports of the spring at the time t. The total downward displacement of the weight w, then, is $y+\mathrm{f}(t)$, and the weight must move according to the equation

$$\frac{w}{g}\;\frac{d^2(y+\mathrm{f}(t))}{dt^2}+Py=0 \;\ldots\ldots\ldots\ldots\ldots\ldots (9)$$

The values of $\mathrm{f}''(t)$ are given by the diagram in Fig. 2. By introducing these values, Eq. 9 becomes:

during the first period of motion, from $t=0$ to $t=t_1$,

$$\frac{w}{g}\;\frac{d^2y}{dt^2}-\frac{wa}{gt_1}\;t+Py=0 \;\ldots\ldots\ldots\ldots\ldots\ldots (10)$$

and during the second period of motion, after $t=t_1$,

$$\frac{w}{g}\;\frac{d^2y}{dt^2}-\frac{wa}{g}+Py=0 \;\ldots\ldots\ldots\ldots\ldots\ldots (11)$$

The period of free vibration of the spring (corresponding to $\mathrm{f}''(t)=0$ in Eq. 9 is

$$\tau=2\pi\sqrt{\frac{w}{Pg}} \;\ldots\ldots\ldots\ldots\ldots\ldots (12)$$

Eq. 10 and the conditions that $y=0$ and $\frac{dy}{dt}=0$ at $t=0$, are satisfied by the solution

$$y=\frac{wa}{Pg}\left(\frac{t}{t_1}-\frac{\tau}{2\pi t_1}\sin\frac{2\pi t}{\tau}\right) \;\ldots\ldots\ldots\ldots (13)$$

Eq. 11 and the conditions that at $t=t_1$ the values of y and of $\frac{dy}{dt}$ must be those given by Eq. 13, are satisfied by the solution

$$y=\frac{wa}{Pg}\left(1-\frac{\tau}{\pi t_1}\sin\frac{\pi t_1}{\tau}\cos\frac{\pi(2t-t_1)}{\tau}\right) \;\ldots\ldots (14)$$

Eqs. 13 and 14, therefore, are the true equations of the relative motion of the weight w with respect to the supports of the spring.

Mr. Westergaard

The maximum value, D, of y occurs at or after the time $t=t_1$, and is defined, therefore, by Eq. 14. One finds

$$D=\frac{wa}{Pg}\,(1+C)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(15)$$

where

$$C=\frac{\tau}{\pi t_1}\left[\sin\frac{\pi t_1}{\tau}\right]\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(16)$$

The ratio C measures the overthrow of the spring. The calibration constant and the curve at the top of Fig. 3 are defined in terms of C by the equations

$$K=\frac{1}{1+C}\;\frac{Pg}{w}\qquad\text{and}\qquad k=\frac{1}{1+C}\ldots\ldots(17)$$

The values of t_2 shown in the lower diagram in Fig. 3 are obtained as the smallest positive values of $t-t_1$ for which y in Eq. 14 reaches its maximum.

In conclusion it may be stated that the calibration factor $K=\dfrac{Pg}{2w}$ may be used provided the period τ of free vibration of the spring is not too large and not too small; τ should not be larger than twice the period t_2 of constant maximum acceleration and should not be smaller than about ten times the period t_1 of increase of the acceleration. Accordingly, t_2 would have to be at least five times as large as t_1. The suggestion may be ventured, however, that even if t_2 is less than five times t_1, if one is certain that the character of the acceleration diagram is as shown in Fig. 2, one may determine by Eq. 14 the values of both t_1 and a; to do so there should be obtained two deflections by using two different accelerometers with different periods of vibration of the spring.

Mr. Smith

MR. SMITH (*by letter*).—The mathematical analysis which Mr. Westergaard has shown in his discussion clears up some very important points relative to the constant which should be used in calibrating a spring accelerometer. It explains why in some cases the coefficient may vary from 0.5 to 1. In the physical calibration of these instruments, it was noticed that when using accelerometer springs of different stiffness and measuring the same acceleration, the constant K would in some cases have considerable variation. The value of t_1 may in some cases be determined from the autographic space-time curve and the period τ of free vibration of an accelerometer spring may readily be computed. In this way, it is possible to determine the ratio $\dfrac{t_1}{\tau}$.

A CONSTANT LOAD RATE TESTING MACHINE FOR TEXTILES

By G. B. Haven[1]

The increasing use of fabrics for mechanical purposes and the substitution of heavy cotton duck for many other engineering materials, such as leather, steel, and rubber, have brought into prominence the testing of textiles during the last ten years to an increasing degree. Many yarns and fabrics are now purchased upon just as accurate specifications for strength as would be the case with iron and steel. Simultaneously there has naturally arisen a demand for the more accurate testing of fabrics and it is in relation to this question that this paper is written.

For a long period the testing of textiles has had but little scientific value inasmuch as it was carried out in haphazard fashion with but little regard to the actual condition of the fabric and with very great errors in preparing the samples and in making the strength determinations in the testing machine. About ten years ago it was firmly impressed upon the American Society for Testing Materials and the U. S. Bureau of Standards at Washington, that, if textile tests were to be considered of importance, they must be carried out under very exact atmospheric conditions. Cotton is highly hydroscopic, that is, it automatically gathers to itself a large percentage of its weight in the form of moisture. The same statement might be made in regard to practically all textile fibers. This moisture absorption makes a vast difference in the strength of the product, sometimes as much as 50 or 60 per cent. Therefore a "standard" atmosphere has been universally adopted for textile testing and it is no longer expected to obtain accurate results without limited and controlled atmospheric conditions.

In testing materials such as iron or steel, where the stretch is comparatively small, the question of pulling speed is of relatively little importance. As a matter of fact in order to produce the great loads necessary in testing iron and steel the machine speed is limited to a very slow movement. In textiles, however, where the loads are rarely more than 800 lb. the speed of the pulling jaw is of the utmost importance. Textiles do not break like homogeneous materials such as iron or steel. In the latter all the fibers at a given cross-section are ruptured by the test. Textiles are made up of numerous fibers interlaced and at rupture a certain portion are broken, while a larger percentage are disentangled and slip by one another without breakage. When one considers this fact it is perfectly evident that the element of time plays a very important part in the apparent strength of any textile. To emphasize this fact the following experiment was performed: Groups of five specimens each, of an 8-oz. cotton duck with 45 threads in the warp and 32 in the filling per inch, each 3-ply, were selected and conditioned under a temperature of 71.4° F. and a humidity 41.8 per cent relative to saturation. These were tested in a 150-lb. vertical Scott testing machine, with a variation in the speed of the pulling jaw ranging from 2.3 in. per minute to 24 in. per minute, as shown in the accompanying Table I. The results make it perfectly evident that in this class of fabric a variation in the tensile strength of 10 per cent may be secured between the extreme limits given in Table I. A similar series of tests was also carried out on paper-makers' felt and with this stretchy fabric the variation is a little over 20 per cent.

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[1] Professor of Machine Design, In Charge of Textile Laboratory, Massachusetts Institute of Technology, Cambridge, Mass.

TABLE I.—EFFECT OF JAW SPEED UPON APPARENT TENSILE STRENGTH OF FABRICS

Specimen	Jaw Speed, in. per minute				
	2.3	6	12	18	24
8-oz. COTTON DUCK					
No. 1	39.5	42.5	40.0	45.0	46.0
No. 2	44.5	46.0	43.5	44.5	46.0
No. 3	40.5	40.5	45.5	47.0	47.0
No. 4	39.5	44.0	47.5	43.0	45.0
No. 5	44.5	45.0	44.5	45.0	46.5
Average	41.9	43.6	44.2	44.9	46.1
PAPER-MAKERS' WOOL FELT					
No. 1	36.0	41.0	41.5	40.0	43.0
No. 2	34.0	38.5	48.0	41.0	51.0
No. 3	37.5	44.5	38.5	43.5	51.0
No. 4	40.5	32.0	42.5	43.0	38.0
No. 5	41.0	45.5	38.0	44.0	44.0
Average	37.8	40.3	41.7	42.3	45.4

From the above experiment it is clear that either some definite rate of load application must be universally adopted or else some testing machine must be perfected in which the elasticity of the fabric exerts no influence whatever upon the rate of load application.

To make this statement more concrete, a vertical fabric testing machine, set up in the usual manner with a jaw speed of say 12 in. per minute, might rupture a steel tape in five seconds. A piece of balloon fabric might consume eight seconds, a piece of belting duck ten seconds, a piece of tire fabric fifteen seconds, and a piece of papermakers' felt might require twenty seconds in breakage. The above specimens of steel tape and fabric might be so chosen in width and material as to be of precisely the same gross tensile strength when tested under a quiescent or dead load.

From the above it is evident that the elasticity or stretchiness of the specimen determines the time elapsing between the application of the load and the instant of rupture. A very stretchy specimen will consume many seconds before rupture takes place, while a steel tape with its characteristic rigidity would be broken in a very short interval of time. Therefore, to carry out accurate tests upon textile materials, some form of testing machine should be devised in which the elasticity of the material exerts no influence upon the rate of application of the load.

From time to time testing machines have appeared in which the principle of load application consisted of imposing a dead weight upon the specimen by pouring fine shot, sand or water into a receptacle. The size of the orifice determined the amount of weight added uniformly per unit of time. In this manner the Avery machine of England was perfected and used, and similarly cement-testing machines have been built in this country where buckets of shot are used to load the specimen. Such machines have never been used extensively, chiefly because of the slowness of their operation and the liability to untidyness due to spilling the shot, sand, or water. For many years, therefore, textile testing machines in this country have consisted essentially of two types: spring machines and dead weight or "pendulum" machines. In the former, a nicely calibrated spring is used to measure the load. Naturally springs are subject to oscillation and eventually permanent extension, so that such machines have not in general been regarded as accurate. A very large percentage of all the machines used at present consists of dead weight or pendulum machines. The principle of operation is illustrated in Fig. 1. A pendulum, having a radius *R* and weighing *W* lb. is nicely pivoted upon ball bearings at *O*. The upper end of the pendulum is bound to the cylindrical drum *D* around which a chain *C* is wrapped. The lower end of the chain *C* is pinned to the upper jaw *J*. The lower jaw *K* is attached to a screw and nut, the latter being actuated by a gear train in such a manner as to give a speed of the pulling jaw *K* of 12 in. per minute. The figure just mentioned has been recommended as standard by the Bureau of Standards but has as yet been by no means universally adopted. Certain specifications in this country at present read as low as 3 in. per minute. Others in England are set at 18 in. per minute, while 20 and 24 in. per minute have been widely used among manufacturers of tire fabric.

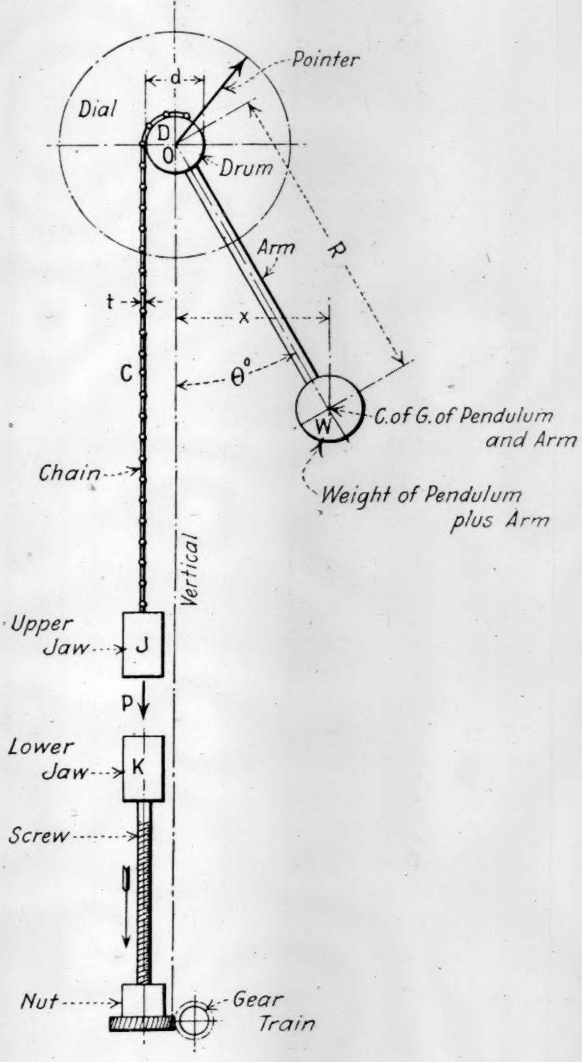
Pointer
d
Dial
D
O
Drum
Arm
R
t
x
C
θ°
W
C. of G. of Pendulum and Arm
Chain
Weight of Pendulum plus Arm
Vertical
Upper Jaw
J
P
Lower Jaw
K
Screw
Nut
Gear Train

FIG. 1.—Diagram Showing Principle of Operation of Pendulum Type of Testing Machine

Of course if the jaws J and K were rigidly connected by, say, a steel specimen, the full speed of the pulling jaw K would be immediately transferred to the drum V and through it to the finger on the dial. It has been proposed by numerous writers upon this subject to define the fastest rate at which a rigid specimen could possibly cause the reading to reach a given figure as the "machine rate of loading." This could be made constant for a rigid specimen and might be, as proposed, 250 lb. per inch of movement of the upper jaw J. On the other hand, the rate at which the load was actually applied to a given specimen, depending as pointed out above upon its stretch, would vary widely with the different character of specimens. This has sometimes been defined as the "fabric rate of load" and is indicated by the load imposed upon the actual fabric per unit of distance traveled by the pulling jaw K. The fabric rate of load cannot be expressed as pounds per inch or pounds per second since it would presumably vary widely between two specimens of the same fabric. At present, machine rates of load vary from 60 to 500 lb. per inch of travel of jaw J, while the fabric rate of load would easily vary three or four hundred per cent with the character of the various fabrics tested. It is therefore evident that, while such conditions exist, there must be wide discrepancies in the apparent strength of textile materials and any effort made to eliminate such difficulties should command immediate and careful attention.

Many attempts have been made to show the best angle through which the pendulum of a textile testing machine should swing in order to make the machine rate of loading as uniform as possible. Let Fig. 1 represent the elements of the testing machine of this class. The meaning of the various parts is evident.

Let the following nomenclature also be assumed:

$C.\,G.$ = Position of center of gravity of pendulum and arm;
W = Weight of pendulum plus arm in pounds;
d = Net diameter, in inches, of drum D;
t = Net thickness, in inches, of chain C;
$d + t$ = Effective diameter, in inches, of drum D;
P = Pull between jaws J and K, pounds;
x = Horizontal displacement, in inches, of $C.\,G.$ from vertical;
θ = Angular displacement, in degrees, of pendulum from vertical.

From Fig. 1, taking moments about the axis of drum D,

$$Wx = \frac{d+t}{2}P$$

But $\sin\theta = \dfrac{x}{R}$

Therefore $x = R\sin\theta$

And $WR\sin\theta = \dfrac{d+t}{2}P$

Solving, $P = \dfrac{WR\sin\theta}{\dfrac{d+t}{2}}$

or

$$P = \frac{2WR\sin\theta}{d+t}$$

To compensate for the friction of the various parts a coefficient k is sometimes included in this numerator, the value of the same to be determined in calibrating the machine. From this it is evident that the load applied by machines of this character is proportional to the sine of the angular displacement of the pendulum. It is very desirable, as pointed out above, to have the machine rate of loading as nearly uniform as possible. A uniform machine rate of loading would mean that the linear movement of the head jaw J, Fig. 1, would be strictly proportional to the amount of pull P between the jaws. This could never be accomplished by a machine in which the rate of loading was proportional to the sine of the angular displacement. If, however, a small angle is chosen for θ the difference between uniformity and the actual rate of loading will not be very great.

Fig. 2 illustrates the deviation from uniformity in the machine rate of loading for machines of this character where the angular displacement of the pendulum is allowed to have any value from a very small angle to 90 deg. In Fig. 2, the curve represents the value of $\sin\theta$ as the angle of swing passes from zero to 90 deg. By a proper selection of constants, the pull may be plotted as a direct function of $\sin\theta$ so that the curved line of Fig. 2 represents in reality the increase of the pull as the pendulum swings through the various angles. In each case, if the machine rate of load application was uniform the pull would follow the straight line stretching from zero to the plotted point on the sine curve. The sine curve lies, in every case, above the uniform line and therefore throughout the swing the rate of loading is too rapid, namely, is at a figure higher than that of uniformity. If now the deviation of the straight lines from the sine curve be measured for every angle, a plot similar to Fig. 3 may be constructed which shows the percentage variation from uniformity for the various angles of swing. If, for example, the pendulum were allowed to swing through 90 deg. in registering the breaking strength of any fabric, there would be a maximum variation in the machine rate of loading of about 21 per cent above uniformity, and similarly if the swing were limited to 70 deg., there would be a maximum deviation of $11\frac{1}{2}$ per cent above uniformity. It is evident from Fig. 3 that it is not possible in pendulum type machines to make even the machine rate of loading

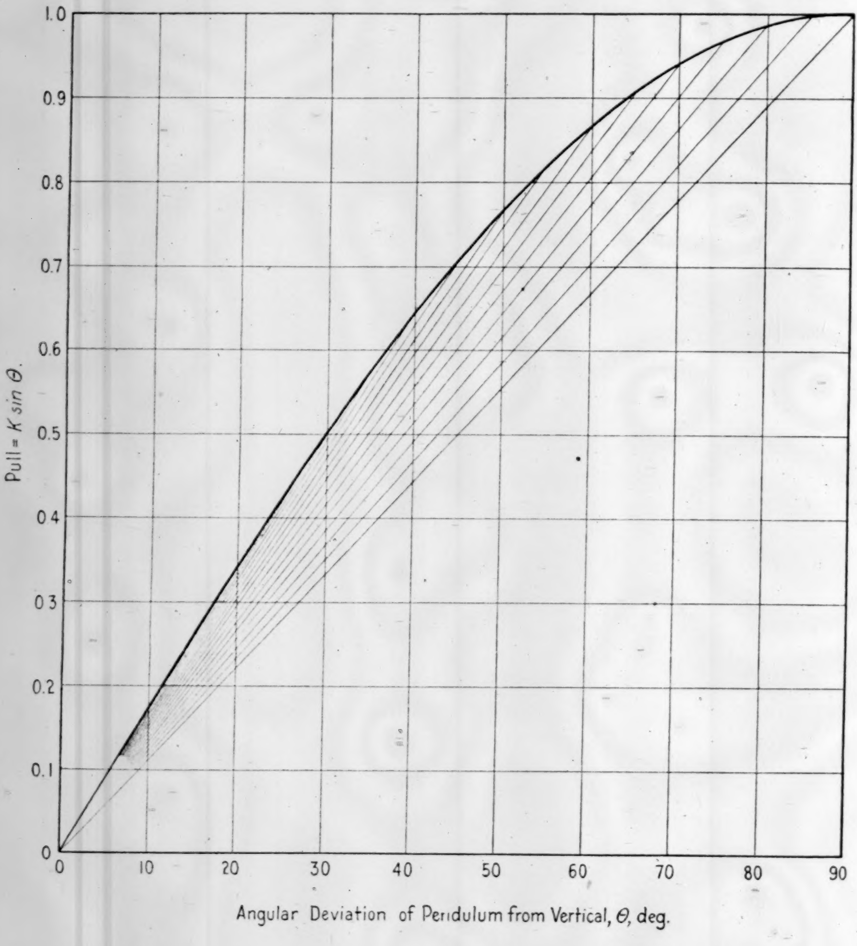

FIG. 2.—Deviation from Uniformity in the Machine Rate of Load for Pendulum Type Testing Machines

uniform and if the maximum variation is to be limited to say 5 per cent, the angle of swing should be limited to about 48 deg. The latter figure has been set at 40 deg. in the tentative standards of this Society,[1] which gives a maximum variation of about 3½ per cent above the desired uniform rate of loading. For this type of machine this is probably as close a figure as can be expected.

The new type of testing machine which forms the subject of this paper is illustrated by Fig. 4 and was perfected by Mr. Henry L. Scott. Two of these machines have been constructed for experimental purposes. It should be here noted that many adjustments, speed changes, and other variations have been embodied in the design which would not appear in the commercial form of this machine. At the top of a rectangular frame are provided heavy ball bearings for the trunnion axes of a tilting beam. The latter moves with very little friction and is nicely balanced so that it can be made to swing through certain angles promptly and with negligible internal strains. The tilting beam provides an inclinable rectilinear track along which roll heavy

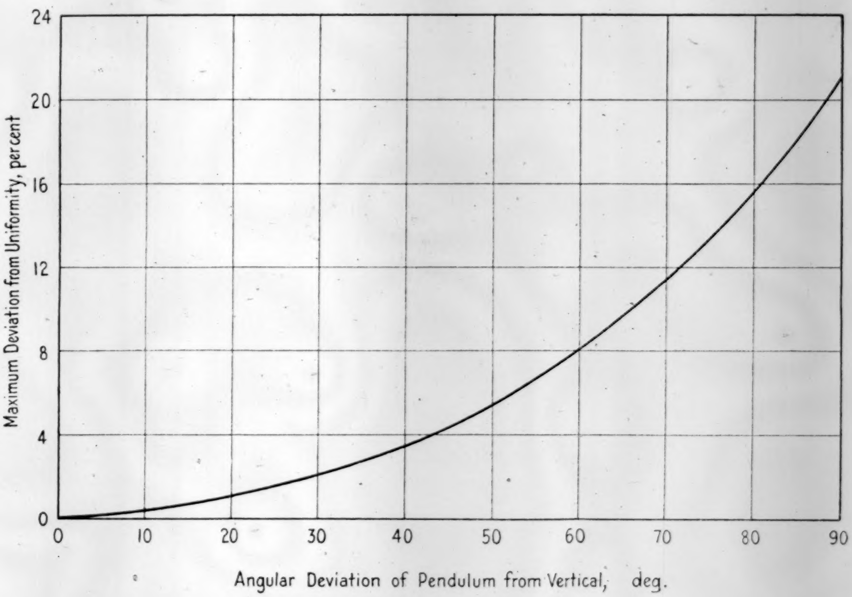

FIG. 3.—Percentage Variation in Uniformity in Machine Rate of Load for Various Angles of Swing

cast-iron disks. These disks constitute the load element of the machine and are yoked to a chain which runs over a guide pulley and downward in a vertical direction to the upper jaw. When the tilting beam is horizontal there is naturally no pull upon the jaw chain. As the inclination of the beam upon its trunnion axis increases, the component of the disk weights increases similarly and produces a greater and greater pull upon the upper jaw. If it were possible to swing the tilting beam to a vertical position the full weight of the disks would be thrown upon the specimen. The weights, at all times, are free to travel down the inclined plane which the tilting beam constitutes, but the pull on the jaw chain depends simply upon the angular inclination of the tilting beam. In the event of a rigid specimen the disk weights would remain near the top of the inclined plane. With a stretchy specimen between the jaws, the weights would automatic-

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[1] Tentative Methods of Testing Textiles (D 76 – 22 T), *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 971 (1922).

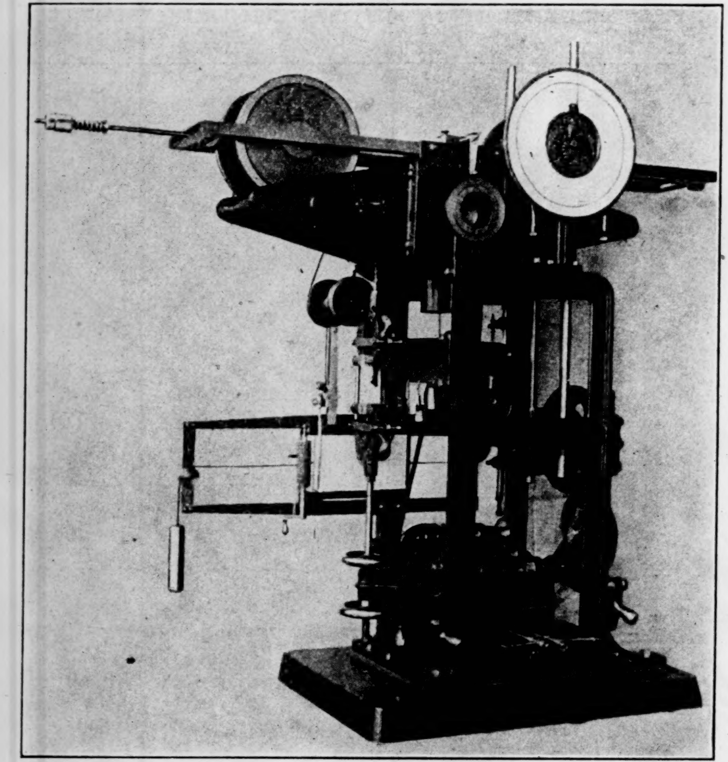

FIG. 4.—Constant Load Rate Testing Machine

ally assume a position farther down the inclined plane. Provided, however, that the angular inclination of the beam was the same in the two cases above mentioned, the pull on the specimen would be identical. The adjustable lower jaw of the machine is wholly stationary and merely holds the lower end of the specimen. By means of the upper jaw the load is applied in the manner described and, by measuring the inclination of the beam, the amount of the load may be determined. In this manner the stretch of the fabric is automatically taken up and the fabric rate of load may be made uniform and of any desired amount, whatever the character of the fabric.

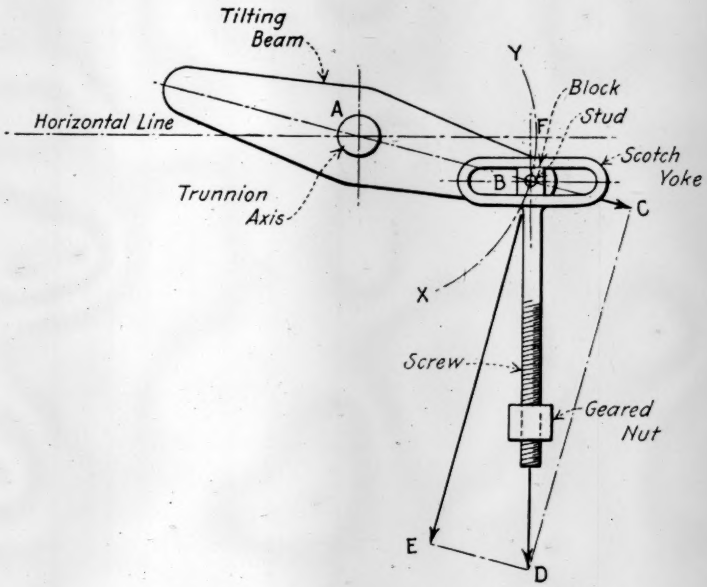

FIG. 5.—Diagram of Mechanism for Securing Uniform Increase in Load

An electric motor suspended from a bracket upon the left side of the machine furnishes the power for driving a train of gears near the base of the machine. A clutch is included in this gear train in such a manner that the action of the machine may be stopped promptly at any desired point and the tilting beam return speedily to a horizontal position. The driving mechanism at the base terminates in a typical change-gear train at the extreme right of the machine. By this means a large variety of speeds may be transferred from the driving motor to a geared nut upon a long vertical screw in the upper right-hand portion of the machine. This screw terminates at its upper end in a slotted crosshead or Scotch yoke. This slotted crosshead is visible in Fig. 4. The block in the slotted crosshead is pinned to a stud set rigidly in the tilting beam of the machine. The position of this stud is therefore fixed with relation to the main trunnions of the upper beam and describes a circle about the center of the trunnions as the beam tips. If now the screw be given a uniform vertical velocity by the gear train, the Scotch links will be drawn down at a uniform rate and the main beam will be tipped upon the trunnions in such a manner as to give a uniform rate of increase to the pull upon the fabric. This is easily seen from the diagram of Fig. 5. The point *A* is assumed to be the center of the trunnions and *B* that of the fixed stud. The line *AB* is therefore invariable in length and the path of the stud is shown by the dot-and-dash circle *XY* as the beam tips about *A*. For simplicity, the rolling weights are supposed to be concentrated at *B* and their total weight represented by the line *BD*. Resolving this weight, the component *BE* will represent the mutual pressure between the weights and the track, while the component *BC* will represent the pull upon the chain leading to the fabric jaw for every position of the tilting beam. The right triangles *FAB* and *CDB* are similar, having their sides mutually perpendicular.

TABLE II.—RATE-OF-LOAD ANALYSIS

Gear Ratio	Without Load		With Load	
	Time for 60 lb. on Dial, sec.	Load Applied per Minute, lb.	Time for 60 lb. on Dial, sec.	Load Applied per Minute, lb.
24 : 120	54	68	55	67.5
24 : 96	42	85.5	43	85.5
36 : 120	33.5	109	36	101
24 : 60	24.5	147	27	132
48 : 96	20	180	22	167
84 : 120	15	240	17	232

$$\text{Therefore } \frac{FB}{AB} = \frac{BC}{BD}$$

Again *AB* is a constant radius and *BD* is a constant weight

$$\text{or } \frac{FB}{\text{a constant}} = \frac{BC}{\text{a constant}}$$

$$\text{and } FB = \text{a constant} \times BC$$

This may be interpreted by saying that if the vertical screw is given a uniform rate of travel *FB*, then the rate of increase in the load *BC* will also be uniform. The motor of the gear train will impart to the vertical screw a uniform downward velocity and then, by means of the Scotch yoke, the tilting beam and trunnions will impart to the fabric a uniformly increasing load. The rapidity with which this load is applied may be set at any reasonable and desirable rate by establishing a suitable rate of advancement for the vertical screw. The change gears provided with the experimental machines are such as to give a rate of load varying from 68 to 240 lb. per minute. Table II[1] illustrates the relations between the various gear ratios, the time necessary to indicate 60 lb. on the dial, and the load applied per minute, both with and without any load upon the machine. Due to slight slippage in the belt and other irregularities there is a small loss of time and load between the empty and loaded conditions.

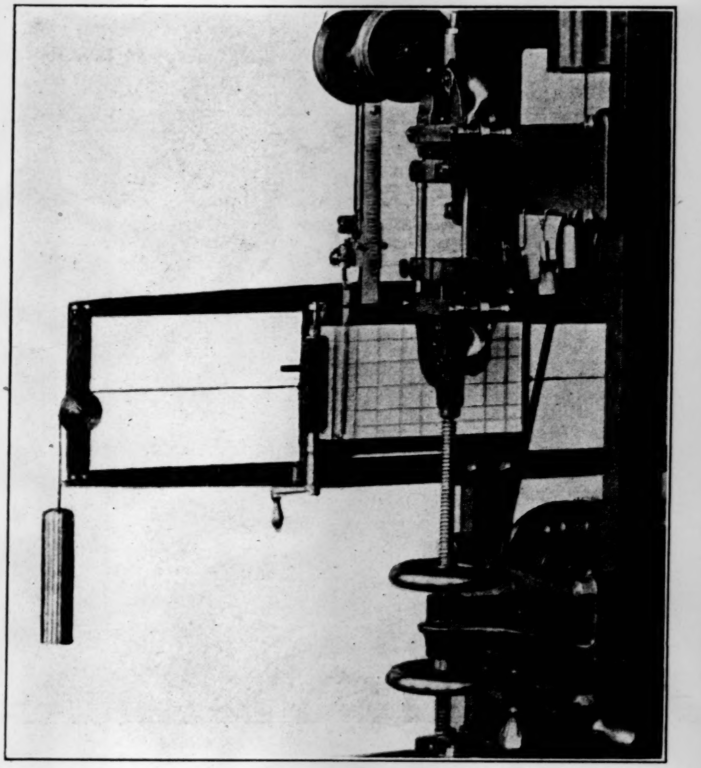

FIG. 6.—An Enlarged View of Fabric Jaw Recording Mechanism

In Fig. 4 is plainly indicated the recording mechanism, consisting of a horizontal sliding platen on which is mounted a pad of cross-section paper. The horizontal movement of this platen is derived from the mechanism applying the load and reads naturally in uniform increments. The vertical motion of the pencil is derived from the stretch of the fabric and indicates for every load the precise amount of extensibility in the specimen. The rupture of the fabric is not only plainly indicated to the eye, but is similarly recorded upon the diagram. In this manner, a set of diagrams give in a true and indisputable manner the precise tensile strength of the specimens. The determination of the amount of the strength does not depend, as in a pendulum type machine, upon ratchets or other mechanism to hold the weight in its highest position. The operation of the machine progresses smoothly and rapidly until the fabric is totally ruptured and the diagram gives then the final and maximum strength. A slight pressure upon the right clutch lever returns the beam automatically to a level position, the correctness of which may be read by a small spirit level attached to the front of the beam.

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[1] Thesis of Hayes and West, Massachusetts Institute Technology, 1922.

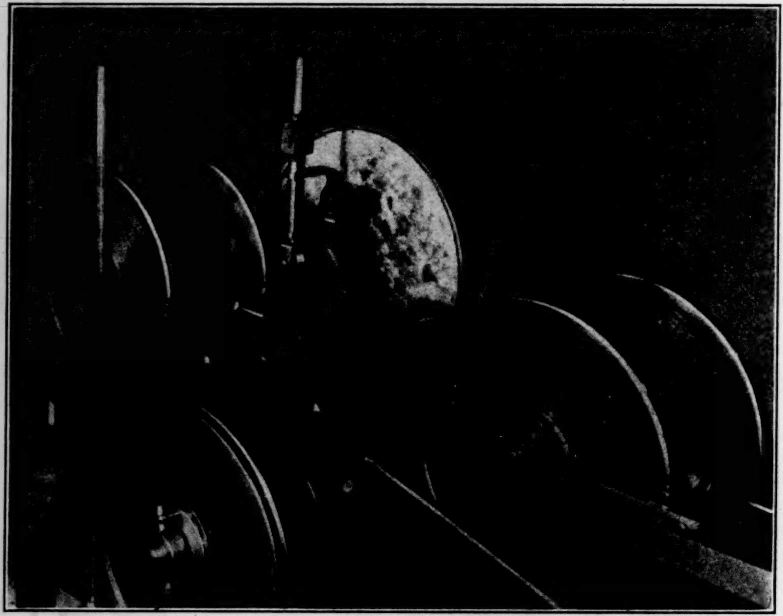

FIG. 7.—Rolling Disks and Yokes at Top of Machine

Fig. 6 illustrates in an enlarged view the fabric jaws and recording mechanism. By adjusting the lower jaw the machine is adapted to any reasonable length of specimen. The jaws are of the flat swinging anvil type and are closed very quickly by depressing upon each a latch fastened to an eccentric sleeve inside of a bearing. The tightness of grip for various thicknesses of fabric may be adjusted by a small worm and wheel and eccentric sleeve in the rear jaw. Fig. 7 illustrates the rolling disks and yokes at the top of the machine. The manner in which the loading component is transferred from the rolling weights to the specimen by means of the chain is also plainly shown.

The author has made a careful series of experiments upon the new constant load rate testing machine to determine what advantages it possesses over the present-day pendulum type.

For this purpose a light single-ply balloon fabric, weighing about 2½ oz. per sq. yd. was chosen. This is a rather brittle fabric and not very extensible. The peculiarities of any testing machine would in general be as evident upon this type of fabric as any. About one hundred specimens were cut accurately of sufficient width to give a gross tensile strength of about 70 lb., which was within the capacity of the new constant-load-rate machine, as manufactured at present. These specimens were thoroughly conditioned for a long period under standard temperature and humidity and were then broken in rapid succession in four types of pendulum machine and three settings of the constant-load-rate machine. Full detail of these tests is given in Table III. None of the 80 tests were thrown out but are all listed in this report. It is evident, as would be expected, that there is a gradual apparent reduction in the tensile strength as the capacity of the machine diminishes. The time of breakage was carefully taken with a stop watch and was averaged for each group of ten samples. As a check upon the readings of the new machine, the 150-lb. pendulum machine was slowed down to the same rate as that of the new machine, namely, 260 lb. per minute. It is to be noted that the behavior of the pendulum type machines was exactly as predicted. The larger

TABLE III.—TENSILE STRENGTH OF BALLOON FABRIC AS DETERMINED BY VARIOUS TESTING MACHINES

Specimen	Pendulum Type, Capacity					Constant-Load-Rate Type, Rate of Load Application		
	800-lb. Gross	400-lb. Gross	300-lb. Gross	150-lb. Gross	150-lb. Capacity Slowed down to a Rate of about 260 lb. per min.	Slow, 60 lb. per min.	Medium, 110 lb. per min.	Fast, 260 lb. per min.
No. 1........	70.0 lb.	69.0 lb.	67.0 lb.	62.5 lb.	61.5 lb.	62.0 lb.	69.5 lb.	69.0 lb.
No. 2........	79.0 "	71.0 "	72.0 "	72.5 "	65.0 "	67.0 "	65.5 "	62.5 "
No. 3........	77.0 "	72.0 "	67.5 "	62.0 "	65.5 "	68.0 "	66.0 "	68.0 "
No. 4........	77.0 "	70.0 "	68.0 "	62.0 "	63.0 "	61.0 "	67.0 "	67.0 "
No. 5........	73.0 "	70.0 "	68.5 "	67.5 "	53.0 "	67.0 "	68.5 "	64.5 "
No. 6........	74.0 "	67.0 "	68.0 "	70.0 "	65.5 "	64.0 "	65.0 "	66.0 "
No. 7.......	79.0 "	71.0 "	65.0 "	65.0 "	58.5 "	63.0 "	66.0 "	69.0 "
No. 8........	77.0 "	71.0 "	68.0 "	69.0 "	62.0 "	64.0 "	67.0 "	66.0 "
No. 9........	74.0 "	67.0 "	68.0 "	63.0 "	58.0 "	64.0 "	70.0 "	66.0 "
No. 10........	74.0 "	67.0 "	66.0 "	64.0 "	65.0 "	64.0 "	67.0 "	67.0 "
Average...	75.4 lb.	69.5 lb.	67.8 lb.	65.8 lb.	61.7 lb.	64.4 lb.	67.2 lb.	66.5 lb.
Time......	2.55 sec.	3.10 sec.	3.85 sec.	5.25 sec.	15.9 sec.	68.4 sec.	36.2 sec.	14.8 sec.

the capacity of the machine the higher the apparent tensile strength and the less time required to break the specimen. The variations in the results obtained in the new constant-load-rate machine were in general so small as to be negligible, but they indicate, nevertheless, that in general the more rapid the rate of load application the higher the strength. The results show just what variation may be expected in commercial practice if pendulum type testing machines are used and their total capacity is not very definitely limited with regard to the strength of the specimen. The Society has set certain wide limitations in regard to the gross capacity of testing machines.[1] It is still possible, however, to test 50-lb. specimens upon a machine of 400-lb. capacity and also 20-lb. specimens upon a machine of 150-lb. capacity. It is to be noted from our experimental results upon the new constant-load-rate machine that, even though the rate of load application be increased from 60 lb. per minute to 260 lb. per minute, the apparent strength of the fabric fluctuated only about 3 per cent, while with the pendulum type of machine there was a variation in the same fabric of over 14 per cent. For this reason the author firmly believes that the new type of machine is destined to become the permanent form of apparatus for testing textiles. The points of excellence in its design and operation are as follows:

1. The rate of application of the load is uniform whether the specimen is long or short, rigid or stretchy, weak or strong.

2. It makes but very little difference what rate of load application is actually chosen. A standard rate may be adopted, but other rates will give results within two or three per cent of one another if used.

3. The charts present an indisputable record of the strength. They cannot be tampered with and a novice can read them.

4. Variations in tensile strength in a given fabric are much less in the new form of machine than in the pendulum type. It is therefore to be expected that the results will be more accurate.

Incidentally, it may be remarked that the present form of the machine is wholly of an experimental type, having numerous speed changes and other complications which would not be necessary when the machine is adopted for practical use. A much simpler form of this machine is at present in process of design, in which many complications have been eliminated and the mechanism reduced to the simplest possible form. When this machine is produced, it is hoped to make a much wider range of tests to determine its excellence and capacity.

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[1] Tentative Methods of Testing Textiles (D 76 - 22 T), *Proceedings*, Am. Soc. Testing Mats., Vol. 22, Part I, p. 971 (1922).

A NEW COMBINED VISCOMETER AND PLASTOMETER

BY E. C. BINGHAM[1] AND H. A. MURRAY, JR.[2]

To obtain the plasticity of a substance, it is necessary to measure the flow at two or more shearing stresses. Even in the measurement of viscosity, the use of more than one shearing stress is highly desirable.[3]

The variation in shearing stress may be obtained in a variety of ways. The one generally employed is by varying the applied pressure. This is cumbersome. A much more elegant procedure is to keep the applied pressure P constant and to vary the shearing stress F by changing the dimensions of the capillary, since the shearing stress per square centimeter at the wall of the capillary is dependent upon the radius R and the length l as follows: $F = PR/2l$. There are several methods for securing this result. One may employ several distinct capillaries in the plastometer having a variety of dimensions to be used successively. Instead of using the capillaries one after another in parallel, they may be united together in series as needed by means of glass and rubber tubing. This method has been studied by Mr. Boyer in this laboratory (Gayley Chemical and Metallurgical Laboratory). There are many other variations in the procedure which have been considered, but we have temporarily abandoned all of these leads which seemed to be more or less promising in order to investigate a new principle in flow measurement which seemed to offer the possibility of a vast saving of labor in these measurements.

When a substance enters an empty capillary under a given head, the shearing stress continually changes, the flow becoming slower and slower as the shearing stress decreases. Since this shearing stress may be easily calculated, it is only necessary to measure the time required for the material to fill the capillary to successive points in order to obtain the data necessary for viscosity or plasticity calculations.

Two difficulties presented themselves in the practical working out of the method, for while on the one hand the flow through the capillary must be sufficiently slow so that the time of efflux can be measured with the required precision, on the other hand the rate of flow must be sufficiently rapid so that the material will flow in telescoping layers.

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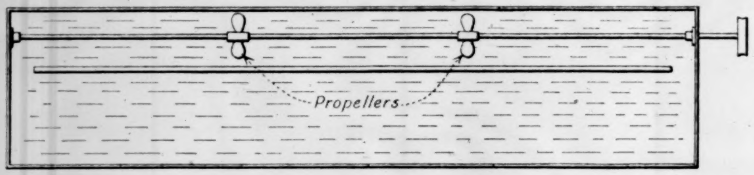
[1] Professor of Chemistry, Lafayette College, Easton, Pa.

[2] Lafayette College, Easton, Pa.

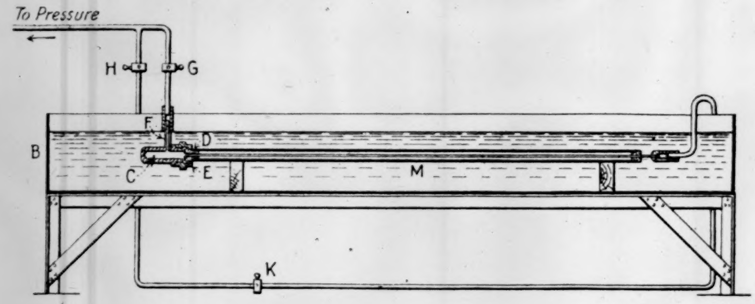
[3] See *Proceedings*, Am. Soc. Testing Mats., Vol. XVIII, Part II, p. 373 (1918).

A good stopwatch is liable to an error of 0.3 seconds, but a chronograph may be read to 0.01 second. To get an accuracy of 0.1 per cent five minutes are required with a stopwatch, but only ten seconds with the chronograph provided that the observer's personal error can be kept within the limit.

The use of such short times of efflux makes possible the employment of rather rapid rates of efflux without the use of excessively long capillaries. A capillary 100 cm. long with marks at 10-cm. intervals seems satisfactory. A time signal is given each time that the material being studied reaches one of the marks on the capillary. Of course it is not known at just what instant the material starts to enter the capillary, so no determination can be made over the first portion of the tube, but each succeeding 10-cm. interval of the tube may be used to calculate the fluidity or plasticity.



Top View of Bath Showing Two Compartments.



Longitudinal Section.

FIG. 1.—A New Combined Viscometer and Plastometer

DESCRIPTION OF APPARATUS

A cylindrical container *E*, Fig. 1, 2 cm. in inside diameter and 10 cm. deep, is closed by means of a cover *D* into which the end of the capillary is cemented. The cover is made tight by means of a fiber gasket and a screw cap. Pressure is admitted to the container through the side-tube *F*, which is placed as near the top of the container as practicable, so that the material will not enter the side-tube either when the container is in the vertical position for filling or in the horizontal position during the measurement. By means of the three-way stopcock the far end of the capillary may be connected to either the pressure or the air. A brass frame *M* is used to protect the capillary during the manipulation.

For absolute measurements it is necessary to calibrate the tube throughout its entire length by means of a mercury thread and to estimate the major and minor axes. For ordinary purposes it is much safer to calibrate the different lengths of the capillary with a liquid of known viscosity, for which purpose we have used castor oil.

The pressure was obtained by pumping air into a tank which acted as a stabilizing reservoir during the measurements. The pressure was read on a mercury manometer.

The chronograph was obtained from the Bausch and Lomb Optical Co. It was connected to an accurate clock which gave impulses every second. The key for the operator was placed on the table beside the bath.

The bath was constructed of galvanized iron, 15 by 30 by 150 cm., and it was painted white on the inside to get the maximum visibility. It was divided into two compartments by means of a metal partition, which was cut away at the ends of the bath to allow circulation of water. Through the smaller compartment, 12 cm. wide, there ran a shaft carrying propellers which were actuated by a $\frac{1}{12}$-h.p. motor. The temperature was kept constant at 20° C. with a deviation of only 0.01° C.

While the material is coming to the bath temperature, both ends of the capillary are connected with the pressure in such a manner that the material does not enter the capillary an appreciable distance. When ready to make a series of measurements, the container is turned to pressure, the far end of the capillary is turned to air and the chronograph key pressed sharply as the material reaches each narrow scratch. As soon as the series is finished the container is turned from pressure to air and the pressure and temperature immediately read.

CALCULATION OF RESULTS

The volume of flow is evidently $\pi R^2 (l_2 - l_1)$, where l_1 and l_2 are successive lengths measured from the end of the capillary within the container. The shearing stress $F(=PR/2l)$ must now be obtained by integration as the average shearing stress between the two limits l_1 and l_2. Thus

$$F=\frac{1}{l_2-l_1}\int_{l_1}^{l_2}\frac{PR}{2l}\,dl=\frac{PR}{2(l_2-l_1)}\quad\log_e\frac{l_2}{l_1}\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

$$F=\frac{PR}{20}\log_e\frac{l_2}{l_1}\left(\text{when } l_2-l_1=10\text{ cm.}\right)\ldots\ldots\ldots\ldots\ldots(2)$$

$$F=0.1151\ PR\log\frac{l_2}{l_1}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3)$$

when changed to Briggsian logarithms.

The yield value may be obtained graphically by plotting the rate of flow against the shearing stress, or it may be obtained by formula.[1] Having found the yield value the mobility is calculated by formula.[2]

The corrections with this instrument are similar to those generally used[3] except in the following particulars. The kinetic energy correction appears to be negligible.

There is a correction for capillarity. From the fundamental equation

$$2\ \pi R\gamma=\pi R^2hg\rho$$

we obtain as the force which is assisting the flow

$$h\rho=\frac{2\gamma}{Rg}$$

The hydrostatic head correction is obtained by calculation from the fraction of the container which was empty at the beginning of the experiment. On turning the cylindrical container on its side the surface of the material at any cross-section will form a chord which subtends at the center of the circle an angle which we will call θ. The area of the segment is

$$\left(\frac{\pi\theta}{180}-\frac{\sin 2\theta}{2}\right)R^2$$

and the volume of the empty part of the cylinder must be

$$\left(\frac{\pi\theta}{180}-\frac{\sin 2\theta}{2}\right)hR^2=\pi ahR^2$$

where h is the height of the cylinder, hence

$$a=\frac{\theta}{180}-\frac{\sin 2\theta}{2\pi}$$

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[1] "Fluidity and Plasticity," p. 219.

[2] *Ibid.*, p. 225.

[3] *Ibid.*, p. 298.

The height of the surface of the material above the center of the cover is $R \cos \theta$. The values of $\cos \theta$ for various values of a have been calculated and are reproduced in Table I. Let h_2 be the distance downward from the center of the cover of the container when in the horizontal position to the axis of the capillary. The true pressure producing the flow is then

$$P = M \pm N + \rho(h_2 + R \cos \theta) + \frac{2\gamma}{Rg} \ldots\ldots\ldots\ldots (4)$$[1]

where ρ is the density and γ the surface tension of the material being measured.

TABLE I.—VALUE OF COS θ TO BE USED IN CALCULATING HYDROSTATIC HEAD CORRECTION

FRACTION EMPTY, a	COS θ	FRACTION EMPTY, a	COS θ	FRACTION EMPTY, a	COS θ
0.05	0.793	0.21	0.476	0.37	0.205
0.06	0.775	0.22	0.457	0.38	0.190
0.07	0.755	0.23	0.440	0.39	0.174
0.08	0.733	0.24	0.423	0.40	0.156
0.09	0.710	0.25	0.405	0.41	0.142
0.10	0.688	0.26	0.386	0.42	0.125
0.11	0.667	0.27	0.370	0.43	0.110
0.12	0.647	0.28	0.354	0.44	0.095
0.13	0.625	0.29	0.336	0.45	0.079
0.14	0.606	0.30	0.321	0.46	0.064
0.15	0.587	0.31	0.305	0.47	0.044
0.16	0.566	0.32	0.289	0.48	0.032
0.17	0.550	0.33	0.272	0.49	0.015
0.18	0.530	0.34	0.255	0.50	0.000
0.19	0.512	0.35	0.237		
0.20	0.494	0.36	0.223		

RESULTS

For the calibration of the instrument, we have used castor oil supplied us through the courtesy of the National Lead Co., the fluidity of which was found to be 0.0934 on viscometer No. 6, calibrated by both Hyden and Stone.[2]

Many hundreds of determinations with different capillaries and at different applied pressures have been made. Only an example or two can be given here. Using capillary No. 17 the following values for castor oil at 20° C. were obtained with the different lengths of the capillary, using an applied pressure of 1030 g. per sq. cm.

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[1] "Fluidity and Plasticity," p. 298.

[2] *Ibid.*, p. 295. *et seq.*

There is no steady drift in the fluidity values as calculated from the measured radius of the capillary and given in the fourth column of the table, yet the shearing stress is varied many fold as is also the length of the tube employed. By repeating these determinations many times, we have found that the average deviation from the mean is about 0.8 per cent, and that certain lengths give times of efflux which are consistently too large or too small. This is evidence, which has been reinforced in a variety of ways, that all of the capillaries which we have been able to study have possessed irregularities of bore which we have been unable to take account of in calibration.

Another evidence of the difficulty of exact measurement of the radius of different capillaries comes when we compare the absolute fluidities obtained with tubes of apparently similar bores. Thus capillary No. 15 had a radius of 0.02583 cm. and that of No. 16 was 0.02544 cm. The fluidities of castor oil at 20° C. obtained with these were 0.0915 and 0.0886, respectively, which is far outside of the 0.8 per cent error of efflux measurement. The same sort of evidence was obtained on causing the flow to take place in opposite directions in the same tube, by reversing the tube end for end. Capillary No. 18 gave a fluidity of 0.0935, but when the tube was reversed the apparent average fluidity fell to 0.0892. In the face of this difficulty in obtaining capillary tubes of uniform bore, we are unable to state positively as yet whether the fluidity is independent of the radius of the tube as the theory predicts or not. We can only say that there is no evidence of drift in the measurements we have made with tubes of different radii, nor is there any evidence of drift when the applied pressure is varied.

TABLE II.—FLUIDITIES OF CASTOR OIL

Time, sec.	V/t, ml. per sec. $\times 10^6$	F, dynes per sq. cm.	Fluidity Observed	Percentage Deviation	Factor	Fluidity Corrected	Percentage Deviation
14.81	1797	1017	0.0918	−1.8	1.018	0.0923	**−1.0**
24.58	1083	595	0.0943	0.9	0.989	0.0933	**0.1**
34.66	768	422	0.0943	0.9	0.993	0.0936	**0.5**
44.46	569	327	0.0948	1.4	0.989	0.0937	**0.5**
55.21	482	268	0.0934	−0.1	0.990	0.0925	**−0.8**
65.38	407	226	0.0933	−0.2	1.002	0.0935	**0.3**
76.22	349	196	0.0924	−1.1	1.013	0.0936	**0.4**

Fortunately the difficulty which we have encountered in obtaining capillaries of uniform bore does not make it impossible to use the method which we are here studying. In fact, this same difficulty enters into all absolute measurements of fluidity by the capillary tube method, so that the best determinations of fluidity for an easily purified substance like water differ by about one per cent from each other in some cases; so it is customary to take some substance such as water at 20° C. as a reference standard and calibrate our viscometers against it. We have therefore calculated a factor for each successive length of each capillary, examples of which are given in the sixth column of Table II. Using this factor we obtain the fluidities given in the seventh column of the table. In a considerable number of observations, the average deviation of the individual observations from the true fluidity is now only 0.4 per cent, which is about the magnitude of the average observer's error in obtaining the time.

To obtain a check upon the method, it is desirable to compare the fluidities of other substances on the two types of viscometers. We have used a pale mineral oil and have obtained values agreeing within one per cent. But since this mineral oil has nearly the same fluidity as castor oil at 20° C., we continued this comparison using castor oil over a range of temperatures. The values are given in Table III. In using the mineral oil the possibility of change in viscosity on standing was avoided by practically simultaneous measurement in both instruments.

TABLE III.—COMPARISON OF FLUIDITIES OF CASTOR OIL AT A VARIETY OF TEMPERATURES AS DETERMINED ON THE BINGHAM VISCOMETER AND THE BINGHAM AND MURRAY COMBINED VISCOMETER AND PLASTOMETER.

TEMPERATURE, DEG. CENT.	FLUIDITY, VISCOMETER NO. 6	FLUIDITY, COMBINED VISCOMETER AND PLASTOMETER NO. 16
10	0.0374	0.0375
20	0.0934	0.0934
25	0.1370	0.137
30	0.2000	0.198
40	0.3890	0.391

As a type of plastic material, we have studied a paint containing 60 per cent of lithopone and 40 per cent of acid-refined linseed oil. Data obtained with capillaries No. 17 and No. 18 are shown in Fig. 2. Each capillary gives points on a linear curve and the two curves intersect satisfactorily on the axis at a shearing stress of 18.1 dynes per sq. cm. At no shearing stress which we have been able to reach as yet has the curve shown a tendency to bend off toward the origin, indicating seepage or slippage. In this respect the results are more satisfactory than those obtained with the Bingham and Green plastometer.

These curves prove that a change in the radius or length of the capillary is without effect on the plasticity constants. They prove that this paint has a definite and precise friction and mobility. The mobility as obtained with capillary No. 17 was 0.552 with an average deviation of the individual measurements of 1.2 per cent. Capillary No. 18 gave 0.553 with an average deviation of 0.5 per cent. The mobility of this paint found by Bruce and Wolbach was 0.617, which has been more recently confirmed by Jacques to be 0.598.

The yield value as found by Bruce and Wolbach was 110 and by Jacques 106, using the Bingham and Green plastometer, whereas we obtain 18.1 with both capillaries within the experimental error. Experiments are being continued to discover the cause of this discrepancy.

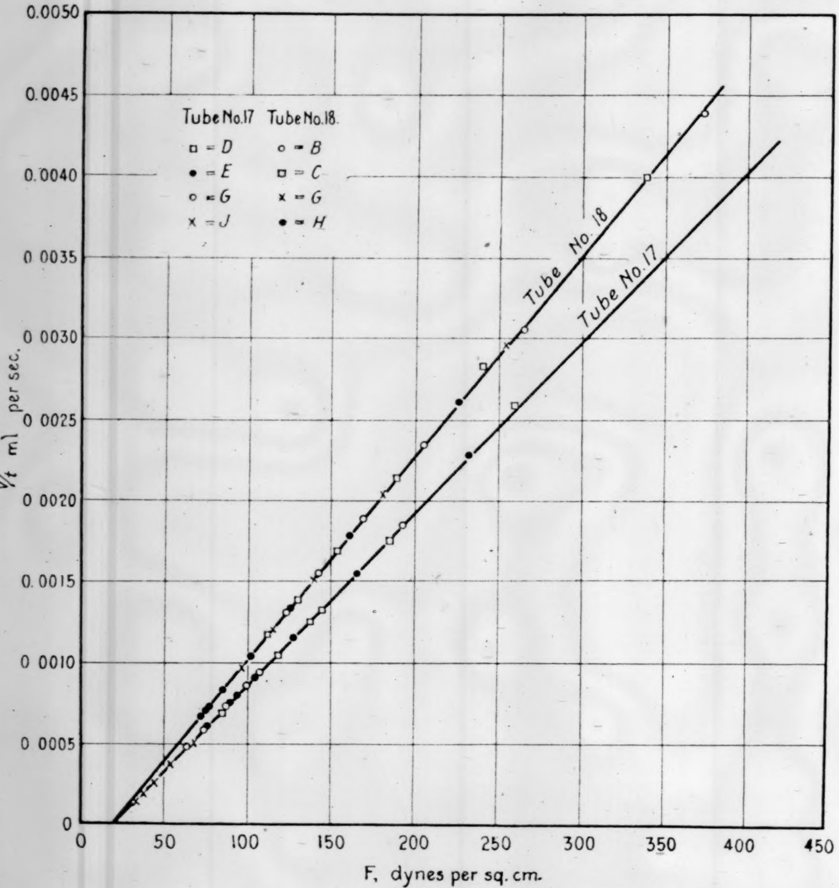

FIG. 2.—Tests of Lithopone Paint

Letters refer to successive series of measurements.

Acknowledgment.—The authors desire to thank Mr. William M. Smith for assistance in connection with mathematical problems which have arisen during this investigation.

DISCUSSION

MR. HENRY GREEN.[1]—There are three outstanding reasons why Bingham and Murray's paper demands careful attention: **Mr. Green**

1. It opens up a comparatively new field for research in the subjects of viscosity and plasticity.
2. It is theoretically unsound.
3. It arrives at certain conclusions of an erroneous nature that are so dangerous that they destroy the good qualities the paper contains.

The first reason is self-evident and requires no further comment.

As to the second reason, examine their Eq. 1 and note the use to which it has been put. In this equation the shearing stress F is determined by integrating an infinite number of Fs between the limits l_2 and l_1, equally spaced by a distance dl, and the result divided by the number of Fs integrated, that is, by $(l_2-l_1)/dl$.

A method of this kind gives a mathematical average for the shearing stress on the basis of dl being maintained constant. Perhaps it was on account of an ardent desire to be as exact as possible that these investigators resorted to the trouble of determining their shearing stress in this manner. If so, why were they not equally exacting in calculating the correct v/t caused by their average stress? Even the wording of their own statement shows that they maintained some doubt as to the validity of their method, for they state that, "The volume of flow is *evidently* πR^2 (l_2-l_1)." Why not decide this important point with certainty? A few minutes at their disposal and the application of elementary mathematics would have sufficed to prove that πR^2 (l_2-l_1) was *evidently not* the correct v/t to use.

Another fact in regard to this aspect of their paper should be pointed out: that though absolute formulas for a v/t that will correspond to some pre-decided F can be determined for liquids, these formulas become only approximately true when used with plastics, such as paint. The reason for this is, that in the case of plastics $(v/t)\times l$ is not constant, v/t being the rate of flow at any distance of penetration, l. Hence, I feel justified in considering Bingham and Murray's work to be theoretically unsound, and especially so in regard to plasticity.

---

[1] Research Division, New Jersey Zinc Co., Palmerton, Pa.

Mr. Green

In spite of this defect, however, their method is useful, for it happens when $l_2 - l_1$ is made sufficiently small, that then any kind of average F will closely correspond to any kind of average v/t. It is entirely on account of this fortuitous circumstance that Bingham and Murray were able to determine, with only small error, the viscosity of castor oil.

The following quotation from their paper should be noted:

> "At no shearing stress which we have been able to reach as yet has the curve shown a tendency to bend off toward the origin, indicating seepage or slippage."

From this the reader must infer that the investigators have succeeded in accomplishing two truly remarkable things:

1. Eliminated slippage by mechanical means.
2. Rectified the plastic flow curve.

It is apparent, from the quotation just given, that Bingham and Murray first have not correctly visualized the phenomenon of slippage, and second, that they do not understand the reason for the curvilinear nature of the true plastic flow curve, having attributed it to slippage at the wall of the tube. So long as they retain this mental attitude they will find it impossible to make progress with their method. Slippage is a troublesome condition that cannot be eliminated by any mechanical device whatever. As slippage invariably precedes plastic flow it is evident that any method that succeeds in stopping slippage would necessarily stop all flow.

E. Buckingham,[1] has proved that the relationship between F and v/t must be curvilinear, except in the region of total slippage. Bingham and Murray have concluded, however, that with their method this relation becomes linear. Since their laboratory ability is above reproach, this apparent contradiction can be explained only as one of faulty interpretation of experimental data.

In attempting to solve a problem of this kind it is a great temptation to shun the obvious and seek for a mystery. No mystery, however, exists here. There are only three possible causes for the linearity of Bingham and Murray's curves:

1. Curve confined to the region of total telescopic flow.
2. Curve confined to the region of total slippage.
3. Curve confined to only a minute section of the full curve.

On account of the small shearing stresses employed, the first cause need not be considered. In order to decide which of the two remaining causes will give the correct solution, it is necessary to replot their curve for tube 17, Fig. 2, alongside the full plastic-flow curve of a normal paint. From their Table II, the radius of tube 17 can be calculated, and is found to be approximately 0.09204 cm. The distance $l_2 - l_1$ is taken as 10 cm., as their Eq. 2 implies this length.

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[1] E. Buckingham, "On Plastic Flow Through Capillary Tubes," *Proceedings*, Am. Soc. Testing Mats., Vol. 21, p. 1154 (1921).

Next, a normal paint is run on a Bingham and Green plastometer; the constants, yield value, mobility and $\epsilon\psi$ determined by the Buckingham formula, then resubstituted in the Buckingham formula together with the radius and length of Bingham and Murray's tube 17. Volume-of-flows per second are now calculated for convenient pressures and a plastic-flow curve plotted from them. We then have a curve of a normal paint run through Bingham and Murray's tube 17 but plotted over its full extent such as would be obtained with a Bingham and Green plastometer. (See Fig. 1.)

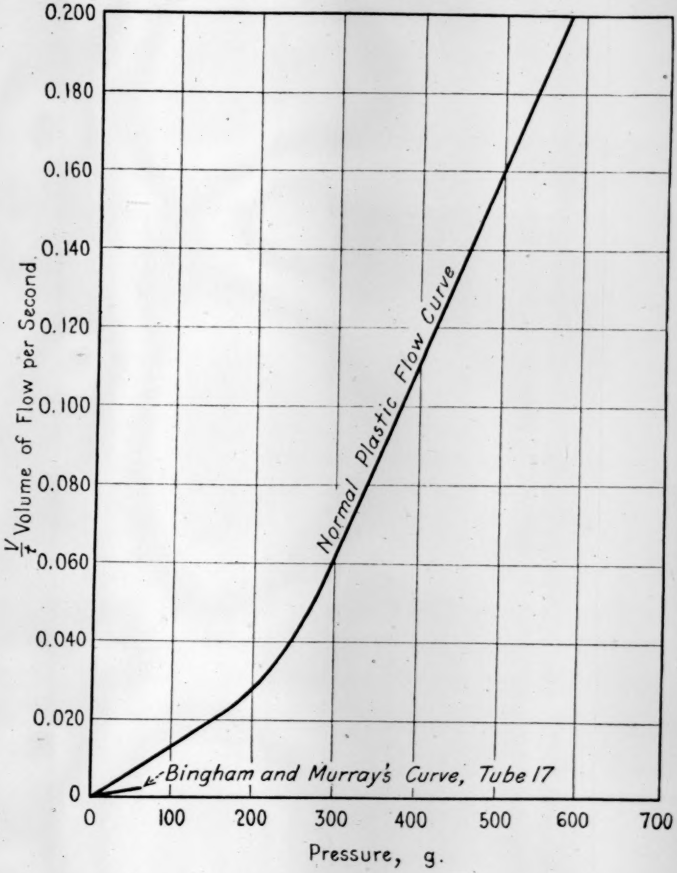

FIG. 1

Mr. Green Alongside this curve, for comparison, is replotted Bingham and Murray's curve for tube 17, over the extent used by these investigators with their method. The result speaks for itself, it being evident: that nothing new in regard to the law of plastic flow has been discovered; that the curve has not been rectified; that slippage has not been eliminated; and, if their paint used was anywhere near normal, that their curve is confined entirely to the region of total slippage. In addition it is obvious that a mere fragment of the plastic-flow curve, as obtained by Bingham and Murray, is useless for the determination of yield value and mobilty.

Mr. Bingham has stated that my arguments do not hold, for the reason that he has also used pressures as high as those employed in the Bingham and Green type of instrument, and still obtains linear results. He overlooks the fact that it is not a question of the magnitude of pressures used but of the relative length of curve plotted. Any curve, no matter how great its curvature, will appear linear if only a minute section of it is examined at a time.

Mr. Herschel MR. W. H. HERSCHEL.[1]—I think it has not been sufficiently emphasized either by Mr. Bingham or Mr. Green, that surface tension is involved in this method of determining plasticity. Now anybody who has measured surface tension knows that it is not an easy thing to measure. The idea seems to be that when you have a dynamic method like this a fresh film is constantly being formed and that consequently the surface tension is changing. At any rate, Washburn[2] and others[3] who have experimented with this dynamic method and who regard it either as a method of measuring viscosity if the surface tension is known, or as a method of measuring surface tension if the viscosity is known, all have had great difficulty in getting results to conform with their equation. Now the originality of Mr. Bingham's work appears to be in applying this dynamic method to plastic materials, but I do not think he has told us very much about how we are to determine the dynamic surface tension of a plastic material, and that is the problem which is involved in making a success of this method.

Mr. Job MR. ROBERT JOB.[4]—I should like to ask Mr. Bingham whether the apparatus can be used in making determinations upon rather liquid asphalt; also I should like to ask what the simplest method is for cleaning the tube.

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[1] Associate Physicist, U. S. Bureau of Standards, Washington, D. C.

[2] E. W. Washburn, Physical Rev. 17, p. 273; 1921.

[3] E. K. Rideal, Phil. Mag., 44, p. 1152; 1922.

[4] Vice-President, Milton Hersey Co., Ltd., Montreal, Canada.

Mr. Bingham

MR. E. C. BINGHAM.—Mr. Green will probably recall that in 1920 we used the conception that the shearing stress at the wall of the capillary varies with the radius of the tube and that on the train from Palmerton to Easton we discussed the possibility of the flow at the wall taking place before it did at the center of the tube.

Mr. Buckingham has indicated the mathematical result of assuming that the flow takes place in a certain manner. He does not affirm that plastic flow does take place in this manner. He does prove that for the assumed type of flow the flow-stress curve is bent, particularly at the lower shearing stresses.

By making a different assumption, I was earlier led to a flow-stress curve which is linear, although I early pointed out that it would be complicated by either seepage or slippage. We shall undoubtedly accumulate enough data presently so that the nature of plastic flow will be more fully understood.

We are trying to make the simplest possible assumptions and none at all when we can avoid it. The linear flow - shearing stress curves described in this paper seemed satisfactory from this point of view. The only difficulty, which we trust is only temporary, arose in comparing different types of instrument. We do not see now that we can utilize Mr. Green's suggestion in regard to Buckingham's assumed method of flow.

The suggestion that we are really on a different part of the flow - shearing stress curve with the two instruments has occurred to us and seems to have been eliminated. It is possible that the average rate of flow is slightly different from the flow which we have taken, as Mr. R. E. Wilson has privately pointed out. We shall try to devise a method for testing this out.

The paper shows that we attempted to allow for surface tension, and we believe that any correction is entirely inadequate to explain the difficulty. It appears to be possible to use tubes so large that capillarity will not be appreciable.

As to the use of an instrument of this type to measure hard naterials, it seems to me that it may be readily possible. Experiments that have been made prove that soft metals as well as non-metallic substances such as gelatine may be made to flow in telescoping layers under appropriate pressures.

In cleaning the capillary, it is unscrewed from the container, mounted in a vertical position and connected to suction. Benzene will quickly remove most of the paint, but a thin layer of pigment may stubbornly resist this solvent, hence it should be followed by chromic acid and then water, alcohol and ether.

Mr. Bingham (*Author's closure by letter*).—To say that "the relationship between F and v/t *must* be curvilinear" as Mr. Green does may be a little too dogmatic, for this statement is founded on certain assumptions. We must be careful to guard against forcing our facts to fit this or any other theory until the assumptions upon which it is based are wholly justified. We are grateful for the criticisms which our paper has received and it is proposed to discuss the subject in detail in a later paper.

We have already considered the possibility of plotting the average rate of efflux against the average shearing stress. It was first suggested in correspondence with Mr. R. E. Wilson.

If it is true that "any curve, no matter how great its curvature, will appear linear if only a minute section of it is examined at a time", then Mr. Green's suggestion that a much greater range of shearing stresses be employed will be fruitful. But in spite of its plausibility, we do not find ourselves able to accept it. The curvature is defined by the expression d^2v/dF^2 where v is the rate of efflux and F is the shearing stress. Apparently the curvature does not depend in any way upon the length of the curve, although it is obvious that the measurements must be sufficiently numerous and exact to indicate the true course of the curve. Our measurements from 0.0026 to 0.0001 g. per second of efflux in tube 17, for example, seemed ample to allow us to extrapolate with some assurance to the shearing stress corresponding to an efflux of 0.0000, particularly since the curve was quite linear over the range studied. This is the shearing stress which we have somewhat arbitrarily defined as the "yield value."

Should we find later that the flow is not actually zero when the shearing stress is equal to the yield value, or if the v *versus* F curve should change its character at much higher shearing stresses, it would not affect the definiteness of the property that we have studied. As in viscous flow several regimes are known, it would not be surprising if there should be found more than one regime of flow in plastic solids. There should be no difficulty in establishing this point.

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